



O. 1344

Ba June 1876  
Bb

PL



22102460154



WELLCOME HISTORICAL MEDICAL LIBRARY STORE,  
WELLCOME CHEMICAL WORKS,  
805, BAY 1,  
DARTFORD, KENT.]





THE  
OBSTETRICAL JOURNAL

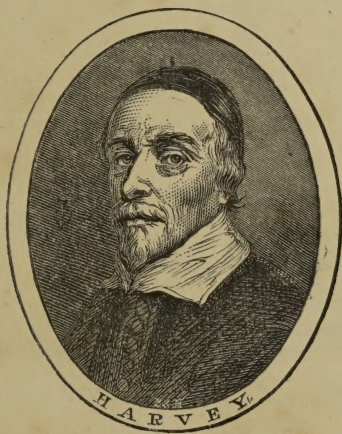
OF

GREAT BRITAIN AND IRELAND

INCLUDING

Midwifery and the Diseases of Women and Children

EDITED BY J. H. AVELING, M.D.



VOL. III

APRIL, 1875, TO MARCH, 1876

LONDON

J. & A. CHURCHILL, NEW BURLINGTON STREET

WELLCOME INSTITUTE LIBRARY	
Coll.	welMOmec
Call	ser
No.	WPQ1
	7016



# LIST OF ORIGINAL COMMUNICATIONS.

	PAGE
Contribution to the Study of Non-cancerous Tumours of the Breast. By Dr. Charles Monod. Translated by C. J. Cullingworth . . . . .	1
On the Postural Treatment of Prolapsus of the Funis Umbilicalis. By John Brunton, M.A., M.D. . . . .	16
Notes of One Hundred Cases of Abnormal Labour, under the care of W. Y. Martin, L.R.C.P., and F. H. Y. Grosholz, L.K.Q.C.P. . . . .	32
The Puerperal and Pyemic Processes. By Dr. Hjalmar Heiberg . . . . .	73
Reposition of the Prolapsed Funis Umbilicalis. By J. Braxton Hicks, M.D., F.R.S. . . . .	84
Prolapsus of the Funis Umbilicalis. By Patrick Jamieson, M.A., L.R.C.S.E. . . . .	86
Influence of Posture on Women. By J. H. Aveling, M.D. . . . .	145
A Case of Rupture of the Uterus, with Protrusion of Intestine—Complete Recovery. By O. Lowsley, M.R.C.S., L.S.A. . . . .	166
Retroversion of the Gravid Uterus—Retention of Urine—Recovery. By A. W. Edis, M.D. . . . .	170
A Letter in Facsimile and other hitherto unpublished Memorials of Harvey. Collected by J. H. Aveling, M.D. . . . .	217, 310
Some Cases of Puerperal Illness in Private Practice. By C. J. Cullingworth . . . . .	221, 299
Notes on Puerperal Fever. By F. H. Y. Grosholz, L.K.Q.C.P.I., L.M. . . . .	237
An Address on Obstetric Medicine, and its Position in Medical Education. By Robert Barnes, M.D., F.R.C.P. . . . .	289
Cursory Notes on Ovarian Pathology. By J. Matthews Duncan, M.D. . . . .	361
On the Relative Frequency of the Different Cranial Positions. By J. G. Swayne, M.D. . . . .	372
Notes on Puerperal Fever, with Remarks on Midwifery Statistics. By Henry M. Madge, M.D. . . . .	377
Case of Congenital Deficiency of the Peritoneum, Resulting in Intestinal Obstruction and Simulating an Abdominal Tumour. By Lawson Tait, F.R.C.S. . . . .	425
A Case of Natural Turning. By P. B. Giles, Jun., L.R.C.P., M.R.C.S. . . . .	432
On Accidental Concealed Hemorrhage, with Cases. By J. Brunton, M.A., M.D. . . . .	437
Abdominal Cyst, with Pulmonary Complication. By G. Granville Bantock, M.D. . . . .	489
The Mucous Membrane of the Body of the Uterus. By John Williams, M.D. . . . .	496
On the Avoidance of the Frequent Use of the Speculum. By James Braithwaite, M.D. . . . .	504
On the Diagnosis and Treatment of the Curable Forms of Fibroid Tumours of the Uterus. By Alfred Meadows, M.D., F.R.C.P. . . . .	561
Four Cases of Ovariectomy. By C. H. Routh, M.D. . . . .	579
The Treatment of Hooping-Cough by the Iodide of Silver. By Robert Bell, M.D. . . . .	589
On Laceration of the Gravid Uterus. By J. Ashburton Thompson . . . . .	633, 718, 804
On the Tolerance of Sedatives in Malignant Uterine Disease. By W. H. Day, M.D., M.R.C.P. . . . .	646
A Case of Difficult Labour from Abnormal Rigidity of the Os. By George Calderwood, M.D. . . . .	657
On the Weight of New-Born Children. By E. Ingerslev . . . . .	705, 777
Folding Short Forceps. By W. Draper, M.R.C.S., L.M. . . . .	715
Remarks on an Unsuspected Cause of Defective Hearing, and of Deaf-mutism in Infancy and Childhood. By J. R. Cassells, M.D., M.R.C.S. . . . .	799

# ABSTRACTS OF SOCIETIES' PROCEEDINGS.

	PAGE		PAGE
Clinical Notes on the Early Course of Cancer of the Cervix Uteri . . . . .	41	Notes on a Case of Triplets, complicated by Double Uterus . . . . .	469
Epithelioma of Cervix Uteri complicated with Pregnancy . . . . .	47	On Dysmenorrhea . . . . .	469
Description of a Diprosopus Triophthalmus Monster . . . . .	48	Syphilitic Placenta . . . . .	472
On Prickle Cells in the Capsule of a Fibrous Polypus of the Uterus . . . . .	54	Note of Inquiry with reference to some points in the Management of the Third Stage of Labour . . . . .	474
Anencephalous Fetus . . . . .	55	Vomiting in Connexion with Pregnancy . . . . .	474
Ovulation without Menstruation . . . . .	56	Neuralgic Dysmenorrhea . . . . .	475
On Metro-peritonitis following the use of the Ordinary Female Syringe; a plea for the Vaginal Irrigator . . . . .	56	Case of Rupture of the Umbilical Cord during Delivery . . . . .	476
On the Relation of Puerperal Fever to the Infective Diseases and Pyemia . . . . .	97, 180, 248, 322	Remarks on Hooping Cough and its Treatment with Carbolic Acid Vapour . . . . .	476
On the Risks run by Obstetricians . . . . .	123	The Pneumonias of Childhood as Illustrated by Clinical Charts . . . . .	477
Fetal Monstrosity (Phoco-Melos) . . . . .	126	Contributions to the Determination of the Diminution of the Uterus after Delivery . . . . .	477
Hair in the Female Bladder . . . . .	127	Exhibition of a Pessary . . . . .	512
Fibroid Polypus . . . . .	127	Case of Complete Inversion of the Uterus . . . . .	513
Case of Central Rupture of the Perineum . . . . .	128	On a Dissection of a Uterus . . . . .	515
A Case of Extra-uterine Gestation . . . . .	129	Report of, and Observations upon, a Case of Puerperal Tetanus . . . . .	516
On Protracted Labour, Hour-glass Contraction, Hemorrhage, and the introduction of the Hand into the Uterus . . . . .	134	The Development of Cysts in the Stroma of the Ovary . . . . .	527
Fibrous Polypus . . . . .	202	A Case in which the Fetal Heart was heard early in Pregnancy . . . . .	529
Enlargement of the Thyroid Body in Pregnancy . . . . .	203	On the History of the Short Straight Midwifery Forceps as a Tractor, and of the Long Double Curved Forceps as a Compressor and Lever . . . . .	532
Exhibition of Crosse's Work on Inversio Uteri . . . . .	213	Hydrocephalic Head impeding Delivery . . . . .	599
Suppurating Tumour of Left Ovary . . . . .	268	General Anasarca in a Fetus . . . . .	599
Specimen of Extra-uterine Fetation . . . . .	269	Case of General Dropsy in a Fetus . . . . .	600
Intra-mural Calcareous Tumour impeding Labour . . . . .	269	On a new Form of Blunt Hook and Sling for assisting Delivery in Cases of Breech Presentation . . . . .	601
Case of Extreme Dropsy . . . . .	269	On Prolapse of the Funis during Labour . . . . .	602
Case of Monstrosity . . . . .	269	Note on a Diseased Placenta . . . . .	603
Placental Polypus . . . . .	270	Case of Spina Bifida . . . . .	603
Report of a Case in Midwifery Practice . . . . .	270	Further Observations on Fibrous Tumours of the Uterus . . . . .	604
Case of Ovarian Tumour . . . . .	277	On the Use of the Actual Cautery in the Enucleation of Fibroid Tumours of the Uterus . . . . .	617
Specimen of Blighted Ovum . . . . .	320	Note on the Discharge of Ova, and its relation in point of Time to Menstruation . . . . .	620
Specimen of a Uterus . . . . .	320	Anencephalic Fetus . . . . .	667
Dermoid Cyst . . . . .	321	Irregular Pigmentation . . . . .	668
Venous Coagulum . . . . .	321	Epithelioma of the Cervix Uteri . . . . .	668
Colloid Tumour . . . . .	321	Note on the Post-mortem Diagnosis of a Nulliparous Uterus . . . . .	670
Extra-uterine Fibroid removed by Gastrotomy . . . . .	321	Note of a Case of Ruptured Vagina during Labour . . . . .	671
On Prolapsus and Retroversion as alternating misplacements of the Gravid Uterus . . . . .	394	Spontaneous Rupture of the Vagina . . . . .	672
On the means of ascertaining the length of Gestation . . . . .	397	Case of Cesarean Section . . . . .	673
Cases of Fatal Hemorrhage after Childbirth . . . . .	413	Exhibition of Polypt . . . . .	674
A Case of Intra-uterine Fibrous Polypus . . . . .	415	Case of Hypogastric Fistula . . . . .	674
Note on Intra-uterine Craniometry . . . . .	417	Exhibition of Specula . . . . .	674
Embryotomy, its Various Modes of Procedure; with Illustrative Cases . . . . .	457	Emmenologia . . . . .	675
On a Form of Accidental Hemorrhage alternating with Watery Discharge . . . . .	458	Dr. Lombe Atthill's Inaugural Address . . . . .	688
A New kind of Midwifery Forceps, and an Obstetric Thimble for rupturing the Membranes . . . . .	458	Maternity Hospitals . . . . .	695
Ovarian Dropsy: some points in its Pathology and Treatment . . . . .	459	On Puerperal Fever . . . . .	737
On the Prevention and Management of Miscarriages . . . . .	459	Memoir of Sir Charles Locock . . . . .	755
Placenta Prævia . . . . .	460	Sarcoma Uteri . . . . .	759
Case of Inversion of the Uterus of three Months' standing . . . . .	460, 462	New Preparation of Ergot . . . . .	822
Memoir of the Life of William Smellie, M.D. . . . .	463	Cyst of Larynx in an Infant . . . . .	822
The Examination of the Female Bladder . . . . .	465	Carneous or Fleshy Mole . . . . .	822
Intemperance in Women, with special reference to its Effects on the Reproductive System . . . . .	465	Fibroid of Uterus . . . . .	823
Obstetrical Statistics . . . . .	466	Malignant Tumour of Omentum removed Post-mortem . . . . .	824
On Uterine Flexions and Displacements, and their Mechanical Treatment . . . . .	466	New Chloroform Inhaler . . . . .	824
Nitric Acid as a Caustic in Uterine Practice, and its Superiority as such to Nitrate of Silver . . . . .	467	Fibroid Tumour of the Uterus . . . . .	825
On the Management of the Lying-in Woman . . . . .	468	Extra-uterine Fetation . . . . .	825
Notes of a Case of Transfusion by Aveling's Apparatus . . . . .	469	Double or Dicotyledonous Placenta . . . . .	826
		Laceration of the Perineum . . . . .	830
		Against the Pendulum Movement in working the Midwifery Forceps . . . . .	834





# OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

~~~~~  
No. XXV.—APRIL, 1875.  
~~~~~

Original Communications.

---

## CONTRIBUTION TO THE STUDY OF NON-CAN- CEROUS TUMOURS OF THE BREAST.

(*Arch. gén. de Méd.*, VI<sup>e</sup> Sér., t. 25, p. 22, Jan. 1875.)

By Dr. CHARLES MONOD,

*Chef du Laboratoire de l'Hôpital des Cliniques.*

Translated by CHARLES J. CULLINGWORTH, Surgeon to  
St. Mary's Hospital, Manchester.

NOTWITHSTANDING the large number of publications upon tumours of the breast, the study of their anatomy remains to a great extent obscure and difficult. Yet there are some points in their history about which there can no longer be a possible doubt. Such at least is the impression left on our mind by the microscopical examination of a considerable number of specimens, which we have made during the past two years, at the histological laboratory of the College of France.

It is not our intention to offer here a complete study of the question. We shall limit ourselves to the indication, in few words, of the conclusions to which the examination of our preparations has led us.

Our attention has been chiefly directed to breast-tumours,  
No. XXV.—VOL. III.

called *benign* or *non-cancerous*. We shall principally apply ourselves to the elucidation of their general characters, after having briefly sketched those of cancerous tumours rightly so-called. This will necessarily bring us to deal with the question, always disputed, of the frequency and even the existence of adenomata of the breast.

The anatomical diagnosis of true cancer of the breast is seldom beset with any difficulty. It appears most frequently, in fact, with characters precisely like those which it bears in every other region of the body : a fibrous framework, forming alveoli, in which are enclosed cells of variable form and size. Further, and upon this point we are wishful to insist, the constituent elements of the mammary gland, its *acini* and excretory ducts, disappear before the development of the cancerous tissue.

Nevertheless, in certain cases, where the entire breast is not yet invaded by the pathological product, gland-tissue can be met with intact at the borders of the tumour. With fortunate and sufficiently extensive sections, one may perceive, in fact, at one end of the preparation, cancerous tissue, recognisable by the characters enumerated above ; at the other, some gland lobules of normal size and appearance. The neighbouring tissue, and this more and more as the seat of complete degeneration is approached, presents traces of a manifest alteration, in which the first stage is an abundant cell-proliferation. Cell-elements appear on the field in increasing number and size. Here and there they form little groups, and are located between the bundles of connective tissue, which seem to open out for their reception. Here evidently is seen the first stage of the alveolar formation of the cancer. These rudimentary alveoli may sometimes only contain one or two large cells.\*

The lesion continuing its march of invasion, the cells continually increase in number and size, and the spaces which they occupy enlarge in proportion, and become more and more numerous. Little by little also the gland-lobules,

---

\* D'Espine, "Arch. de Physiologie," 2<sup>me</sup> Sér. t. i. p. 177, 1874.



hitherto intact, disappear, obliterated as it were by the march of the pathological process.

Thus the point is arrived at by degrees where the alveolar tissue of the cancer alone exists; the glandular tissue has entirely disappeared.

Quite different is the physiognomy of the non-cancerous tumours of the breast. Here the gland-tissue persists, altered in form it is true, but yet always recognisable by means of the regular epithelial coat which lines the enlarged gland-spaces.

This persistence of the gland-tissue explains the confusion into which many authors have been led, and the numerous cases described under the title of adenomata of the breast. We shall endeavour to show that the characteristics of these tumours must be sought for, not in the glandular element, always more or less perverted, but in the intermediate tissue; and that the exact analysis of that tissue alone permits us to define their nature with precision.

We are aware that this mode of meeting the question before us is not new, and has been already partially set forth in previous publications. Recent discussions, however, having shown that it was far from being sufficiently known and popularized, we have thought that it might perhaps be useful to take advantage of our personal researches, in order to bring it once more into prominence.

Non-cancerous tumours of the breast, clinically very different from each other, present certain common anatomical characters which allow of their being brought together, and show, on the other hand, important differences which justify the recognition of a certain number of varieties amongst them. Their physical characters are very variable, and in no way help to place them in distinct categories. It will be sufficient to remember in fact, that they include the largest and the smallest breast-tumours; that their consistence, which generally reaches neither the softness of encephaloid, nor the hardness of scirrhus, may for the rest vary considerably; that their form is still less characteristic. Nevertheless, to the unaided eye, they all present one character in common, which has not escaped careful observers.

The fundamental tissue of the tumour, even when it appears at first dense and of homogeneous structure, is traversed by numerous clefts and furrows: some scarcely appreciable by the unaided eye; others quite visible and admitting a probe easily; others again, still more considerable, may open out into actual cavities of a cystic appearance, and of an elongated or rounded form. It is usually easy to make out that these cavities communicate with each other here and there; others, on the contrary, appear completely isolated. The largest often contain in their interior a mammillated projection, in form like a mushroom, which seems to be developed at the expense of one of their walls. This projection itself presents a pinked or furrowed (*déchiquetée*) surface; it is subdivided into a great number of lobes, and smaller and smaller lobules, which give it a cauliflower aspect. The tissue of which it is composed, appears on section of the same nature as the rest of the tumour. Frequently, however, it is of softer consistence, and may even, according to the variety to which it belongs, be in process of softening.

We shall see presently that this papillary appearance, which only reveals itself to the unaided eye when these vegetations have acquired a large size, is found under the microscope throughout the whole of the tumour, in different stages of development, from the simple upraising of the cyst-wall to the completely formed papilla.

It is fully proved at this day that these hollow spaces, whose existence had not been overlooked by those occupied with the study of breast-tumours, are no other than the gland-cavities of the breast, abnormally developed and altered in form by the pathological tissue which has arisen in the gland. They have been described by M. Ranvier, under the name of *kystes lacunaires*, to distinguish them from true cysts, such as may be met with in tumours of the breast.

All these cavities present a perfectly smooth surface, which recalls in every particular the appearance of an epithelial surface. It is easy, indeed, to show that they are really covered by epithelium. For this purpose it is sufficient,

if the tumour has been recently removed, to let fall into the cavity of one of these cysts a solution of nitrate of silver 3 parts to the 1000; when permeation has occurred, a thin fragment of the cyst-wall is placed under the microscope, the section being parallel to the surface. An epithelial layer is at once recognised, in all respects similar to that which the wall of a normal gland duct would furnish under similar circumstances. This epithelium is found also on examination of thin sections under the microscope, made after hardening. These are the results furnished by this second mode of study, which we must now describe. We have no need to insist in this place upon the inadequacy of preparations obtained by the scraping of specimens in the recent state, or the crushing of small fragments torn from the surface of the tumour. By this means epithelial cells are easily obtained from the gland-ducts, often grouped indeed in a manner which gives a true cast of the culs-de-sac; and one is thereby led quite naturally to make an anatomical diagnosis of a glandular tumour or adenoma. The elements of the intermediate tissue between the culs de-sac most frequently in fact are overlooked by that mode of examination; either because being very firmly placed in the situation they occupy, they are less easy to remove by scraping; or because being less characteristic in form, they attract less attention.

An examination of the liquid collected by means of scraping (and particularly that obtained from a fragment which has been in Müller's solution for twenty-four hours) is not without its use. Only in that way can the elements of the tumour be observed in a state of isolation, and accurate ideas be gained with regard to their form and size.

But in order to form a complete anatomical diagnosis, the analysis must be carried further; it is not in reality the observation of the isolated elements of a tissue, but of their mutual arrangement, which will permit us to come to definite conclusions as to its nature. The examination of thin sections, taken from a mass suitably hardened, and coloured in the ordinary way, ought then always to follow that of the tumour in its recent state. After colouring



similar preparations with the picro-carminate of ammonia, an arrangement at once strikes us which recalls and explains what had been noticed during the examination of the tumour with the unaided eye. We see at the first glance, with a low power, that the pathological tissue is traversed by numerous cavities or empty spaces, bordered by wavy lines of a deep red colour, contrasting by their staining with the surrounding parts. With a higher power it is easy to note that these deeply-coloured lines, which form the borders of the hollow spaces in the tumour, are composed of prismatic epithelial cells, in regular rows and in a single layer. These hollows and epithelial surfaces evidently correspond to the more or less altered glandular structure of the breast. It is indeed possible to perceive by the side of hollow spaces of considerable diameter, where it is difficult to recognise the proper structure of the transformed gland, true gland-tubes which have preserved their normal texture and appearance, presenting themselves in the form of longer or shorter ducts, without any appreciable cavity, owing to the exact apposition of their opposite walls, and dividing several times at one end, recalling in short the aspect of a glandular canal leading to a vesicle (*acinus*). At other times the tube-walls are separated from each other; they then appear elevated by more or less numerous *papillæ*, as if the surrounding tissue were trying to force its way into their cavity.

Lastly, these glandular spaces have, further on, become quite unrecognisable; partly on account of the enormous dilatation they have undergone, and partly on account of the existence in their interior of projections of gradually increasing size. The presence of the epithelium above described is the only point in common which unites them together, and allows these diverse forms of the same lesion to be approximated.

Between these three every intermediate degree may be observed, differing one from another only in the greater or less dilatation of the cavities, or the more or less considerable budding of the walls. This appearance, with differences of detail, may be found in tumours which, as we shall show immediately, are yet of an entirely different nature.

Nevertheless, one may say in a general way, that the more rapid the development of the tumour has been, or the more remote the examination from its commencement, the fewer gland-tubes of nearly normal aspect will one find, the more considerable, on the contrary, will be the hollow spaces and the budding of the walls.

Finally—and this is the point upon which we are chiefly wishful to insist—it follows from these facts that, as we have indicated above, it is not in the glandular element that the distinctive characters of the different tumours of the breast ought to be looked for. These distinguishing characters can only be found in the stroma intervening between the gland-vesicles and the dilated excretory ducts, in the tissue which constitutes the walls of the hollow spaces, or the buds which project into their interior. This tissue, in the preparations that we have examined, has appeared sometimes with the characters of fibrous tissue, either completely formed or more frequently young, and abundantly supplied with cell elements; sometimes presenting in every particular the characteristics of sarcomata and myxomata when developed in any other part of the body.

We shall not insist upon the histological differences which separate these three varieties of pathological tissues. In certain cases some difficulty may be experienced in distinguishing them one from another; in tracing, for example, a well-marked line of demarcation between fibrous tissue in course of development and true sarcomatous tissue; besides, mixed forms will occur, in which there will be a mixture, often in unequal proportions, of sarcomatous and myxomatous places. The study of these questions belongs to the anatomical diagnosis of tumours in general.

The important point, if our way of understanding the mode of constitution of the tumours under consideration is correct, is that, each one presenting a common point of structure—the alteration of the gland-ducts, and each offering, on the contrary, a distinctive character—the mode of constitution of their stroma, it is evidently according to the nature of this last that they must be distinguished, and consequently named. In one word, they must be considered as *fibromata*, *sarcomata*,

or *myxomata* of the breast, and not as more or less altered *adenomata*.

Nevertheless it does not seem to us to follow from the facts we are here stating, that the notion of a concomitant glandular hypertrophy must be altogether discarded. Increase in the number of the *acini* is possible, and even probable. If on the one hand, indeed, the small proportion of the glandular element of the breast in that of a young woman, who has had no children, and especially of one who has not suckled, be taken into account ; and, on the other hand, the large number of dilated and altered *acini* met with in little fibrous tumours of the breast, developed under the same circumstances—one is forcibly led to admit that a process of hypertrophy in the gland itself has accompanied the formation of the neoplastic tissue in the intervening stroma. But, to repeat it once more, it is not in that alteration that the characteristic of the lesion must be looked for, because it is found with analogous characters in tumours of different kinds. In support of this view an argument may be adduced, already put forth by MM. Cornil et Ranvier in their “*Manuel d'Histologie pathologique*,” and drawn from the examination of tumours of this kind, returning after removal. The new tumour no longer contains gland-tissue ; an evident proof that, in the primitive tumour, the glandular hypertrophy constituted a simply accessory element.

To sum up, if it is sought to give an account of the mode of general development of these tumours, it may be granted that there is formed in the interlobular tissue, following the same mechanism as in the connective or fibrous tissue in any other part of the body, a neoplastic tissue, which assumes, according to the case, the characters of a fibroma, a sarcoma, or a myxoma. At the same time, and probably on account of the irritation in the neighbourhood, the *acini* of the gland hypertrophy, increase in number and size ; they are, so to speak, thrust into prominence (*éталés*) and altered in form by the processes set agoing around them. The cavities limited by their walls constitute spaces perfectly available, where the newly-formed tissue tends to develop at ease in the form of buds and *papillæ* which project more and more into their



interior. In this respect these tumours may be compared with those which develop in other cavities of the economy, and which generally show also a tendency to assume this budding and papillary form. We know, indeed, the frequency of papillary growths in the bladder, in the rectum, in the mucous membrane of the mouth, tongue, larynx, &c. Here too the anatomical form is similar in every case, as is indicated by the generic term *polypi*, under which the greater part of these cases have long been confounded. A more exact examination has since proved that, in cases of that kind, we may have to deal with tumours essentially different the one from the other.

Non-cancerous tumours of the breast generally present themselves under two leading forms. In the one, the tumour, generally small, is traversed by the narrow clefts we have described, but of true cystic cavities it presents few or none : in that case it turns out generally a fibroma of the breast, but occasionally also a sarcoma. This last fact explains the recurrence of tumours apparently benign, their growth becoming suddenly rapid, or yet again their pretended cancerous transformation. In a second form, cystic dilatations, often of considerable diameter, predominate (cysto-sarcomata) ; the tumour is usually large, it is accompanied sooner or later with ulceration, or rather perforation of the skin, through which a fleshy-looking granulation appears. These two forms are referable however to one fundamental type ; they constitute a natural group of tumours, which, for the anatomist and the clinical worker (*clinicien*), deserve to be separated from cancer. Attentive observation of the stroma will also permit their being divided into a certain number of varieties, important to be distinguished from the double point of view of anatomical diagnosis and exact prognosis.

We have only had in view, in these observations, the *papillary* forms of the non-cancerous tumours of the breast, which are the most frequent. We know that they may assume another form, that of fibromata, sarcomata, and myxomata *en masse*. We have had no opportunity of observing them, and in regard to them we confine ourselves to this simple mention.

It will not be uninteresting to recall briefly the different phases through which the history of the variety of breast-tumours we are describing has passed. Sir Astley Cooper, first of all,\* relying only upon observation of patients and examination of specimens with the unaided eye, knew how to distinguish from cancer of the breast certain tumours whose anatomical and clinical characters deserved the qualifying epithet of benign. He divided them into two classes: the first, which he denominated *hydatid disease of the breast*, comprised the diverse varieties of cystic disease of the breast. The second constituted in his view the type of a benign tumour—it is the *chronic mammary tumour*; it is generally developed between the ages of seventeen and thirty, its principal anatomical character is a multiplicity of lobules separated by clefts and depressions; this arrangement persisting whatever size the tumour may acquire.

The division of A. Cooper, based upon numerous observations, may, in its leading features, be preserved even at this day. Amongst non-cancerous tumours of the breast, some are chiefly remarkable for the large number of cysts which occupy them; others, approaching nearer in their aspect to the normal gland tissue, are only traversed by elongated cavities, many of which may elude a superficial examination. Astley Cooper did not see the relation which existed between these two groups of tumours, in appearance so different. Indeed, it could not be made out with the insufficient resources for the examination of tumours which were then at his disposal. The intervention of the microscope did not at first seem as if it would throw much light on the question. Yet it allowed Müllert† to establish upon a definite basis the group of cysto-sarcomata. The hydatid disease of the breast of Astley Cooper comprised three varieties: simple cysts, compound cysts, and parasitic or hydatid cysts properly so-called. These last might be set aside on account of their extreme rarity; their characters, altogether special, evidently formed

---

\* Cooper, A., "Illustrations of the Diseases of the Breast." Part I. London, 1829. P. 20 *et seq.*

† Müller, J., "Ueber den feineren Bau und die Formen der krankhaften Geschwülste." P. 56. Berlin, 1838.

them moreover into a distinct class. J. Müller brought together the two first varieties of Astley Cooper, and referred them to the same origin, by pointing out that simple cysts properly so-called were very rare, and that more frequently in cases of that kind we were dealing with cysts formed in a neoplastic tissue, cavities tunnelled in a sarcoma. These cysts too might be simple or compound, as had been admitted before him, or might assume a third form, which he described carefully, and denominated *cysto-sarcoma phyllodes*; it was characterized by projections and numerous vegetations which pierced through into the cavities of the tumour.

Müller confirmed then the theory, already broached by Astley Cooper, that tumours, quite distinct from cancer, may be developed in the breast, composed of a special neoplastic tissue, equally distinct, too, from simple hypertrophy of the breast either partial or complete. The mistake of this writer was in not inquiring into what part the gland might take in the constitution of the tumour, particularly in not seeing the relation which existed between the normal cavities of the former and the cystic cavities of the latter. He left quite unnoticed the tumours described by Astley Cooper as chronic mammary tumours, which might perhaps have enabled him to recognise this relationship.

In France, Lebert, Velpeau, Robin, and Broca fell into the opposite error. Searching under the microscope for characteristic elements in the pathological tissues that they examined, and finding in this group of breast tumours, evidently non-cancerous, only normal gland-elements, connective tissue, culs-de-sac, and epithelial cells—they were led to look upon these products only as modifications more or less considerable of the normal gland without true neoplasm. The names they gave to these tumours clearly indicate the idea they entertained as to their nature—partial hypertrophy of the breast (Lebert), adenoid tumours (Velpeau), adenomata (Broca). The views of these observers are too well known in France for it to be necessary to expound them at length. They will be found, moreover, very clearly and faithfully set forth by M. Broca in the article “Adénome,” in the *Diction-*



*naire encyclopédique*. What they may be reproached with is, that they did not sufficiently take account of the modifications undergone by the intervesicular (*interacinaire*) tissue of the gland. That study would have enabled them to distinguish amongst the group of tumours, similar in appearance, which they united under the name of adenomata, varieties corresponding to the teachings of the bedside: they were not long, in fact, in noticing that in these tumours, reputed benign, the prognosis was far from being always the same. Their merit is to have brought into prominence the important point which had escaped Müller, the recognition of the relation which existed between the pathological product and the normal gland, and especially between the cavities of the tumour and the normal hollow spaces of the breast. Lebert did not even go far enough in that direction. The closed cysts of that writer, since called by M. Broca glandular cysts, were the only ones formed, according to him, by the dilatation of the normal cavities of the gland. He did not recognise the same origin for those cysts which he named *lacuneux*, and which, he held, were tunnelled out in the fibrous framework of the breast.

We have seen above that in fresh specimens, examined immediately after removal, it was possible to show that these clefts or *lacunæ* were themselves derived from the gland-ducts.

Busch,\* in Germany, had already established the reality of that fact by simple examination with the unaided eye. In a specimen that he had, he was able to pass a fine stylet into one of the lactiferous ducts in the neighbourhood of the nipple, and to reach the cystic spaces, large and small, of the tumour. By the same means Baur† has since arrived at the same conclusion.

We pass by the writings of Birkett,‡ Paget,§ Metten-

\* Busch, W., "Chirurgische Beobachtungen." Berlin, 1854. Pp. 84, 85.

† Baur, Reichert's und Du Bois-Reymond's "Archiv." Berlin, 1862. P. 169.

‡ Birkett, J., "Diseases of the Breast." London, 1850.

§ Paget, Sir J., "Lectures on Surgical Pathology." Third Edition. By W. Turner. London, 1870.

heimer,\* Schuh,† Rokitsansky,‡ and Reinhardt,§ who did not materially advance the subject, and come immediately to those authors who, with regard to the anatomical diagnosis and hence also to the prognosis, attach more importance to the study of the intervesicular tissue than to the alteration in form of the glandular portion of the breast properly so-called.

A first trace of the necessity of that distinction is found in a communication made to the Société anatomique by M. Verneuil, at a time when Lebert's views were yet in full favour. Giving the result of the microscopical examination of a tumour, he said,|| "In the portions of the tumour which preserved a glandular appearance, the form of culs-de-sac is maintained, while the elements of the walls are altered and have been replaced by those of the fibro-plastic tissue. This remark is important with regard to the erroneous prognosis to which the purely glandular appearance of the tumour would have given rise ; concerning himself only with the form, the surgeon might have fancied that he had only to do with a hypertrophic tumour, while in reality he was dealing with a fibro-plastic tumour, more liable to return." The indication there was correct and useful to remember, and it called for new investigations.

Billroth, first of all, in his well-known memoir,¶ gave us a collective account of non-cancerous breast-tumours from sundry remarkable points of view. The alterations in the gland-ducts are there laid down in all their details ; according to him they are but accessory lesions, simple alterations in form, consecutive to the development of neoplastic products in the intermediate tissue ; the dilatation and lengthening

\* Mettenheimer, "Beschreib. eines Cystosarcoma phyllodes." Müller's "Archiv," 1850.

† Schuh, F., "Erkenntniss der Pseudoplasmen." Wien, 1851. "Pathologie und Therapie der Pseudoplasmen." Wien, 1854.

‡ Rokitsansky, C., "Lehrbuch der pathologische Anatomie." Dritte Auflage. Wien, 1861. Bd. iii. p. 527 *et seq.* ; and "Wien. Sitzungs-Berichte," 1851.

§ Reinhardt, "Pathologisch-anatomische Untersuchungen." Berlin, 1852.

|| Verneuil, "Bulletin Soc. anat.," 1858, p. 329.

¶ Billroth, T., "Untersuchungen über den feineren Bau und die Entwicklung der Brustdrüsen-Geschwülste." Virchow's "Archiv," Bd. xviii. Berlin, 1860. P. 51 *et seq.*

of these ducts explain the formation of the cysts and clefts which traverse these tumours. Billroth did not the less give them on account of their external resemblance to the gland the name of adenoid sarcomata, to distinguish, he said, this variety of sarcomata from sarcomata spoken of as fibrous, gelatinous, encephaloid, and medullary. Here, at any rate, was a mistake in words. Indeed, if these last forms of sarcomata constitute distinct varieties, the sarcomatous tissue in each of them has its special characters, resulting from modifications undergone in its intimate structure. The qualifying adjective, adenoid, has quite a different meaning; it simply recalls the seat of the development of the neoplasm, and does not at all point out any special character of the pathological tissue properly so-called. The diverse species, indeed, of sarcoma in general may be met with in sarcoma of the breast; the distinction, such at least as Billroth meant, is then illusory.

The fundamental idea in Billroth's essay was nevertheless correct. It is quite as applicable to fibromata and myxomata, as to sarcomata of the breast. M. Ranvier long since upheld that opinion in sundry communications of the Société anatomique, and summarized by him in his "*Manuel d'Histologie pathologique*."\* He it was who, more than any other in France, raised his voice against the old conception of adenomata of the breast, such as was held by Lebert, and the writers who succeeded him. His argument rested chiefly on the three following points:—the constant multiplication of glandular epithelial cells, with dilatation of the excretory ducts and acini, and various modifications of their contents, each time that a newly-formed product arises in the stroma of any gland, a fact which explains the considerable part which the gland appears to play in the structure of the tumour; the absence, in tumours which have recurred, of hypertrophic glandular portions which in the original tumour passed for the characteristic alterations, and which established unanswerably that we must seek further, to wit, in the intervesicular

---

\* Cornil, G., et Ranvier, L., "*Manuel d'Histologie pathologique*," 1<sup>re</sup> partie. Paris, 1869. P. 94.



tissue, for the elements of the anatomical diagnosis of these tumours ; lastly, the exact definition of true adenoma, which necessarily led to a mind open to the recognition of its extreme rarity.

Virchow, at the same time, in Germany, came by a different route to analogous conclusions. Thus, in his treatise on tumours, as in the work of MM. Cornil and Ranvier, it is no longer a question of adenoid tumours or adeno-sarcomata of the breast, but only of sarcomata, fibromata, myxomata of the breast, whose investigation only comprises one special branch of the investigation of sarcomata, fibromata, and myxomata in general.

We shall notice, in conclusion, two publications devoted to this subject, both of quite recent date.

The first is an inaugural thesis at Zürich, in 1871. The author, M. v. Wyss,\* furnishes an interesting and very complete study of non-cancerous tumours of the breast ; relying upon some personal observations, he sets forth the whole anatomical and clinical history. For the rest he adopts altogether the views previously announced by Billroth and Virchow.

M. Cadiat,† in a very elaborate memoir, looks at the subject from quite a different stand-point. Accepting the views of Velpeau, Lebert, and Robin, he sets himself to prove their truth by new arguments, into the *minutiæ* of which we cannot enter.

We shall content ourselves with producing the leading conclusions of the author ; it will be seen to what extent they differ from our own. According to him, indeed, "Adenoma with multiple forms is the most common breast-tumour . . . it begets the cystic tumour, whence are derived the cysto-sarcoma, and even the tumour called sarcoma. . . . Almost every tumour of the breast is composed of a mixture of a glandular element with a fibro-plastic element. This glandular element, which always precedes the other in development,

---

\* Wyss, H. v., "Beitrag zur Kenntniss der Brustdrüsen Geschwülste." Diss. Zürich, 1871.

† Cadiat, 'Du développement des tumeurs cystiques du sein.' "Journ. de l'Anatomie et de la Physiologie." Par C. Robin. No. 2, 1874. Paris. P. 183.

and which is always found with all its varieties in the very midst of the fibro-plastic tissue which surrounds and invades it, may be regarded as the fundamental element of mammary tumours; it may be said that it is that which is their characteristic. They are then essentially glandular."

We have set forth, in the foregoing pages, the reasons which hinder us from adopting that view.

## ON THE POSTURAL TREATMENT OF PRO- LAPSUS OF THE FUNIS UMBILICALIS.

By JOHN BRUNTON, M.A., M.D., L.F.P.S.G., L.M.,

Medical Officer to the Great Northern Railway Company; Surgeon-Accoucheur to the Royal Maternity Charity; Councillor of the Medical Society of London; Fellow of the Obstetrical Society," &c.

THE larger portion of this paper was read to the Medical Society of London, on the 5th December, 1870. Several reasons actuated me in writing it.

*Firstly*.—Because of the very high infant mortality that is met with in cases of labour where prolapse of the funis occurs, notwithstanding the *ordinary* means of treatment.

*Secondly*.—Because we now possess a means that hitherto was comparatively unknown to most of those engaged in the daily practice of midwifery.

*Thirdly*.—Because this method of treatment is not mentioned in most of our manuals and standard books on obstetrics, and if mentioned, it is only in a cursory way. All that Dr. Barnes, in his most able work on "Obstetric Operations," says of the method on which I am about to speak—viz., the postural method of reduction of the prolapsed funis, is that, "by the knee-elbow posture, indeed, all instruments may occasionally be dispensed with" (p. 12). "The postural or knee-elbow position will much facilitate this operation"—*i.e.*, reduction of the cord (p. 179).\*

\* Before I wrote this, Dr. Wilson, of Glasgow, however, had published some cases, with a description of the operation—in form of a pamphlet—and so had Dr. Dyce, of Aberdeen. More lately, Dr. Meadows describes the operation in his "Manual of Obstetrics;" and Professor Leishman, of Glasgow, in his "System of Midwifery," enters into the merits of the question.

*Fourthly.*—Because the means—treatment by posture—as far as it has been tried, has been so eminently successful as to reduce the ratio of infant mortality to a minimum, without in the slightest degree endangering the life of the mother, or even of producing after results, which, though unpleasant, are by no means fatal in their character.

I may illustrate what I mean by this, by saying, that as the use of the forceps has been advocated for speedy delivery when the cord is presenting, too rapid a delivery, in our anxiety to save the life of the child, may produce laceration of the perineum, not in itself dangerous, but very unpleasant both to patient and medical attendant.

It is not my intention in this paper to enter so fully into the discussion of the subject of prolapsus of the funis, as rather to review the various methods which have been suggested for the treatment of that complication of labour.

Prolapse of the funis is an accident which, while it produces comparatively no danger to the life of the mother, is exceedingly hazardous to that of the child.

In the great majority of cases, if the prolapse is permitted to continue till the delivery of the child is accomplished by the natural efforts of the mother, death of the child results,—the cause of death being obviously from pressure which obstructs the fetal circulation, “as surely as if breathing were prevented after birth.”

Occasionally we meet with cases in which, the funis being prolapsed, delivery takes place, and the child is born alive; these cases, rare as they are, occur in women who have large pelves, with soft and dilatable parts, and in whom the delivery is very rapid. But in the greater number of cases death is sure to follow, if we do not protect the funis from pressure, or deliver the child with a measure of rapidity,—in other words, subject the cord to pressure for as short a time as possible.

I here enumerate, as briefly as possible, the various causes of prolapse as set down by various authors on obstetrics:—

I. An excessive amount of liquor amnii, the child being of the ordinary size.

No. XXV.—VOL. III.

C



2. The same cause, with a small child.
3. A wide pelvis, with ordinary sized child.
4. A wide pelvis, with small child, and with this, the presence of twins.

5. Malposition of the fetus, as shoulder or arm presentation, which does not permit the circular os uteri at all points to closely embrace the presenting part, on account of its irregularities.

6. Deficient action of the lower segment of the uterus, for "the uterus is the chief means by which the cord is prevented from falling down between the presenting part of the child and the passages, from the closeness with which its inferior portion encircles it; without this, from the erect posture of the human female, there would be a liability to prolapsus of the arm or cord in every labour." (Rigby.)

7. For a similar reason to that of malposition, a narrow pelvic brim—the funis is apt to float down with the escaping liq. amnii.

8. Position of the placenta near the os, and therefore low insertion of the cord.

9. Length of cord; if very long. (I have seen it sixty-three inches.)

10. Dropsy of the cord, and therefore excessive weight of it.

These are the principal predisposing causes. The usual exciting ones are—

1. Sudden large gush of liquor amnii, when rising to the erect posture.

2. Early rupture of the membranes when the child is small, premature, or malpresenting.

It is not necessary to say anything about the *diagnosis* of prolapsed funis, for it is easy; and as to *prognosis*, we all know from the multitude of cases detailed by obstetric authors, and the anatomical relations of the parts concerned, that it is very serious as regards the child.

As to treatment, all authors are agreed, that if the funis presents with the membranes unruptured, it is wise and prudent to let it alone; for it is obvious that while the waters are retained, but slight (if any) pressure can be exerted on

the funis. At all events, it is hardly possible for the child to be destroyed by the compression in such circumstances.

But if the liquor amnii has escaped and a loop of funis presents, what is to be done?

We must either protect the funis from compression until natural delivery occurs, or relieve it from pressure as speedily as possible.

Now what are the available resources for such procedure?

I. Drs. Smellie, Gooch, Denman, Hamilton, Dewees, Burns, Campbell, and a host of authors recommend version, and show an amount of success, provided the membranes have not been ruptured and the parts are dilatable. Indeed, Mauriceau, in thirty-three cases recorded, turned nineteen times, and lost only one child; but in this case there was an arm presentation requiring version. Many others have been very successful in a similar way.

It is true that by version delivery is usually completed very rapidly, and in a much shorter time than it would be otherwise. This is an advantage certainly, but the risk at the same time is great, for every one knows the large percentage of infant deaths in breech and footling (natural) cases as well as after the operation of version. There is not a complete protection of the cord from pressure. There exists a considerable amount of danger to the child. How often have we been disappointed in version and in breech and footling cases, when the body of the child has been delivered, the pressure of the head upon the cord has been fatal to the child, and this especially in *first* cases, where if I mistake not, the mortality is as high—two in three.

But it is curious to note how Dr. Denman qualifies his recommendation to turn in prolapse, he says, “no attempts to save the child are on any account to be made, but such as can be practised without the chance of injuring the mother.”

Now we come to the gist of this question—viz., treatment by version. It is advised *before* the membranes have been ruptured; at this period there is little or no danger to the cord. But if the membranes have been ruptured, and the liquor amnii has drained away, with the uterus firmly clasped round the fetus, and the cord in the vagina, is turn-

ing advisable? is it safe? or is it safer than other means? or are other means safer than turning? Every one of us who has any experience, knows what risk the mother runs in the operation of version in such circumstances; and every accoucheur who has practised the operation, understands the difficulties to himself, the pain and the unpleasantness; certainly chloroform is an aid of great value, but one has not it always at hand, or does not feel justified in administering it. At the same time is the risk to the child lessened? For my part I think the risks are increased both ways, for the very introduction of the hand, which must, according to Dr. Rigby, be "done with the greatest possible caution," adds to the pressure on the cord, and the very operation of changing the axis of the child, and the withdrawal of the operator's hand, with the foot, subjects the prolapsed cord to additional risk; while to crown the whole, we have yet to resign the cord to the pressure that accompanies the delivery of the body and head.

Dr. Merriman lays down certain conditions in which it is, in his opinion, justifiable to turn. These are supported by Dr. Ramsbotham—viz., when pulsation exists in the cord, the head not having yet entered the pelvic brim; when the pains are not strong, and the soft parts are relaxed and dilatable. Though it is not possible to lay down an exact rule for every case, each should be judged of on its own merits, and the accoucheur is bound to use due deliberation and caution; not a deliberation of such a hesitating character, as to allow sufficient time to pass, for the pressure on the cord to jeopardize the infant or prove fatal to it.

On the whole, whether the membranes are ruptured or not, my opinion is against version, because I shall show that we possess a much more reliable means of treatment, in every sense of the word, safe for the infant, and safe for the mother. If we have these means, it is our bounden duty to employ them. While in the great majority of cases version is, as a means of treatment, to be excluded, still, we might find it on some rare occasion useful, and it is not to be forgotten it was successful in the hands of Mauriceau. Obviously in complex presentations with the funis, it is



the only method to be employed—such as, arm with funis, *placenta prævia* and funis.

The question of version, and the unavoidable pressure on the funis, suggests a most important question, which has a very strong bearing on all the methods of treatment—viz., How long is compression of the cord necessary to cause the death of the fetus? I have searched a number of obstetric works, and in only one—viz., Dr. Alex. Hamilton's, of Edinburgh (1784), have I found a time mentioned; he says, “a pressure on the umbilical cord perhaps for ten minutes, by interrupting the circulation, will be sufficient to destroy the life of the child.”

This time seems far too long, for I am sure in our experience in versions, for example, two minutes' pressure, from non-delivery of the head, has been quite sufficient to destroy the life of the fetus. Notwithstanding there are cases on record and to be met with, where the prolapsed cord has apparently ceased to pulsate, and yet the child has been delivered in time for its resuscitation. Dr. Tyler Smith especially notices this fact. He says, “cases are on record in which the cord, being pressed upon from above, the loop hanging down is pulseless from pressure on the arteries, but the heart may beat for a considerable time after this, and the child be ultimately born alive.” These cases unfortunately are rare. Sometimes change of posture of the cord allows the otherwise feeble heart to rally, and the pulsation of the cord is restored.

II. We now come to the next position. The membranes have been ruptured, the cord prolapsed, and the head is fairly engaged in the cavity of the pelvis. If the os is fully dilated turning is out of the question, or at least not much in it. What is to be done? Most authors, from early days, recommend the application of the forceps to complete rapid delivery. Many lives have been saved in this way. Care must be taken not to enclose the loop of cord within the blades of the forceps. “The extraction must be as rapid as is consistent with the mother's safety.”

It is obvious that in primiparous cases delivery cannot be effected with sufficient rapidity without great danger to the

maternal parts. Also, as a great many cases of prolapse are in first labours, the applicability of the forceps must be limited for the above reason. What may be the exact proportion of prolapses in *primiparæ* I have not been able to ascertain ; in my own limited experience, afterwards to be related, two out of five were such ; elsewhere I found three in twelve.

Admitting the advantage of the forceps in certain conditions, have we any better means of overcoming the difficulty ? When called to attend a labour we have not always a bag of instruments with us. Should prolapse occur, the delay which follows before procuring the instruments would be fatal to the child. But more anon.

III. The next method of treatment is that of reposition of the cord into the uterus, or if that cannot be done, to place the prolapsed cord in such a position that it shall be pressed upon as little as possible.

(a) This process was recommended by Dr. Merriman. The cord is to be brought into the oblique diameter of the pelvis, opposite to that occupied by the child. This procedure is said to be successful sometimes in *pluriparæ*, with large pelves and soft dilatable parts. Nevertheless, it is plain from the change that occurs in the position of the head during the progress of labour, that while the cord so placed may at one time be free from pressure, it is shortly subjected to it again and hazard arises. In *primiparæ* such measures are of little or no avail. (b) Some recommend the pushing of the cord up in order to hook it over the chin. "No doubt," says Dr. Tyler Smith, "this answers the purpose if the cord will keep there." (c) Dr. Ramsbotham advises us to carry the loop up above the presenting part, and retain it there till the next pain comes on, *in the hope* that the head will be propelled somewhat downwards, while the funis remains above. But he remarks, "we shall often be disappointed by the loop reappearing as soon as we remove our fingers." Is not this the experience of all of us ? (d) Next it is recommended to push up the loop and apply a piece of sponge or soft linen, "to act as a stay on which the loop may rest." This process has succeeded now and

again. But is it certain? By no means. (*e*) Dr. Mackenzie enclosed the prolapsed cord in a leathern purse, and carried it up into the uterus. The operation is, however, difficult, and one now rarely if ever attempted. (*f*) Dr. Michælis, of Kiel, suggested and used a gum elastic male catheter, to which he attached, by ligature, the loop of cord, passed the catheter and funis up into the uterus, withdrew the stilette, and allowed the catheter to remain during labour, being soft it did no harm. He has since abandoned this process for the use of the fingers. Of other mechanical means, some of which are ingenious enough but complicated, I need only mention that introduced by Mr. Perkins, of Exeter, in 1843. (*g*) It is described by Dr. Ramsbotham, in his treatise on Obstetric Medicine and Surgery.

The danger that arises in the employment of the mechanical contrivances is due to the ligature; the cord is very slippery, and necessitates some compression to retain it in Mackenzie's bag, to attach it to Michælis' catheter, as well as to Perkins' whalebone; at the same time there is another danger in the last procedure—viz., in withdrawing the tape, which must necessarily be wet, an amount of friction ensues which obstructs the ready passage of the tape and compresses the cord. I would suggest that it might be well to grease the tape before use, if this method be had recourse to by any one.

Dr. Ramsbotham says that we can easily get, on the spur of the moment, a piece of whalebone, "wherever women are present." I presume he means a piece from an attendant's corset; but, in these days of steel corsets, I am afraid there would be great difficulty in getting the whalebone.

I said that we had not always the forceps with us; neither is it likely that we can always carry an instrument designed for reduction of prolapsed funis, seeing that the accident is so rare (1 in every 276 cases). Have we any other means available?

IV. I alluded above to the unsatisfactory result of simply reducing the cord by the hand. Yet the hand is the best and readiest instrument, and always available. I also hope



to show that no other instrument is necessary for successful reduction of the cord.

Sir Richard Croft advises us to carry up the loop of cord, and suspend it over one of the limbs of the child. He was successful in a few cases. The difficulty of accomplishing the object, as well as the risk to the mother in introducing the hand into the uterus, detracts very much from the value of this operation. Indeed, having introduced the hand into the uterine cavity, one might as well go on to complete version.

Before proceeding to describe the method which, when understood, will claim precedence of all others, I may mention that it is quite unnecessary to operate upon the cord, provided the cord be long lapsed, cold, and pulseless; it is then advisable to let the case terminate naturally, if the presentation is natural. Other measures of course are to be taken according to circumstances.

Notwithstanding, as I have mentioned above, there are cases in which it is advisable to reduce the cord, though it be apparently pulseless, if the prolapse is of short duration and the fetal heart can be heard "per abdomen" still beating, if the pressure can be taken off the cord, the circulation may be restored and the child saved.

Indeed, in spite of what I have said above, it is justifiable to reduce the cord in all cases, if there be no risk to the mother, in the hope of saving the child.

It is very interesting in following out the various methods that have been suggested for reduction of the prolapsed funis, to notice that at intervals some approach has been made towards that process which I am now going to advocate as the best, the safest, and most sure—the *postural method*.

In 1784, Professor Alexander Hamilton, of Edinburgh, says, "the best practice, therefore, is to take the earliest opportunity that the circumstances of the case will admit of, to reduce the cord *by placing the woman in a proper position*, so that the hand of the operator may be carried up, *in the absence of pain*, into the pelvis, and the cord entirely reduced."

Unfortunately, there is no mention made by him of the

*proper position* ; and so we are left in the dark as to his exact meaning and mode of procedure.

Next, Drs. Hardy and M'Clintock approach the true postural method very nearly. They advise, that in attempting manual reduction, the woman should lie upon the side opposite to that on which the cord protrudes—*i.e.*, if the cord be felt on the right side of the pelvis, the woman should lie on her left side, and *vice versa*. By this means a certain amount of backwards and downwards (with reference to the mother's position in bed) inclination is given to the cord, and it may slip up and be retained.

If the cord slip out anteriorly, there is little or no chance of its being retained *in situ* when the mother is placed on her back. But take the opposite position to this ; let the cord slip out posteriorly, reverse the mother's position—let her be placed on her knees with her head low down on a pillow, "after the manner of eastern worshippers," and the difficulty can easily be overcome.

It is somewhat astonishing how near one may approach the desired end and yet not reach it. Not until Dr. Thomas, of New York, read his paper on postural treatment of prolapsed funis has the desired end been achieved (1858).

Dr. Thomas thus describes the history of the operation, and the manner in which it is performed:—"In a course of lectures," he says, "on obstetrics, delivered by me in the University Medical College of this city, about two years ago, I closely investigated this subject, and came to the following conclusions:—*First*, That the causes of the persistence of this accident (whatever may at first have produced it) reduced themselves to two—the slippery nature of the displaced part, and the inclined plane offered it by the uterus, by which to roll out of its cavity ; and, *Second*, That the only rational mode of treatment would be inverting this plane, and thus turning to our advantage not only it, but the lubricity of the cord, which ordinarily constitutes the main barrier to our success. This, I found could be readily accomplished by *placing the woman on her knees, with the head down upon the bed*, in the posture assumed by eastern nations in worship, and now often resorted to in surgical

operations upon the uterus and vagina. Let it be remembered that the axis of the uterus is a line running from the umbilicus, or a little above it, to the coccyx ; and it will be seen that, by placing the woman in this position, it will be entirely inverted.”\*

Advantage here is taken of the (a) Slippery nature of the cord ; (b) The action of gravitation ; and (c) The peculiar way in which the lower segment of the uterus contracts when a pain is coming on—viz., by commencing the contraction in the neck, and gradually passing upwards, so that when the cord is pushed up by the hand just as contraction is coming on, the very action of the cervix itself, so to speak, sucks back the cord that had been prolapsed, and by the closeness of its contraction prevents the cord from falling down again between the presenting part of the child and the passages.

Before appending cases treated by the “postural method,” I may mention that in some cases where the knee-elbow position cannot be had recourse to, we can substitute a modification of Drs. Hardy and M’Clintock’s process. Taking advantage of their suggestion—viz., of placing the mother on the side opposite to that on which the funis has prolapsed—we can add greatly by elevating the mother’s pelvis by putting pillows under her, and lowering her head and shoulders. It is quite obvious that, in the case of a stout, thick-set, apoplectic-looking woman, the postural method might be fraught with some danger.

I now subjoin some cases :—

CASE I.—On July 12th, 1868, at half-past 11 P.M. I was called to attend Mrs. K., in her fourth confinement. Labour had set in slowly about midday, and gradually increased in severity. On my arrival the pains were active ; the vertex was presenting ; no liquor amnii had escaped. In a quarter of an hour a male child was born naturally, and separated in the usual way. Having noticed that the mother’s abdomen

---

\* “Transactions of the New York Academy of Medicine,” vol. ii. part 2 p. 21.



was very large, and that no liquor amnii had escaped with the child, I suspected twins ; and, on examination, detected a second child, with the head presenting, the membranes entire. The os uteri being well dilated, I ruptured the membranes, when a gush of liquor amnii took place. On examination, I now found a large loop of a second funis prolapsed. As it was necessarily under severe pressure during each pain, I resolved to try reduction of the cord by the *postural method*. Placing the mother on her knees, with her face resting on a pillow, I passed my hand into the vagina, pushed up the prolapsed loop of cord, and waited till the uterus contracted. I then had the satisfaction of finding that the cord did not come down again ; and *in a few minutes* a live female child was born. I may mention that I reduced the cord once or twice before trying the postural method, but it prolapsed with each pain.

CASE II.—On October 26th, 1869, I was called to attend Mrs. H., aged twenty-seven, primipara. She had been ill ten hours. Pains active ; occurring every few minutes. On examination, I found the os uteri dilated to the size of a crown piece, the head presenting, a bag of membranes protruding, and in this bag was a loop of funis. Finding no pulsation in the cord, I thought it proper to at once reduce it, in the hope of saving the child. Placing the patient in the knee-elbow posture, with little difficulty I partly introduced my left hand into the vagina, ruptured the membranes, and, just as a pain was coming on, pushed up the loop of cord (six inches in length) alongside of the head, and, as the uterus contracted and the head was forced down, the cord slipped out of reach, and was not again prolapsed. I did not keep the mother two minutes on her knees. She had a tedious labour, with a narrow pelvic outlet, and was delivered by forceps some hours after. The child was dead. There was extensive disease of the placenta—calcareous and fibrous. The mother said she had not felt fetal movements for some days. The mother made a good recovery.

CASE III.—On the 19th December, 1869, I was called to

attend Mrs. F., aged twenty-three, a primipara. When I arrived, I found the pains active, vertex presenting above brim, with a loop of cord prolapsed ; waters escaped about half an hour ; os uteri size of fiveshillling-piece. The loop of cord was in the vagina, pulsating. I applied the "postural method," reduced the cord in the interval of pain, kept the mother in position till *two pains had occurred after reduction*, and then allowed her to take the usual obstetric position ; no further prolapse took place. The labour terminated naturally in about three hours. The operation for prolapse occupied about ten minutes. Child female, full time, alive and well.

CASE IV.—January 12th, 1870. Mrs. E. ; second child ; narrow pelvic brim ; liquor amnii escaped some time ; os uteri well dilated ; head above brim ; vertex presentation ; large loop of cord in vagina ; pulseless, but not cold ; postural method applied *as above* ; no further prolapse ; child born twelve hours after, dead, male.

*Remarks.*—In this case I resolved to give the child a chance, though the cord was pulseless. At her former delivery she required the long forceps.

CASE V.—1st November, 1870. Mrs. H. ; fifth child ; labour active ; liquor amnii escaped just as I entered the house ; vertex presentation above brim ; large loop of cord down and out of vagina ; pulsating ; postural method ; found some difficulty in reducing such a large loop, but by opening my fingers and sweeping the cord entangled in them round the head, I pushed it up just as a pain was coming on. I retained my fingers within the os till the pain had gone, and then I found that the cord had slipped back. No further prolapse. Patient in *five minutes* allowed to take the usual obstetric position. In half an hour the case terminated naturally ; child, female, alive.

*Remark.*—In this case I think the retention of the cord in utero was helped by the irritation given to the os uteri by my fingers.

Cases VI. VII. VIII. IX. X. obtained from my friend

Dr. Wilson, Professor of Obstetrics in Anderson's University, Glasgow, and the *Glasgow Medical Journal*.

CASE VI.—July, 1864, Mrs. —, aged thirty-three, multipara. Had been in labour three hours before my visit. On examination *per vaginam*, the os uteri was found dilated to about the size of a florin, and was somewhat relaxed and dilatable. The head was felt to present through the unruptured membranes. In about an hour after my arrival the liquor amnii was discharged. Upon now instituting a second vaginal examination, I ascertained that a loop of the cord had descended by the side of the head. The umbilical vessels were pulsating vigorously. I at once attempted replacement of the cord by the fingers, and succeeded without much difficulty in pushing the cord up alongside the presenting head, beyond the influence of pressure ; but only a few minutes elapsed when it was again in the vagina as before. Other two attempts were made, but with a similar result. Having no instrument at hand, and being unwilling to subject the patient to the dangers attendant upon podalic version, I forthwith resolved to test the merits of the postural treatment. The patient having been placed upon her knees and elbows, *during the interval of pain*, I carried the displaced loop of cord cautiously down through the os uteri, when it glided away beyond the presenting head into the cavity of the uterus. Abdominal frictions and ergot were now had recourse to, with the desirable effect of increasing the uterine action. The patient was kept in the position above mentioned for *about half an hour*, at the expiry of which period, the head having fairly entered the pelvis, she was allowed to assume the usual obstetric position. There was fortunately no recurrence of the prolapse. In about three-quarters of an hour afterwards the child was born alive and vigorous.

CASE VII.—June, 1865. The subject of this case was Mrs. —, aged twenty-nine years, a pluripara. Had been in labour for upwards of two hours before my visiting her. The os uteri, on examination, was found unusually high



up, and dilated to about the size of half-a-crown. The head was found to present, and by its side a coil of the cord about four inches long was prolapsed, and distinctly pulsating. The liquor amnii had escaped just as I entered the house. I attempted with the fingers several times to return the cord, but it was no sooner replaced *in utero* than it was forced down again by each recurring uterine contraction. I had now recourse to the postural treatment. The patient having been placed upon her elbows and knees, I gently pushed the cord through the os uteri during the absence of a pain. It appeared at once to slip away past the presenting head into the uterine cavity, quite beyond the reach of the fingers. Ergot and friction over the abdomen were now resorted to, with the view of stimulating the uterus to increased activity. As soon as the head came to occupy the pelvic cavity, which it did in about *twenty minutes*, the patient, as in the previous case, was permitted to resume the left lateral position. Labour terminated in little more than an hour afterwards without any subsequent descent of the funis. The child was born living and vigorous.

CASE VIII.—Mrs. C., pluripara ; fifth child, 27th December, 1866 ; in labour seven hours ; membrane ruptured ; loop of funis protruding externally ; postural method ; child alive ; mother kept on hands and knees *one hour and a half*.

CASE IX.—Extract from Glasgow Maternity Hospital quarterly report, from March 15th to June 15th, 1869. "There was one case of funis presentation in which the postural method was adopted successfully."

CASE X.—Extract from report, May 15th, 1870, to August 15th. "Prolapse of funis occurred in three cases, two of which were outdoor patients. When medical assistance arrived, on examination the funis in both instances was found external cold and pulseless, and the labour was allowed to go on without interference.

"The other case occurred in the hospital ; it was detected

at an early stage of the labour, and successfully treated by the postural method."

This then is the postural method, and the above cases are illustrations of its successful application. The five cases detailed from my own practice are the only cases of prolapse I have had for a year or two. The experience is not very large, but the ease with which I accomplished the return of the cord, the successful retention of it *in situ*, and the dangerless character of the operation to mother and child, commend to you this method.

It is a method in accordance with common sense ; it is simple, and the instrument—the hand—is always ready. It might be called the "ready method."

In Dr. Wilson's cases you will find that the patient was kept a long time in the knee-elbow position ; in my cases ten minutes was the longest time required. And I think it quite sufficient, after the cord has been reduced, if the uterus contracts well, to keep the patient in position till one or two pains have occurred. In the last case I attended, I could feel the cord slip away up and out of reach as the uterus contracted, and it is my opinion that irritation of the os uteri by rotating the fingers within the os, is of great service in inducing active contraction of the lower segment of the uterus, and thereby preventing further prolapse.

I have only a small experience in cases of prolapse where the breech or feet have presented. The cases are two in number. In one with very narrow pelvic brim, head, arm, and funis presented. It was necessary to perform craniotomy ; so I did not trouble about reducing the cord. It was pulseless. The second case was a footling, with cord pulsating, wide pelvis, soft parts relaxed and dilatable. Delivery by extraction was easily and rapidly accomplished, and the child saved. In such the early pressure is but slight, and any good (if any) obtained by early reduction, most likely would be balanced by the after danger to which the cord would be subjected in delivering the shoulders and head.

Of the ten cases narrated, in all the cord was successfully reduced ; in eight the child was born alive. Of the two cases

wherein the children were dead, I think both may safely be excluded, for in one the cord was pulseless before reduction, and the labour long and tedious. In the other there was sufficient placental disease, coupled with the difficulty of delivery, to account for the death of the child.

So, as far as experience goes, the mortality is *nil* by the postural method, and I have no doubt but that, were this method of treatment generally adopted, we should have results to show which would astonish those accustomed to the other methods.

One other item I would add. It is very probable that the knee-elbow position may be of great service in cases where pressure of the uterus upon the kidneys or renal vessels has produced uremic poisoning, albuminuria and convulsions. By relieving the pressure, the convulsions may be avoided, or, if existing, terminated. I have, as yet, had no experience of this treatment, but merely throw it out as a suggestion.

## NOTES OF ONE HUNDRED CASES OF ABNORMAL LABOUR,

Under the care of W. Y. MARTIN, L.R.C.P. Edin., L.M. Lond. &c. ;  
and F. H. Y. GROSHOLZ, L.K.Q.C.P. Irel., L.M. Dub. &c.

(Reported by F. H. Y. GROSHOLZ.)

UNDER the belief that the accumulation of statistical data of obstetrical cases, and more especially of such as do not terminate naturally, is of some service in advancing the science of midwifery, we have ventured to publish the records of 100 cases of abnormal labour which have been under our care, at the same time giving a short sketch of the modes of treatment which we have found most successful.

Of these—56 were forceps, 42 turning, and 2 craniotomy cases.

### FORCEPS CASES.

Of the 56 women delivered by the forceps, 26 were *primiparæ* and 30 *pluriparæ*, the youngest being seventeen,

and the oldest forty-two years of age. No mothers were lost.

Of the children, 38 were males and 18 females. Three children were dead, one of these being putrid.

The statistics given by Dr. Churchill show, that in forceps cases on an average, 1 mother in 33 is lost, and 1 child in  $4\frac{3}{4}$ . In our cases all the mothers were saved; 1 child in  $18\frac{2}{3}$  was born dead; but, omitting the one that was putrid, only 1 in  $27\frac{1}{2}$  died as the result of the operation.

The long forceps were used thirty-five, and the short forceps twenty-one times.

The causes needing their employment may be thus classified:—

Contracted pelvis . . . . .	23
Uterine inertia . . . . .	21
Malposition of head . . . . .	4
Convulsions . . . . .	3
Hand and head . . . . .	2
Face presentation . . . . .	1
Extraction of head in breech case . . .	1
Hemorrhage . . . . .	1
<hr/>	
Total	56
<hr/>	

#### TURNING CASES.

In 37 *pluriparæ* and 5 *primiparæ* turning was necessary. The ages of the mothers varied from twenty to forty-five years. The mothers all survived the operation.

Thirty male and 12 female children were thus delivered. Of these 18 were dead, 8 however being putrid. The statistics of the author above quoted show that turning was fatal to 1 mother in 15, and to 1 child in  $2\frac{1}{2}$ . In all our cases the result was favourable to the mother.

One child in  $2\frac{1}{3}$  was lost; but omitting the eight cases in which the children were putrid, the mortality is reduced to 1 in  $3\frac{2}{5}$ .

And even among these we have five children whose lives had already been lost through hemorrhage produced by *placenta prævia*.



One of the children weighed 13 lbs. 2 oz.

The conditions present necessitating turning may be thus arranged :—

Contraction of pelvis . . . . .	9
Shoulder presentation . . . . .	9
<i>Placenta prævia</i> . . . . .	5
Hand and arm . . . . .	4
Hand and cord . . . . .	3
Head and hand . . . . .	3
Hemorrhage . . . . .	3
Face presentation . . . . .	1
Face and hand . . . . .	1
Brow, hand, and funis . . . . .	1
Back . . . . .	1
Convulsions . . . . .	1
Uterine inertia . . . . .	1

---

Total 42

---

#### CRANIOTOMY CASES.

The women in both these cases were *pluriparæ*, their ages were thirty-eight and forty years respectively.

In each case there was an extreme degree of contraction of the pelvis.

In one case after perforation, Dr. Barnes's cephalotribe was applied with most satisfactory results. Both the mothers recovered. The children were males.

*Remarks.*—The successful results which we have had the good fortune to obtain in our forceps cases are, we think, attributable to the fact that we do not delay their use too long, but apply them before the system of the mother has become so exhausted as to be unable to withstand the additional shock of the operation. The cases which have given us most anxiety have all been those in which on our arrival we found the woman already prostrate.

After having applied the forceps and before commencing traction, we, as a rule, administer a dose of ergot ; and both

immediately before and for some time after the delivery of the child, use constant pressure over the uterus.

In all cases of turning, where practicable, we employ the bipolar method, and are often able to perform version without introducing more than two fingers into the vagina.

We usually bring down only one foot, as we find that a greater degree of dilatation is produced by the trunk passing in this position, than when both feet are brought down; and, consequently, that the delivery of the head is more easily accomplished.

Though the mortality of the children may seem rather high, still, among other things, we think this is to a considerable degree attributable to the fact, that many cases in which we have felt justified in turning, are such as might have been considered by some practitioners as more suitable for craniotomy.

We have been much struck with the success which has frequently attended persevering efforts to resuscitate the still-born child, and have had the pleasure, in more than one instance, of seeing a child, which for many minutes had to all appearance been beyond the reach of human aid, gradually show signs of returning vitality, and eventually become completely restored to life.

Impressed by such occurrences we cannot but think that the bestowal of a little more care and pains in attempting to restore the animation of apparently stillborn children, would be rewarded by a material decrease in infantile mortality.

---

### General Correspondence.

---

#### "STRANGE OBSTETRIC PRACTICE."

*To the Editor of the Obstetrical Journal.*

SIR,—Your contemporary the *Lancet*, January 9th, 1875, publishes, under the above heading, the particulars of a case of M. Blondeau's, which he brought before the Therapeutic Society of Paris in November, 1874, which may be epitomized

as follows :—The patient in question had, in her previous confinements, suffered severely from hemorrhage. When in the sixth month of her last pregnancy she had a violent attack of epistaxis, which was so severe in its effects as to necessitate transfusion. After this the fetus was extruded, but no attempt was made to remove the placenta *for fear of hemorrhage*, and it was left in utero for a *week*, during which period the patient progressed favourably. At the end of a week, the placenta came away spontaneously ; immediately after its expulsion shivering, fever, and tympanitis supervened, and the unhappy patient died, as stated by M. Blondeau, of puerperal septicemia.

My present object is not to criticise this case, but to place in juxtaposition with it another having many points in common—one in which the treatment adopted differed as much as the result.

In September, 1874, I received a message from a medical man, residing near Guildford, to the effect that he had a lady under his care who had miscarried about the sixth month, a week previously, from whom the placenta had not been removed. She had at the time of the miscarriage alarming hemorrhage, and it was the fear of increasing this evil that had induced the doctor to allow the placenta to remain in utero ; but experience clearly demonstrated that this course was not infallible, inasmuch as hemorrhage had frequently recurred during the week, which had not unfrequently proved rather unmanageable and troublesome, and on the eighth day it recurred with such fury as to threaten the immediate destruction of the patient. When I first saw her (a lady in her thirty-fourth year, this being her fifth pregnancy) she was cold, blanched, and greatly prostrated by the frequently repeated hemorrhage which had taken place during the past week, as well as by the persistent vomiting (of a deep green fluid, having a deeper green sediment), which had been continuous during the last three days, in spite of every form of treatment, the stomach being too irritable to retain the smallest quantity of fluid nourishment, stimulants, or even ice.

On examination, the bed contained a considerable number of

napkins, towels, &c., all more or less completely saturated with blood. A large clot occupied the vagina, the os uteri was high up and rather small, having a piece of the placenta protruding into the vagina. The right hand was placed on the abdomen (which was very tender) to steady the uterus, which occupied a central position, extending from the pubis to the umbilicus, and the left hand was insinuated behind the protruding placenta into the uterus. As soon as the fingers reached the fundus uteri free contraction was at once induced. This was followed, in less than five minutes, by the sudden expulsion of a large quantity of very offensive blood clot, as well as the placenta (in my hand). On expulsion, the placenta, &c., were so exceedingly offensive that I could with difficulty refrain from vomiting. While the patient remained in the obstetric position, I passed a long tube up to the fundus uteri, and washed out the cavity with Condyl's fluid in warm water. The whole operation, including the injection of Condyl's fluid, occupied about a quarter of an hour. From this moment there was no recurrence, either of the vomiting or the hemorrhage. For the first two days she progressed most favourably. On the third day, however, the temperature rose to  $104^{\circ}$ , and the pulse to 140. The abdomen became tympanitic and very painful. Thirst was urgent, but there was neither appetite nor vomiting. On the fourth day diarrhea suddenly set in, which continued with considerable severity for forty-eight hours, when it quietly subsided. From this time the temperature and the pulse began to fall, and the patient recovered rapidly and perfectly without any recurrence of bad symptoms. The treatment during the diarrhea was simple, it (the diarrhea) being regarded as eliminative. Turpentine and opium in equal parts were applied externally, dropped on dry flannel; an occasional dose of bismuth and opium, and a nourishing fluid diet, with an occasional stimulant, were the only means adopted.

Dr. A. W. Edis, in his comments on M. Blondeau's case in the *Lancet*, January 16th, gives it as his opinion that M. Blondeau "was fully justified in waiting and watching. Fever and tympanitis set in immediately after the spontaneous expulsion of the placenta, not apparently due to its long re-



tention, but to the admission of air into the enclosed uterine cavity." Without offering any comment on this opinion, I may simply state that an experience gathered from the careful observation of more than four thousand cases of labour, embracing almost every form of complication, leads me to the expression of an opinion diametrically opposed to that of Dr. Edis. Is it not reasonable to suppose that the patient, having lost so large a quantity of blood from epistaxis as to necessitate transfusion, was pre-eminently predisposed to the absorption of poisonous products such as would be present in great abundance both in the uterus and vagina? I fully concur with M. Blondeau in the opinion that the cause of death was "puerperal septicemia."

Apologizing for trespassing so largely on your valuable space,  
I am, yours, &c.,

THOMAS CHAMBERS.

Bolton Row, Mayfair, March, 1875.

---

## THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

APRIL, 1875.

---

### THE CASE OF MR. PEACOCK.

It is unfortunate that we should have to commence the duties of our third year by commenting upon such a distressing case as that which has been recently investigated at the Warwick Assizes. Being altogether so little to the credit of obstetric practice, it would have been more in consonance with our feelings to have looked upon it as so exceptional as to render it capable of being passed over in silence. There are, however, connected with it so many points of practical interest that such a course becomes impossible. It is unnecessary to recapitulate the principal features of the case,

for they have already appeared in this Journal. It may be simply stated in addition, that on March 2nd Mr. Peacock was indicted for having at the parish of Mancetter, on the 19th of June last, feloniously killed and slain Ann Woodward; and the case was tried before Lord Chief Justice Coleridge, who, after a trial extending over two days, sentenced the prisoner to confinement in gaol for six months without hard labour. The Obstetric evidence which we propose now to examine was very freely, and in some cases, we think, harshly criticised by Judge and Counsel. In considering it, however, we shall endeavour to form an opinion unbiassed by their comments. Although the records of most large lying-in hospitals contain cases of vaginal rupture, it is generally admitted to be a very rare accident. What caused it in this case there was no evidence to decide. It may be produced spontaneously and unavoidably, or carelessly during Obstetric operations. From the excellent character, both moral and professional, which Mr. Peacock received, there appeared to be no grounds for believing that the laceration which ended so disastrously was caused by any want of care or skill. Having occurred, hernia of the bowels was an almost necessary sequence. So common, indeed, is this complication that Obstetric writers constantly impress on their readers the care which is requisite to prevent them falling into fatal error. "Beware of grasping the intestines and pulling them down along with the feet," says Blundell, writing of the way in which turning should be effected after rupture. And again, in speaking of the manner in which the third stage should be managed after the same accident, "Great care must be taken that you do not draw down any other parts together with the after-birth, and more especially the intestines." As to the amount of intestines which may be protruded through a rent in the vagina, we have no satisfactory records. It undoubtedly varies very much, and depends chiefly upon the length and strength of the intestinal attachments, the size and position of the laceration, and the amount of abdominal compression which forces them through the aperture. Supposing the patient to sit or stand for a moment, gravitation would of course also have a power-

ful prolapsing influence. In the present case it appears that intestines to the extraordinary length of fifteen feet found their way in some manner outside the vulva, and one of the main questions which the Obstetric experts had to decide was, whether this large amount of abdominal viscera could be forced through the vaginal rent by natural efforts. The answers to this question were unfortunately very much at variance, the medical gentlemen for the prosecution expressing their belief that such a length of intestine could not be spontaneously protruded, and those for the defence stating their conviction that such an accident might have occurred without manual interference. These opinions given, we believe, in perfect good faith, prove at least that our experience upon this point is at present very limited, nor can it be hoped that our knowledge in this direction will be soon extended unless it be by carefully conducted post-mortem experiments, which might to a certain degree elucidate the subject. The next point to be decided was, what ought to be done with intestines found protruding through a rent in the vagina? Again, the answers to this question were not unanimous, the gentlemen for the prosecution urging that it would be most proper to endeavour to return and retain them in their normal position, whilst those for the defence stated, in a more or less emphatic way, their belief that a patient's comfort would be best insured and her life prolonged by removing the portion of the bowels which had escaped. Although this latter opinion was uttered by gentlemen of the highest professional ability, we cannot help regretting its publication. It is impossible to us to conceive a case in which such an operation would be justifiable. We cordially endorse the opinion of the most eminent expert present at the trial, who pronounced its performance by the defendant to be a "rash act," and we sincerely hope that none of our younger brethren, on the strength of recommendations given at a trial, will be induced, should they ever have the misfortune to meet with a case of vaginal rupture and intestinal protrusion, to adopt a practice which we believe would be equally disastrous to themselves and their patients. The success of the returning and retaining plan of treatment

has been so satisfactory that no other method need be sought. Collins adopted it in three cases, two of which recovered entirely, and one died of hemorrhage twenty-five days after. It is scarcely a question whether it would not be better to leave the bowels protruding than to cut them off, for by this mode of dealing with them death is not inevitable. Dr. McKeever tells us of a young woman from whose vagina, after labour, nearly four feet of intestines protruded. The sixth day after the accident they sloughed off. For two years the patient voided all feces per vaginam, after which they took their natural course, and she was again safely delivered of a child.

## Abstracts of Societies' Proceedings.

### OBSTETRICAL SOCIETY OF LONDON.

*Meeting, March 3rd, 1875.*

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., *President, in the Chair.*

The following gentlemen were elected Fellows of the Society:—R. J. Maitland Coffin, M.R.C.S. Edin. (Malta), Charles J. Cullingworth, M.R.C.S. (Manchester), John H. Ewart, L.R.C.P. (Manchester), P. Brown Giles, Jun., L.R.C.P. Edin. (Hereford), Augustus Jukes, M.B. (St. Catherine's, Ontario), Alfred E. Austin Lawrence, M.D. (Bristol), Charles Liebman, M.D. (Trieste), Eugenio Rey, M.D. (Turin), Fred. W. Salzman, M.R.C.S. (Brighton), David E. Seton, M.D. (South Kensington), Frederick William Strange (Aurora, Ontario).

### *Clinical Notes on the Early Course of Cancer of the Cervix Uteri.*

Dr. CHARLES LIEBMAN read a paper on this subject. After quoting the opinions of many eminent authors, he narrated the particulars of four cases observed by himself, in which he showed that the disease generally spreads much higher in the lining membrane of the cervical cavity than on the exterior part of the neck. Amputation of the neck he considered useless in all cases of cancer, excepting those of pedunculated canceroid papillary tumours of the cervix.

Dr. WILTSHIRE thought the "beginnings of cancer" of singular importance to the profession as a matter of clinical as well as of patho-



logical interest. It involves an important practical point, for it is obvious that if we can find out the beginning of the disease, and are thus enabled to treat early the implicated tissues, we may at least have the satisfaction of arresting the disease, and that sometimes for a considerable period. Dr. Wiltshire mentioned two cases in illustration of his remarks. In both considerable benefit followed removal of such portions of the diseased tissues as could readily be attained. He had sometimes used chromic acid with considerable success.

Dr. HEYWOOD SMITH remarked on the far greater frequency of cancer in the lips of the uterus than the fundus or body. He had tried Dr. Gibson's method of treatment with plugs of strong perchloride of iron and in a large proportion had seen with some arrest of discharge grave symptoms supervene, symptoms as of septicemia. He laid stress on the rule that Dr. Barnes had insisted on—viz., that in all cases, when practicable, early removal should be had recourse to.

The PRESIDENT remarked that Dr. Liebman had drawn attention to an interesting and pathological subject. The object of his paper was to point out that authorities generally regarded the vaginal portion of the cervix as the seat of the beginnings of cancer, whatever its form might be, but that he had observed cases in which the disease commenced in the cervical cavity, and he had been able to demonstrate the development of masses of epithelioma in that locality, while the external *labiæ* were entirely unaffected. The author, however, did not seem to be conversant with the researches of Virchow and other pathologists, who had observed cancer originating both in the mucous membrane of the cervix and body of the womb, and had demonstrated the development of canceroid in the uterine walls themselves. Cancer beginning first at the fundus uteri was not uncommon. Sir James Simpson had written a paper on the subject, and he believed instances had been published by Dr. Barnes and others. He had in recollection more than one case where he had watched the progress of malignant disease in the body and fundus of the uterus, and where ulceration had made its way into the peritoneal cavity, with the cervix to all appearances scarcely at all involved. The difficulty of diagnosis in cases of cancer of the interior of the uterus was no doubt very great, and he knew no way of distinguishing a malignant growth situated in the fundus from the enlargement of a benign character, except by observing the nature of the discharges, the comparatively rapid growth, and the persistent pain associated with the former.

In the treatment of epithelioma or other malignant disease of the cervix, he believed there was a general concurrence of opinion that when the tumour had a margin of healthy tissue above, it was desirable to amputate the cervix, but he had not yet made up his mind whether in worse cases—viz., when the disease had advanced above the line of demarcation between the cervix and body of the uterus, and it was not possible to remove the whole growth, whether it was desirable by operative procedure to remove a considerable portion by

way of staying the progress and lessening the discharges. He thought this might be justifiable where the discharges and loss of blood were great and rapidly reducing the patient, but where the symptoms were less urgent his experience rather indicated abstinence from interference, as any but palliative measures had seemed to give an impetus to progress and to lead to more rapid development of the disease.

Dr. ROGERS thought the paper was useful to the Society if only to elicit the opinion of the Fellows. He considered it impossible to cure cancer by means of escharotics, though of these bromine was the most useful. If the tissue be scraped first, and destroyed effectually by means of bromine, it often produced an amount of benefit that warranted our doing it, but it required great care, as it was a powerful escharotic.

Dr. BARNES not having heard the paper would limit his observations to one question of treatment referred to by the President. After the experience of quite a considerable number of operations, he had arrived at the conclusion that the more effective as well as the most safe mode of dealing with cancer of the cervix uteri was by the galvanic cautery. If the diseased mass was projected into the vagina in such a manner as to permit of being surrounded by a wire, it should be removed flush with the vaginal roof. There was rarely any serious bleeding, sometimes almost none; now and then if a small artery spouted, the use of the porcelain cautery of the battery had effectually stopped it. In those cases in which the disorder did not form a projecting mass, the button galvanic cautery could be moved over the surface, destroying the necrosed portion. Comparatively healthy granulations commonly followed. The so-called cachexia disappeared for a time; there was almost always freedom from hemorrhage for a time, and the general condition greatly improved. He had only known one casualty from the proceeding: a woman in St. Thomas's Hospital died a few days after operation from peritonitis. In all the other cases relieve and benefit was experienced. In one case the subject had two pregnancies, and she was alive five years after operation, although the disease returned.

Dr. WYNN WILLIAMS remarked that the cancerous matter may be deposited in any of the tissues of the uterus, as in other parts of the body, also that different forms of cancer begin more frequently in one structure than in the other: thus the epithelial form commences in the skin and mucous membranes, whereas the medullary commences in the deeper-seated parts. So it is with the uterus; we may have cancerous deposit in any part of the uterine organs. He had seen in consultation three cases within the last twelve months where he diagnosed cancer of the fundus. He did not think that any one could well mistake the peculiar odour of a cancerous mass in its latter stages for the odour of a disintegrating fibroid. In cancer of the fundus the greatest care should be taken in passing the sound, as a

very eminent physician informed him on one occasion whilst examining the uterus he was unfortunate enough to pass the sound through the cancerous tissues into the peritoneum, the patient dying in a few hours of peritonitis. There can be no doubt that the cancerous mass wherever situated should be removed when practicable, either by the *écraseur*, or by the galvanic cautery as mentioned by Dr. Barnes ; and even if the whole of the mass be not removed, if the uterus be mobile, and the surrounding tissues not infiltrated, we should endeavour to destroy the remaining portion by injecting bromine into it or applying it by means of cotton wool. This he had done in several instances where the neck of the uterus had been removed and the actual cautery applied by other medical men, with the result apparently of removing the whole of the disease, for after a lapse of six or seven years the disease had not reappeared.

Dr. ROUTH said the Society might recollect his bringing the subject of uterine cancer forward several years ago, and subsequent experience only confirmed the opinions then expressed, indeed one of those women, a case then believed to be hopeless, was, he had heard, still alive and in perfect health. He did not believe cancer of the fundus was quite so rare as had been stated—occasionally they were overlooked and mistaken for fibroids ; indeed it was quite impossible in many cases to make a diagnosis, because the bloody or fetid discharges were common to cancer and disorganizing fibroids, or might occur independently of these ; but if there was marked pain at night, which persisted for any length of time, with menorrhagia, it was safe to put down the case as one of cancer. Perhaps some very rare forms of neuralgic uterus with menorrhagia might puzzle us occasionally, but the previous history generally cleared up the case. When the cancer affected the cervix exclusively, and always in cases of papillary or epitheliomatous cancer, it was the right thing to remove it at once. He had done so in several cases and many had had no recurrence. He usually employed the ordinary wire *écraseur* in preference to the electric cautery, as it was much more easy of application, especially if the tumour was large or irregular, because the electric wire was so thin ; then again the electric wire often burnt a good way around beyond itself, and if the cervix had to be taken away high up, there was fear of injuring the attachments of the vagina and uterus and so getting into Douglas's space from the after sloughing, and producing death by peritonitis. He was not satisfied with leaving the case to heal at will after such incision. He waited four or five days and then destroyed the surface again by means of bromine or the actual cautery, allowing the destruction to extend to part of the lining membrane of the uterus if he had reason to fear it was diseased at all. And then he often expedited the healing process, especially by a solution of gastric juice, which had a marked effect in bringing about cicatrization. This he had proved often, not only in cancerous sores, but in others which turned out very obstinate and difficult of cure by other means. The



marked difference between it and the perchloride of iron in healing power could be seen often if we dressed a wound half with the iron and half with gastric juice, and he was glad to find that Dr. Barnes confirmed the healing powers of this agent. A relapse would not justify cessation from interference. A second operation often proved effective where the first had failed. Lastly, in cases of hopeless cancer accompanied by fetor and loss of blood, and when a patient was dying a miserable death, he thought we should interfere. Destruction of the ulcerating surface often both stopped the general cachexia and gave great relief, and the patient gained temporary restoration of health. Where the hemorrhage proceeded from the cavity he applied the persulphate solution of iron or the perchloride directly to the cavity on lint, and more recently by means of an instrument similar to that devised by Simpson for passing caustic into the womb—though larger—he introduced into the cavity, either after dilatation by sea-tangle or without, if the opening was sufficiently patent, the dry solid perchloride, with the best results. Sometimes the arrest of hemorrhage was instantaneous and seldom gave rise to any trouble, once only he had seen some metritis which supervened and lasted three or four days. In any case he thought it was perfectly unjustifiable to leave even hopeless cases of uterine cancer to die a miserable death and be a pest to themselves and others, in our present state of knowledge, when so much temporary relief could be given.

Dr. AVELING stated that after scooping or removing by any other means the cancerous structures, he had found nitric acid as efficacious as and more easily applied than the actual cautery.

Dr. WILTSHIRE said he had used chromic acid with great benefit, it acted as a deodorant as well.

Dr. BLOXAM said that all present would agree that it was of the utmost importance in those cases which were beyond the reach of operation, to mitigate the suffering caused to patients and friends by the extremely fetid discharges. He had heard bromine named, but not iodine or iodoform. The last was both a deodorant and an anodyne, and deserved more notice than it had yet received in this country. It could be applied in powder or wool, as a pessary with cocoa-butter, or in solution in glycerine.

Dr. EDIS remarked that the object of Dr. Lieberman's paper was to show that in the majority of cases of cancer of the uterus the disease commenced in the lining membrane of the cervix and not primarily in the lower portion of the cervix itself. He (Dr. Edis) had long recognised the fact, and in place of amputating the cervix by means of the *écraseur* or galvano-caustic, he had been in the habit of employing the actual cautery in the form of a pear-shaped bulb, to destroy the lining membrane and interior of the cervix, thus producing a conical cavity, the base representing the os uteri, which ultimately contracted, leaving merely a small aperture. In several instances this had been attended by marked success, the patient living for many



years after the date of operation without any apparent return of the disease. The chief points enabling us to arrive at a correct diagnosis as regards the fact of its being cancer were the almost undilatable condition of the cervix, the extreme tendency to bleed on the slightest touch, the fixidity of the cervical mucous membrane, and the rapidity with which the disease extended.

Dr. J. BRAXTON HICKS said, with regard to the deep burning of the galvanic wire mentioned by Dr. Routh, this was obviated by making the battery more powerful, and screwing up the wire more quickly. The galvanic wire had one great advantage over the ordinary *écraseur*, it cut quite clean and knife-like, and was adaptable to the more sessile growths ; whereas the *écraseur* drew in and puckered up such forms, and brought away the softer portions, sliding off the harder, which it left behind. This he had seen frequently. With regard to the practice advocated by most of the speakers—viz., removing those parts which we could, and destroying the rest by cautery, caustics, or styptics or other plan, he coincided ; indeed, these he had advocated in papers elsewhere and at this Society. It afforded much benefit and comfort to the patient, removing the cachexia very effectually till the return of the disease. This he had shown in a paper in the *Guy's Reports* some years since, and concluded that the so-called malignant cachexia was owing, not to the malignant diathesis, but to the absorption of the material from the malignant growth prior to the existence of sloughing ; and thought after breaking up occurred it was increased. Regarding the place of origin of malignant disease, he considered as the result of observation that it could spring from any tissue in the uterus and then spread to other tissues ; that each fresh tissue affected would take on the action more according to its own nature than of that to which it was influenced, for instance, that epithelioma would not communicate epithelioma to fibrous or connective tissue, but supposing epithelioma was first affected, and the malignant action extending to fibrous tissue, then rather that schirrous was formed, and so in connective tissue we get the infiltrating kind like that which occurs in the connective tissue in the breast. However, this opened up the question as to the nature of malignant disease at large, which was too wide a subject for the present time, but he would say that he had seen on the confines of malignant disease of the cervix removed by amputation under the microscope, the mode of extension taking place in the nuclei of the fibre cell, enlarging, then dividing inside, the old cell bursting out its centre. In like manner, as he had pointed out in the *Guy's Reports*, and again in the *Pathological Transactions*, he had seen the nucleus in the wall of the blood-vessels enlarge and then divide into two or three cells bulging into the canal of the blood-vessel, giving it a head like appearance, but ultimately going on so far as to obstruct the current completely, and this would serve to explain why it was that sloughing so readily occurred in some of these growths.

Dr. MURRAY quite agreed with the President as to the difficulty of

diagnosis in cases of cancer of the fundus uteri and fibroid disease of the body of the uterus. He had lately seen a case where not until after death was it shown that true cancer of the fundus uteri existed, and death resulted from perforation of the uterine walls with hemorrhage into the peritoneal cavity. During life none of the symptoms present more especially belonged to either cancer or fibroid disease.

---

*Epithelioma of Cervix Uteri complicated with Pregnancy: Removal of Diseased Portion: Subsequent Delivery of a Healthy Child: Recurring Pregnancy.*

By CHAS. T. SAVORY, M.D.

Dr. CLEMENT GODSON communicated the particulars of this most interesting case:—J. W., aged thirty-five, married sixteen years, eight children, had a sanguine and at times watery discharge, offensive in character. A large cauliflower excrescence was detected and removed by the *écraseur* on October 27th, 1870; on January 12th, 1871, she was delivered of a female living child, both doing well. On June 25th, 1873, she was again taken in labour, and after some little difficulty turning was accomplished and a dead child extracted, no undue hemorrhage occurring. Patient succumbed thirteen days after, apparently from sheer exhaustion, two years and nine months from the date of first operation.

Dr. GODSON remembered the case well. The enlargement of the uterus was attributed to the disease supervening so immediately on parturition. Pregnancy being overlooked the patient was perhaps spared the perils attending Cæsarian section. The case showed how much might be done to the uterus and its appendages during pregnancy without abortion taking place.

The PRESIDENT asked whether the question of making incisions in the cervix to facilitate the expulsion of the child had been proposed in place of Cæsarian section?

Dr. MURRAY remarked that a practical point of great interest was, that even if pregnancy be diagnosed there would be no harm in removing the mass.

Dr. J. BRAXTON HICKS remarked on the importance of having some rules for the management of cases of pregnancy in malignant diseases of the cervix. He thought it, however, almost impossible to obtain any. Each case must be judged of by itself. However, in considering whether we should induce labour, or leave the case for Cæsarian section at full time, a primary question would arise, can that woman live till full term? Again, in determining the question in regard to the induction of labour: is the disease limited to the os and lower cervix, or does it extend to the lower part of the body of the uterus, in such a way as to render delivery very difficult? If these are difficult, a third may arise, can we leave the induction to the viability of the child, or

must we procure abortion? So much depended on the amount and position of the disease, and the state of the patient, that Dr. Hicks feared it would be difficult to lay down definite rules. He instanced some of the cases which he had seen.

Dr. HEYWOOD SMITH thought it fortunate delivery did not happen soon after the operation, for he was sure that the puerperal state was a very great element of additional danger. He had a similar case some three years ago. Labour set in about the 5th month, delivery was effected by podalic version, and the child born alive. Removal of the malignant disease of the cervix was effected by means of the écraseur, the tissue cut through being apparently healthy. The patient, however, died on the 5th day. The better practice would have been to have postponed the operation until the puerperal state had been recovered from.

The PRESIDENT thought one of the most interesting points in the paper was that the operation was performed without bringing on labour. It was generally supposed that no operation should be performed during pregnancy, even removing a tooth, but the case proved otherwise.

---

Mr. SPENCER WELLS at the next meeting will introduce the subject for the evening's discussion, "The Relation of Puerperal Fever to Infectious Diseases, and Pyemia."

---

## OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, November 18th, 1874.*

DR. MATTHEWS DUNCAN, *President, in the Chair.*

*Description of a Diprosopus Triophthalmus Monster.*

By ANGUS MACDONALD, M.D., F.R.S.E.

This very rare and exceedingly curious specimen was brought to me by Mr. Crowdy, one of my Dispensary pupils, on the 27th of July last. The confinement had been managed by a midwife, who, however, took fright on seeing the fetus, and sent for Mr. Crowdy's assistance. The medical history of the mother is brief, and really contains nothing which can be construed as bearing on the monstrosity. It runs as follows:—

Mrs. Munro, aged forty-four, was confined on the 27th of July, 1874, of her eighth child. Her general health has been on the whole good, although she suffered at times from weakness arising apparently from over-lactation. She has been in the habit of nursing all her children for long periods, usually for two years; but on the

last occasion for two years and three months. Her children, with the exception of the present fetus, have all been healthy and well formed, and are indeed all alive and well at the present time, except one who died from measles and whooping-cough at the age of two years.

During the period she carried the present fetus, she states that she felt herself gradually getting weaker, and at the time of her confinement felt far from strong.

During the latter months of utero-gestation she felt very large and uncomfortable, so that she believed she was about to give birth to twins. She also suffered from severe pain in the right side, which extended down the right thigh, and which made her think she was going to lose the power of her right side.

Her previous confinements have all been normal, and her recoveries speedy and complete. This one was also easy and short, lasting only two hours. The child presented by the feet, and came easily away. The amount of liquor amnii, according to the statement of the nurse and of the patient, appears to have been excessive.

So far as she could calculate, her confinement was a month too early. She distinctly asserts that she felt the motions of the fetus on the day preceding the labour. After some extra exertion on the day of confinement, the water suddenly came away, and the expulsion of the child followed within about two hours afterwards.

But now to turn our attention to the child, which is the main object of consideration at present.

*General appearance.*—The fetus, which seemed to be, as far as the mother could judge, about the eighth month, is fairly nourished and of good colour. Its length is thirteen inches, and its weight three and a half pounds. The trunk and extremities viewed anteriorly seem regularly formed as well as symmetrical, and proportionately developed. Posteriorly, it is seen that the spinal cavity is very imperfectly closed in, as from the top of the sacrum upwards along the whole extent of the back the skin and other soft structures are entirely undeveloped, in a strip, to the extent of about two inches in width, the skin terminating on each side of this area in a free border as if it had been cut with a knife. The arches and spines of the cervical, dorsal, and lumbar *vertebræ* are also entirely wanting. Immediately within the edge of the true skin on each side run a range of hardened elevations, which look like the transverse processes of the *vertebræ*. There thus exists a condition of complete spina biida, the spinal cord being represented by comparatively small nervous cords lying along the floor of the sulcus formed by this open nervous cavity.

The sex of the child was female. The labia minora still protruded in front of the labia majora. The anus and vagina were perforate, but the nails on the fingers and toes were still short of the tips of the digitals.

The neck both before and behind seemed very imperfectly deve-



loped, and remarkably shortened in the direction of its longitudinal axis.

*Head.*—On the posterior, superior, and lateral aspects, the bones of the skull are entirely absent. The posterior and middle lobes of the cerebrum, as well as the interior, seem wonderfully complete. The cerebellum, however, seems to have been originally wanting, or else to have been destroyed during the labour.

*Face.*—The facial aspect, however, of the head, presents the most distinctive and most obvious peculiarities in this monstrosity.

It is essentially double throughout, but the inner aspects of the two faces are closely united throughout along the mesial line. The integration has proceeded so far as to form, instead of two eyes placed one on each side of the mesial line, one large compound eye with two eyeballs closely united.

On examining this double face we find that the apposition of parts is not according to parallelism, but such that, if we produce downwards in a transverse plane two straight lines representing the directions of the fronto-mental diameters of the faces, they meet at a point either in or very near the umbilicus of the fetus.

The double face is separated from the trunk anteriorly by no distinct neck, but only by an irregularly transverse notch.

The lower *maxillæ* are to be felt separate from one another, and both apparently perfect and well ossified; on each side a slight elevation of surface indicates and represents the chins.

Along the mesial line the union is complete, and the connecting surface even, except at one point midway between the chin and the central eye, where there is a slight depression.

The mouths viewed anteriorly form two irregularly triangular slits, the bases of which are inferior and the apices turned towards the inner ala of each nose. This appearance is effected by the presence of a large hare-lip in both mouths; the lower lip and lower maxilla forming the base of the triangle being normal, whereas the upper lip is very deficient and gapes widely, and a cleft passes backwards and inwards through it as well as through the entire mass of each of the molar and palatal arches at the inner angles of the two mouths, so as to produce both hare-lip and cleft palate, and thus establish free communication between the nasal and oral cavities along their whole length, in connexion with each face.

On passing two probes, one along each oral cavity, it is found that they meet posteriorly in a common pharyngeal cavity.

On the outer aspects of the faces on each side is a well-formed and apparently perfect eye. An ear is also present on each side, and seems regularly formed.

A small portion of both the frontal bones is present about three-quarters of an inch in depth and about one and a-half inches in breadth, and these are taken up in the formation of the orbital processes. No part of a frontal suture is to be felt in either portion of frontal bone.

The most remarkable feature in the double face, however, is the large central double eye.

This eye is composed of two large eyeballs, each with a separate iris and cornea, but united internally by a common sclerotica, or, if not common, at least closely adherent sclerotic coats.

Each cornea is about four lines in diameter, and their inner edges are separated by about two lines of sclerotica. The conjunctival covering is single, and there is a common upper and under eyelid for the compound eye. In its formation, however, there is seen, in its wavy outline, a visible tendency to differentiation into two eyelids.

The trunk and extremities presented nothing abnormal externally, with the exception of the spina bifida and defective neck. But the visceral development, for a careful dissection of which I am indebted to Mr. Thomas R. Ronaldson, is very peculiar and highly interesting. It is as follows:—

“A free crucial incision was made into the body anteriorly, the vertical part running from the upper end of the sternum superiorly to the symphysis pubis inferiorly, the transverse part running from side to side on a level with the eighth rib.

“On separating the four flaps thus made, two upwards and outwards, two downwards and outwards, the *diaphragm* was found in an imperfect condition. It was attached normally on the right side, but a little beyond the middle line it terminated in a round free muscular border which lay in a furrow on the upper and anterior surface of the enlarged left lobe of the liver.

“This border, on being traced more deeply, extended backwards and to the left so as to be attached to the last rib on the left side and to the rudimentary spinal column, lying over the aorta and forming a loop through which the esophagus passed.

“To its upper surface to the right of the middle line was attached the pericardium, the left side of the pericardium joining the diaphragm towards the mesial plane.

“To its under surface the liver was attached by the coronary ligament, which was normal on the right side but deficient on the left.

“From this condition of the diaphragm it followed that there was a communication on the left side between the thoracic and abdominal cavities.

“Parts lying superficially, and seen on opening the body:—

“1. *Thorax*.—In the right side of the thorax the heart, contained within its pericardium, was exposed.

“Superiorly, and lying on it, the thymus gland was situated, principally in the right side, but projecting also into the left side of the thorax. The enlarged left lobe of the liver occupied the greater part of the left side (*superficially*), hiding the other organs from view; but superiorly and externally could be seen a few coils of small intestine. The lungs were out of sight.

"2. *Abdomen*.—The liver occupied the right hypochondriac, right lumbar, right iliac, umbilical, epigastric, and part of left hypochondriac regions, while a large part of its left lobe was contained in the left side of the thorax, its anterior border running almost vertically downwards from near the apex of the left side of the thoracic cavity to the umbilical region, and then more obliquely towards the right iliac region.

"From its under surface the flaccid gall-bladder projected in the umbilical region. To its anterior surface, a few lines from the edge, the umbilical vein entered its surface (in the epigastric region).

"The left hypochondriac, left lumbar, left iliac, and hypogastric regions were occupied by coils of large intestine (corresponding to transverse colon, descending colon, and sigmoid flexure) loaded with meconium.

"Inferiorly to the liver, and in the hypogastric region, the bladder and uterus in addition projected into the abdomen.

"Parts lying deeply, and seen on turning aside the superficial parts :—

"1. *Thorax*.—On separating and turning aside the superficial parts in the thorax, the lungs were found firm and collapsed, lying on each side of and touching the pericardium.

"On turning up the left lobe of the liver, the stomach was found immediately beneath it.

"The esophagus, after traversing the posterior mediastinum, passed through a loop of the diaphragm as previously referred to. It then turned to the left almost directly to enter the cardiac end of the stomach, which lay curled up so as to form the greater part of a circle, the cardiac end lying above the level of the diaphragm and touching the left side of the pericardium as high as the third or fourth costal cartilage.

"The pylorus was situated about three-eighths of an inch below the entrance of the esophagus and in the abdomen.

"The concavity of the small curvature lay directly to the right, and in it lay a lobule of liver substance.

"To the cardiac end of the stomach and superiorly to it, the spleen, about the size of an almond, was attached (by gastro-splenic omentum), and lay in contact with the left lung, between it and the stomach.

"On raising the stomach and spleen, the part of the large intestine corresponding to the transverse colon was found, while lower down it appeared from behind the stomach and lay on a level with it and covered by the great omentum. Also behind the stomach as well as the large intestine, but projecting also superiorly and externally, the great mass of the small intestine, closely coiled up and apparently empty, occupied the greater part of the left thoracic cavity.

"2. *Abdomen*.—On turning aside the liver, which occupied the greater part of the abdominal cavity, the duodenum was seen lying behind it, in the epigastric and left hypochondriac regions. To the

left of the duodenum lay three vertical tiers of large intestine. Anteriorly, and seen on opening the abdomen, was a part corresponding to the transverse colon; behind lay the *caput cæcum coli*, and still further posteriorly part of the descending colon was situated. These parts, as well as a few loops of small intestine, lay in the left hypochondriac region. The left kidney, with its suprarenal capsule, lay in the left lumbar region behind the descending colon; the right kidney, with its suprarenal capsule, lay in the right lumbar region behind the liver.

“*Details as to the Arrangement of the Intestine.*—The duodenum turned towards the left after leaving the pylorus, and, after receiving the ductus communis choledochus, passed beneath the part of the large intestine corresponding to the transverse colon and the lower part of the ileum, and joined the jejunum. The first part of the jejunum ran upwards, and, along with the ileum, formed the coiled-up mass of small intestine which lay above the level of the diaphragm, and which occupied the greater part of the left thoracic cavity. The ileum entered the *caput cæcum coli* on a level with the diaphragm. The *caput cæcum*, with the part corresponding to the ascending colon, passed downwards and to the right as far as the epigastric region, then turned upwards on itself and became continuous with the transverse colon, which passed directly upwards covered by great omentum till it lay behind the stomach in the thorax; turning to the left, it passed beneath the neck of the coiled-up mass of small intestine. It then turned downwards and passed immediately over the bundle of muscular fibres representing the left side of the diaphragm, becoming now the descending colon, and occupying the left hypochondriac, left lumbar, and left iliac regions, the latter as the sigmoid flexure, it entered the pelvis as the rectum.

“Genito-urinary organs natural.”

This kind of monstrosity is exceedingly rare. Speaking of diprosopus or double face in general, August Förster, who is the latest, and perhaps best, authority on monstrosities, states that out of 500 cases of double monstrosity noted by him, only 29 were diprosopi.

But the term diprosopi includes five subvarieties, which may roughly be distinguished in the following manner:—

1. Diprosopus diophthalmus, when only two eyes are differentiated.
2. Diprosopus triophthalmus, „ three „ „ „
3. Diprosopus tetrophthalmus, „ four „ „ „
4. Diprosopus triotus, „ three ears „ „ „
5. Diprosopus tetrotus, „ all four „ „ „

So that when we subtract those cases recorded under the other four subvarieties of this monstrosity, we come to reduce its frequency very much indeed.

The cases collected too by Förster are six in number, gathered from a wide range of literature.

A curious fact regarding these monstrosities is noted by Förster,



and this is, that the female sex very largely predominates. Of the twenty-nine cases to which he makes reference, six were males, sixteen females, and in the case of seven the sex was neglected to be noted. An additional point of some little interest might also be referred to here—viz., that in this case there was undoubtedly a degree of hydramnios, a pathological condition which is known to occur much more frequently in connexion with female than with male births.

The diprosopi are not capable of living, as they have never been known without such an amount of defective development of the brain and spinal cord as to exclude the possibility of prolonged existence. It is also to be noted, that though the rest of the body may appear to be well developed—and so much is this the case that Förster says, “As a rule it is well built”—yet a careful dissection of the visceral arrangements might be expected, as in the present case, to reveal conditions incompatible with vitality, although a superficial external examination might not lead one to expect such derangements.

As to the origin of such a monstrosity, it seems to me, in the present state of our knowledge, idle to speculate.

As far as the mother was concerned, there seems no cause, except that her health was under par. But if the embryo of every woman whose health during pregnancy was poor, took to splitting up in this odd fashion, how frequent, instead of how rare, would double monstrosities be!

We are, however, bound to record such cases, in the hope that they may furnish data for some future worker, to aid him in elucidating those obscure causes interfering with the early differentiation of the embryo, which terminate in producing monstrosities. I meantime have only to say that there is little, if any, scientific ground for the common belief that monstrosities frequently arise from mental impressions received by the mother. In this particular case there is no fact to support such an idea. I am sorry that the desire to possess this singular specimen as an inmate of my museum, in as complete a condition as possible, has as yet proved too strong to allow me to get the face dissected, much as I would wish to do so, on many grounds.

---

*Meeting, November 18th, 1874.*

*On Prickle Cells in the Capsule of a Fibrous Polypus of the Uterus.*

By Dr. J. R. HARDIE.

The patient from whom the polypus about to be described was obtained was a woman fifty-three years of age, with a pasty-looking, blanched countenance, old-looking for her years, who was admitted to Ward XVI., Royal Infirmary, on the 18th August, complaining of occasional severe floodings and a constant copious white discharge. The floodings commenced about six years before admission—that

is to say, when the woman was forty-seven years of age; the bloody discharges lasted generally about a month, and weakened her very much; the interval between them varied from two to four months; she had her last flooding in June. She has had five children, the last sixteen years ago.

On examination per vaginam, a large rounded firm tumour, about the size of an apple, was found occupying the vagina; it could be traced into the cervix uteri, which was dilated round it; it appeared to be attached to, and to spring from, the cavity of the cervix.

The growth was removed by means of vulsella and scissors by Dr. Matthews Duncan on the 20th. The patient, with rapidity, regained health and strength, and was dismissed quite well on the 1st of September. It is interesting to note the unusual age at which the flooding commenced, forty-seven—an age at which we are usually warranted in encouraging a patient suffering from bleeding from a fibrous tumour of the uterus to expect a cessation, or at least a diminution, of the floodings; the intervals likewise would seem to indicate that they had no connexion with ovulation.

On dividing the tumour (to ascertain roughly its nature), it presented the usual appearances of a true fibrous polypus. Its investing capsule was thick. At, and in the immediate neighbourhood of the presenting point, the capsule was observed to present a rough irregular papillary appearance, which, on microscopical examination, was seen to be caused by an irregular denudation of the superficial layers of the flattened epithelium of which the greater thickness of the capsule consisted.

On microscopical examination, the polypus was seen to consist of fibrous and muscular elements—the former predominating. The capsule was composed of numerous layers of epithelium, flattened on the surface of the growth, and becoming more globular in the deeper layers. These epithelial cells have hair-like processes proceeding from them, which are more distinct on those on the surface, and disappear gradually as you proceed inwards; they are seen to interdigitate, but not in the pronounced manner in which processes of a similar nature, which exist in the epithelium of the skin or in some forms of cancroïd, are observed to do. There is evidently a close connexion between these cells and the prickle or rib cell first described by Schultze as existing in the skin.

---

*Meeting, January 13th, 1875.*

*Anencephalous Fetus.*

An anencephalous fetus was exhibited by DR. CAIRNS, who furnished the following particulars:—The mother is thirty-one years of age, and has had previously five children, all healthy. Twice the *placentæ* were adherent, otherwise her labours were natural. During this pregnancy she complained of being ill, and was unable to go about as she had done on all the previous occasions. The labour

lasted four hours. Arms presented. A large quantity of liquor amnii, and a placenta adherent to the fundus. The fetus presented the appearance of having been dead about a fortnight; the mother also complained of feeling sick and ill from about that period to the time of labour.

---

*Ovulation without Menstruation.*

DR. JAMES YOUNG mentioned the following interesting case illustrative of ovulation taking place without menstruation. Mrs. W. was married on the 13th of June, 1867, at the age of twenty-five years. She menstruated on the 13th of July, and her first child was born 4th April, 1868. The patient states that she nursed the child thirteen months, and then menstruated six times—till November, 1869. The second child was born on 15th of August, 1870, nursed five months, when the baby died. From November, 1869, till the present time, January, 1875, the patient has never menstruated, and during that period a third child was born, 31st October, 1871, and a fourth child was born on 12th September, 1873, and now a fifth pregnancy is going on, each child having been nursed twelve months.

---

OBSTETRICAL SOCIETY OF DUBLIN.

*Meeting, February 13th, 1875.*

LOMBE ATTHILL, M.D., *President, in the Chair.*

Dr. F. T. PORTER exhibited a specimen of a fibrous tumour which he said was of interest in this respect, that it was found quite free and unattached either to the Fallopian tubes or to the uterus. He had not heard or read of any similar case. He had heard of tumours attached to the uterus, and afterwards getting free. There was a tumour of smaller size at the other side, and a polypus attached to the os. The specimen was removed from the body of a woman fifty years of age.

The PRESIDENT said every one knew that fibrous tumours might be attached to the uterus by comparatively long pedicles, but here was one perfectly unattached. It was quite possible that it might have been originally attached to the uterus, but that the pedicle had been elongated and finally severed.

---

*On Metro-peritonitis following the use of the Ordinary Female Syringe: a Plea for the Vaginal Irrigator.*

By THOMAS MORE MADDEN, M.D., M.R.I.A.

The use of the vaginal syringe is the most common, and, until recently, was almost the only local treatment resorted to in uterine disorders. As this instrument is now so generally employed in all

cases of real or suspected disease of the womb, that is to say, in nine-tenths of the complaints peculiar to women, and as it is freely ordered by medical men and habitually used by patients without any special caution or apprehension of possible danger, the following case appears to me not unworthy of the consideration of this Society :—

Mrs. W., a healthy young lady, aged twenty-four, was delivered on the 16th of July, 1874, of her first child. The labour, which was very tedious in the first stage, was rendered difficult by rigidity of the os uteri and malposition of the child's head, and had to be completed with the assistance of my long forceps, an instrument of a peculiar construction which I have elsewhere described.\* Her convalescence presented nothing remarkable. On the 24th of August she called on me, complaining of pain in the back, a profuse yellow leucorrhœal discharge, and a distressing bearing-down sensation, which prevented her from walking or even standing with any comfort. The vagina was congested, the os patulous, and the uterus slightly prolapsed and enlarged. Absolute rest was enjoined, a tonic prescribed, and she was directed to use an astringent vaginal injection.

A few nights afterwards I was sent for, but being out of town, Dr. Egan, of Talbot Street, saw her, and found her suffering from intense uterine colic, which had come on during the use of the injection when going to bed a couple of hours previously. She was then cold, almost pulseless, and apparently semi-moribund. Having promptly administered stimulants, and applied sinapisms over the heart, &c., Dr. Egan, as he subsequently informed me, considered her condition so precarious, that he called in the assistance of Dr. Johnston, in consultation; and owing to the efficacious measures adopted, the most urgent symptoms were abated, the warmth was restored to the extremities, the pulse became apparent at the wrist, and the severe uterine pain was for the time considerably relieved.

On the following morning I visited her with Dr. Egan, who kindly left the case in my care, and found her still in a state of extreme prostration, and again suffering from frequent paroxysms of violent uterine pain and almost continual retching, with great tenderness over the abdomen, and especially over the uterus, which was as large and hard as it should be immediately after delivery. Her pulse was 140, and so weak that it could hardly be counted; her respiration was sighing; countenance pale and anxious, and the skin cold and clammy.

Poultices and anodyne stupes were applied to the abdomen; hydrocyanic acid draughts were given to allay the retching, and her strength was supported by enemata of brandy and beef-tea, with small doses of liquor opii. I again saw her the same evening: the incessant sickness and uterine pain continued; she was suffering from constant thirst, but no sooner did she take any fluid, or even a piece

---

\* Vide *Lancet*, June 20th, 1874.



of ice, than it was immediately rejected. She was ordered therefore to take nothing whatever by the mouth. A drop of hydrocyanic acid was added to each enema, and the expedient (which I have not seen alluded to by any writer) certainly diminished, though it did not stop, the vomiting. Notwithstanding the great depression of the pulse, the local uterine inflammation was so marked that I applied a few leeches over the seat of pain, and this gave considerable relief to the suffering, whilst the pulse actually became feebler and less compressible than before their application. Besides this, the solution of muriate of morphia was used hypodermically to relieve pain and calm the nervous system, though no sleep was induced by it.

On the 31st the metro-peritoneal pain had in great measure subsided, but there was yet considerable tenderness over the uterus, which continued to be perceptibly tumefied. Her pulse was 140 in the minute, weak and compressible; tongue dry and furred; decubitus dorsal, and sunk down to the back of the bed, and she was still suffering much from the constant nausea and dry retching. The same treatment was pursued as on the previous day.

*September 1st.*—She had slept a little last night; her pulse had fallen to 120, and the uterine tenderness was much less than yesterday; the retching and inability to take any nourishment or medicine by the mouth, though less marked, still continued. I suggested a consultation. Dr. M'Clintock accordingly saw her with me that afternoon, and I had the satisfaction of having my view of the case confirmed by this eminent gynecologist, who, in addition to the treatment already referred to, advised oxalate of cerium, in small doses, every third hour, and Dr. Halahan's egg drink to check the retching.

*September 2nd.*—The egg drink, in teaspoonful doses, had agreed with her, and the oxalate of cerium pills, which at first had been rejected, were now retained; it was necessary, however, to continue the enemata, &c., as before, though she gradually improved each day until the 5th, when she was able to take a little iced chicken jelly by the mouth, and from this time her recovery was rapid, so that on the 11th she was able to sit up for a couple of hours, and was soon convalescent.

In the foregoing case the uterine colic and subsequent metro-peritonitis followed so immediately on the use of the vaginal syringe as to leave no room for any doubt of their being caused by the fluid having been injected through the patulous os into the uterus, and probably also by some of it having passed through an enlarged or dilated Fallopian tube into the peritoneal cavity.

The probability of an accident of this kind attending the use of the ordinary female syringe is practically ignored by the great majority of gynecologists; yet cases similar to mine, although somewhat rare, have been recorded by other practitioners; but they would appear to have made less impression on the minds of those engaged in this branch of medicine than the importance of the subject demands. Dr. Tilt says: "Only once have I been led to believe that the

patient injected some portion of the fluid into the cervical canal. A lady was suffering from chronic uterine inflammation ; the womb was low and slightly retroflected ; the os uteri patulous ; and after injecting a solution of acetate of lead, as the patient thought, in the usual way, she was suddenly seized with severe uterine pains, rigors and intense cold. She got better when in bed, by means of abdominal poultices and hot drinks, and no bad consequences followed this attack.”\*

Dr. H. J. Bennet says : “ When disease really exists in the uterine cavity, the injections would, no doubt, do much good, and, were they safe, would be preferable to the solid nitrate of silver applied with the porte-caustic ; but there is reason to believe that uterine injections are not safe, and I do not now resort to them. Several deaths occurred in Paris, during my residence there, from metro-peritonitis, brought on by their use. One took place in the female ward of M. Jobert, in the Hospital St. Louis, and under my care, as I was then his house-surgeon. The patient, a fine healthy young woman of twenty-four, was afflicted with a large fibrous tumour of the uterus, which had much developed that organ, and had, no doubt, opened the os internum. M. Jobert was at that time trying the effects of the so-called uterine injections, and injected one astringent injection into the cervical canal of this young female, there being a slight muco-purulent discharge from the os. Shortly after she was seized with rigors, fever, and severe abdominal pain, and in a few days died of peritonitis. I performed the post-mortem and found nothing but the lesions of peritonitis, and the ovarian tumour embedded in a womb developed to the size which it presents in the fourth month of pregnancy. The fluid of the injection must have penetrated freely into the uterus, through the open os, and thence have passed along the Fallopian tube into the cavity of the peritoneum, thus causing fatal peritonitis.”†

M. Bernutz relates the history of a case of peritonitis “ which resulted immediately from the administration of a cold vaginal douche.”‡

These three quotations are the only references that I have met with to the dangers which may follow the use of the ordinary vaginal syringe, and the paucity of similar observations is certainly a fair argument that the accident in question is by no means frequent. Still the mere possibility of such serious, or even fatal results, being thus produced, should, I think, render gynecologists more cautious than is generally the case in their recommendation of this almost universally-employed and much-abused instrument. For my own part, I have been long convinced of the inconvenience of the vaginal syringe in

---

\* Dr. Tilt’s “Handbook of Uterine Therapeutics.” Third Edition. P. 54. London, 1868.

† “On Inflammation of the Uterus and its Appendages.” By J. H. Bennet, M.D. Fourth Edition. P. 146. London, 1861.

‡ “Clinical Memoirs on the Diseases of Women.” By MM. Bernutz and Goupil. Translated by Dr. Meadows. Vol. II. P. 78. London, 1868.

ordinary use, as well as of the possible ill effects of its misapplication. Some years ago I pointed out in this place how uncertain and undue might be the force with which injections can be thus thrown into the vagina or into the uterus, and I endeavoured to show that, even when not positively injurious, this instrument must necessarily be imperfect in its action, and inconvenient in its use. To produce any permanent beneficial effect by injections, in a case of congestion or inflammation of the cervix uteri for instance, the injected fluid must be kept in contact with the inflamed or congested part for a certain fixed length of time, and at a certain unvarying degree of heat, without any sudden alternations in either its temperature or the force with which it is impelled. None of these intentions can be carried out when the common siphon syringe is made use of, as the fatigue of working that instrument, and the position of the patient, which is so irksome during its employment, effectually prevent its being used more than a few minutes continuously. Moreover, the injected fluid is sent by it into the vagina in irregular intermitting jets, the force of which is directed against the inflamed part with a degree of violence that may be injurious, or of weakness that may be utterly ineffective, and which is regulated by the strength or patience of the operator rather than by the necessities of the case. In order to obviate these difficulties, various forms of utero-vaginal syringes, douches, and irrigators have been devised. It may be in the recollection of some present that a couple of years ago I exhibited a very simple, and, I believe, very effective instrument for the same purpose before the Society. The irrigator in question\* (a description and drawing of which may be seen in the second volume of our Proceedings, p. 183), is one I have now used for some years, and which, without asserting that it is in any way superior to other instruments of the same kind, I have found more generally useful than the ordinary syringe. With this irrigator, the accident which forms the subject of the present paper could not have occurred, as it is merely a siphon, so constructed that it can be set in action or stopped in an instant, at the will of the patient.

It has, moreover, the great advantage of being very portable, may be readily used whenever a vessel of water can be obtained, is capable of sending a gentle continuous current of water, plain or medicated, and at any temperature, into the vagina, or even into the uterine cavity itself, if ever that measure—rarely required in gynecological, as distinguished from obstetric practice—should be considered expedient, and this, too, in any position, sitting or recumbent, and for any length of time that may be advisable, without causing the slightest fatigue to the patient. The advantages of the irrigator over the common syringe I have proved by experience during the last few years, and the comfort and benefit which those

---

\* Made by Whyte, Upper Sackville Street, Dublin.

patients who have employed it have derived from its use would lead me to recommend any gynecologist who has not hitherto done so, to give a trial to the irrigator as a substitute for the vaginal syringe in most cases. This instrument is not only effective and easily used, but is also easily constructed. Dr. Graily Hewitt, Dr. Kidd, and several others, have recommended and devised irrigators which many would, perhaps, prefer to the one I have brought before this Society. But it is in the power of any one to convert an ordinary vaginal syringe into an excellent irrigator by the mere addition of two pieces of india rubber tubing and a small stop-cock to it. These may be attached to any syringe, which may be thus made perhaps fully as effectual as the most complicated instrument that could be devised for the purpose.

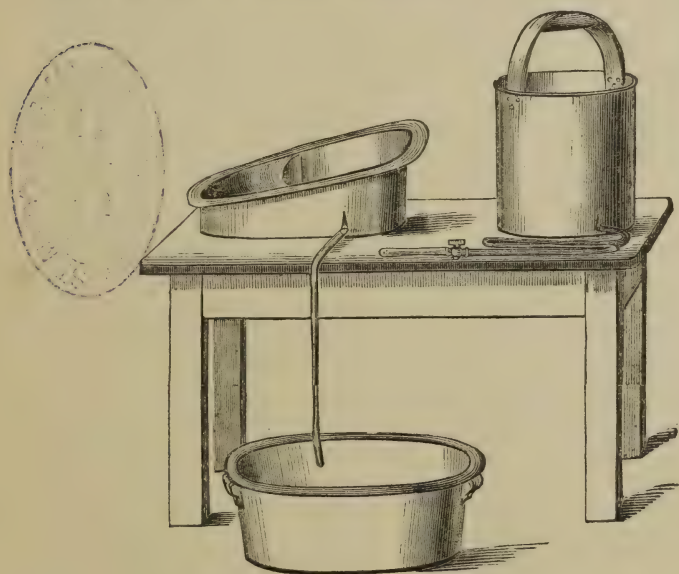
In another point of view cases, such as those just referred to, appear to me of some interest—namely, as to their bearing on the recent discussion as to the safety of strong astringent injections into the uterus immediately after delivery, in order to arrest hemorrhage. Against this practice it has been urged that there is danger of forcing the injected fluid through the open uterine sinuses into the circulation, or through the Fallopian tubes into the peritoneal cavity, thus causing, in the former case, death from embolism, and in the latter giving rise to fatal peritonitis. As on a former occasion, when this subject was under the consideration of the Obstetrical Society, I expressed a strong opinion against the probability of such an accident being thus produced, I feel bound to say that the case I have now related has, to some extent, modified my views on this point. For if metro-peritonitis may be occasioned, as it was in this instance, by a fluid injected into the uterus five weeks after delivery, it is obviously much more probable that a similar effect might be thus occasioned immediately after parturition, when the uterine vessels and passages are so enlarged and pervious. But I should also add that the possibility of such an accident does not in the least alter my opinion of the utility of the powerful styptic brought into midwifery practice by Dr. Barnes, nor would it deter me from again resorting to the injection of the solution of perchloride of iron in any case of post-partum hemorrhage which could not be otherwise controlled.

The PRESIDENT observed that Dr. Madden's brief paper opened up a very important subject for discussion. He did not think the occurrence of uterine colic following the injection of fluids by the syringe, was a very rare occurrence, inasmuch as he had seen three cases of it in his own practice. In one case only a few drops of glycerine were injected into the cavity of the uterus, as recommended by Dr. Marion Sims, and it produced most intense colic, but no peritonitis or endo-metritis followed. Some two years ago he was called, late at night, to see a patient whom he had directed to use a weak solution of borax injected into the vagina with an ordinary syringe. He found her in a state of collapse, suffering from pain referred



to the uterus and sickness of stomach. Her symptoms were speedily relieved, and no inflammation followed; while a less severe attack occurred in a patient who used tepid water only. He thought these cases, in which the injection of a fluid into the uterus was followed by colic, were far from being of very rare occurrence, and he advised that the central hole in the nozzle of the syringe be stopped, as a means of preventing this accident. He did not think, however, that the data given by Dr. Madden carried out his theory that the fluid passed into the Fallopian tubes, and thence into the peritoneum. The phenomena in Dr. Madden's case might be explained by the occurrence of a severe attack of endometritis in the first instance, followed by peritonitis. The exact same train of symptoms which Dr. Madden had described—the prostration, collapse, and vomiting—occurred in a patient where he (the President) had swabbed out the uterus with perchloride of iron. The patient was suffering from profuse hemorrhage, occurring some weeks after abortion; the os was patulous, and he had no difficulty in passing a pledget of cotton, saturated with the styptic, into the uterus; this was followed by a train of symptoms exactly similar to those Dr. Madden had described, but it was impossible that the fluid was passed through the Fallopian tubes. Certainly Dr. Madden was quite right in saying that vaginal injections were not perfectly free from danger. He (the President) greatly preferred a douche, similar to that spoken of by Dr. Madden, to the use of a vaginal syringe, and he had recently (acting on the suggestion of Dr. Emmett, of New York) carried out that plan extensively. Dr. Emmett advocated strongly this vaginal irrigation with water, varying from  $95^{\circ}$  to  $105^{\circ}$  of temperature. He based his treatment on the analogy that existed between the effects produced by it and an ordinary linseed poultice, or other hot application, applied to the skin. He says:—If you leave a linseed poultice on for some time, the skin becomes corrugated and white, showing that, though the first effect of the warmth was to attract the blood to the surface, this was followed by contraction and diminution in size of the calibre of the blood-vessels. He (the President) did not think this analogy held good, for, in the case of the application of a poultice, four or five hours elapsed before bloodlessness of the part occurred, while the vaginal douche could hardly be prolonged over fifteen or twenty minutes. Therefore, although considerable benefit followed in many cases from the hot vaginal douche, he did not think the theory was not quite correct. To use the vaginal douche efficiently it was necessary that the hips be raised higher than the shoulders, so that the vagina may be distended with the fluid; that the stream of fluid should be continuous, and that it should be kept up to an equal temperature. He (the President) had tested this method extensively. In a case of metritis, where the temperature of the water was raised to  $110^{\circ}$ , the patient derived much benefit. In another, a case of vaginitis, the patient had been subjected previously to treatment of various kinds without benefit. Two gallons

of water, temperature  $105^{\circ}$ , were used twice daily. She derived the greatest relief, and was, in fact, cured by it. The great difficulty in carrying out this treatment, where a large quantity of water necessarily is used, was that the bed-pan on which the patient lay had to be emptied constantly. To obviate that difficulty he had introduced the plan of attaching a tube to a bed-pan of peculiar shape, by which means the water was carried away. The pan must be placed on a firm surface, and, if this precaution is taken, no trouble whatever is experienced in using it. The water for the douche should be contained in a case such as that used by surgeons for irrigation, and be raised to the height of a few feet above the level of the couch or table on which the patient rests. The President exhibited the apparatus, which was of a very simple construction.\*



Dr. JOHNSTON said he thought Dr. Madden was under a mistake in mentioning his name, as he did not remember having been called in to see the case.

Dr. KIDD said uterine colic, following vaginal injection, was not an uncommon event. Patients will use vaginal injections for weeks without suffering any inconvenience, and then it sometimes happened that they got colic. As a rule it came on almost while the injection was being used, but sometimes it would not come on for a con-

---

\* Made by Fletcher and Philipson, 10, Lower Baggot Street, Dublin.

siderable interval. He had never seen it followed by any inflammatory attack. It produced a good deal of pain and, sometimes, collapse, but stimulants and anodynes generally relieved the patient. It had always seemed to him that these cases arose from some of the fluid getting into the uterus from the tube, and, like the President, he had sometimes desired the parties to have the central aperture closed. On the other hand, if the central aperture were plugged, the plug was likely to be shot out by the injection. He believed that an easier way of preventing the accident was to direct the patient to put the tube up the vagina as far as can be done, and then withdraw the tube a short distance. So far as his experience went these cases occurred principally where the os was patulous, and there was marked retroflexion of the uterus. A very eminent medical gentleman told him that he never saw uterine colic occur when he had used a warm injection, but that he had seen it frequently occur after cold injection. He did not see the analogy between uterine colic and any condition that might occur from the injection of perchloride of iron after delivery.

Dr. MACSWINEY said they had heard a valuable discussion respecting uterine colic, but he thought Dr. Madden referred to something else. He (Dr. Madden) related a case in which he detected the existence of inflammation of the peritoneum. It was well known to most persons that the introduction of the finger into the uterus may produce uterine colic, but he should like to know whether it was in the experience of obstetricians that inflammation of the peritoneum could be produced by the injection into it of water hot or cold. It must be admitted there were few cases recorded in which serious inflammation, extending over days, was produced by the use of an irritating vaginal injection.

Dr. DARBY was under the impression that it was physiologically impossible to inject any fluid by the vagina and uterus through the Fallopian tubes into the cavity of the abdomen. They all knew that uterine cramp and metritis were not uncommon, and they also knew it was not an unusual practice of late to inject perchloride of iron to check hemorrhage. In such cases he considered it would be an error to assume that the uterine colic was a consequence of the injection. He should rather say that it occurred as a coincidence, and that metritis was due to some other cause.

Dr. M'CLINTOCK said the communication of Dr. More Madden raised an important practical question. Every one who had dabbled in gynecology thought he was perfectly safe in practising vaginal injection. Dr. Madden, however, had given them another instance of what they were all familiar with, that the simplest remedies and operations, apparently the safest, will in rare and exceptional cases prove highly injurious or even dangerous to life; and instances might be given where even a small incision had been followed by death. But the occurrence of such rare instances ought not to deter us from the use of any remedy, whether surgical or therapeutical. They could

not speak positively as to the cause of the alarming symptoms described by Dr. Madden. They never could know whether any of the fluid went into the cavity of the uterus or not. All they could say was that such was possible, and, in Dr. Madden's case, the circumstances were highly favourable for the entrance of the fluid into the uterine cavity, as the lady had only been three weeks confined, the os was patulous, and the uterus prolapsed. Hitherto he (Dr. M'Clintock) had been in the habit of telling his patients that they might use the syringe freely, and that it could not do any possible harm; but now he saw such a direction would not be always a safe one. He had seen sharp pain follow an injection, but no serious consequences. He could quite understand that when the injection was introduced cold it might be injurious, and he had generally told his patients to use it tepid and gradually reduce the temperature. He had had an opportunity of seeing the case which Dr. Madden had brought under their notice, and he agreed in Dr. Madden's diagnosis. There was no doubt whatever but that she had a dangerous attack of metro-peritonitis following the use of the injection, and evidently produced by it. The case was therefore very striking and remarkable, and should be kept before their recollection.

Dr. FITZPATRICK said he had been using the syringe for many years, and was he now to be deterred by the narration of Dr. Madden from continuing to employ that valuable instrument? Of course, it was important to use an instrument properly adapted to the purpose in view. The orifice ought to be at the side, and not at the end of the tube.

The PRESIDENT repeated that a patient, who was under his care not long ago, was ordered to use a syringe, and on one occasion, while in the act of using it, was seized with intense pain, and exhibited the usual symptoms of uterine colic. When he saw her she was in great pain, and yet there was no tenderness on pressure; the attack was non-inflammatory, and passed off in a few hours, the symptoms being relieved by appropriate treatment. The President, after briefly alluding to the other cases which had come under his observation, in one of which uterine colic had been produced by the injection of a few drops of glycerine, expressed his opinion that they had not diverged from the question; because the subject of Dr. Madden's paper was uterine colic followed by metritis, and not metritis alone. He also stated that he fully concurred with Dr. Kidd and Dr. M'Clintock, that cold water for vaginal injections was objectionable.

Dr. MORE MADDEN said he had only to express his gratification that his brief paper had led to so lengthened and interesting a discussion. The President had related cases of great importance bearing out his views, the only difference being that in his (Dr. Madden's) case, as Dr. MacSwiney had observed, peritonitis followed, the fluid having passed from the uterus into the peritoneal cavity. With



regard to irrigation, various forms of irrigators had been used. In addition to the very ingenious and, in many respects, very valuable apparatus shown by the President that evening, he (Dr. Madden) had exhibited an instrument for the same purpose two years ago, and others had done the same. As to Dr. Kidd's observation, that these cases of uterine colic arose from retroflexions of the uterus, he could assure him that there was no such thing in the present case. The patient had been carefully examined by Dr. M'Clintock and himself, and there was no displacement of the uterus. Dr. Fitzpatrick declined to give up the use of the syringe, but that was, of course, a matter for his own consideration. Dr. M'Clintock had most succinctly summed up the object of his paper, which was to show that in certain cases the vaginal syringe was not so desirable an instrument as it was often supposed to be, that it might give rise to unpleasant symptoms, and that in the majority of cases the use of the irrigator was preferable to it.

## Obstetric Summary.

### *A Case of Immediate Transfusion by Aveling's Method.*

By JAMES R. CHADWICK, M.D., of Boston.

Mrs. M. N., thirty-two years of age, entered the Massachusetts General Hospital on May 27th, 1874, eleven weeks after the delivery of her second child. The labour had been short, but attended by profuse hemorrhage; this hemorrhage had recurred frequently until two weeks before her entrance to the hospital, when it ceased. The convalescence had been satisfactory in other respects, so that she was able to be out at the end of the third week. The repeated losses of blood, however, soon began to undermine her health, previously robust, until she was completely blanched, had a persistent headache, and frequent attacks of syncope; she was in this condition on entering the hospital. Bowels constipated, pulse 108, urine dark-coloured.

May 28th. Iron, eggs, broth, and two ounces of sherry three times daily were ordered. Dr. F. Minot, in whose ward the patient lay, invited me to perform transfusion, if I thought advisable. On learning her history, I declined to operate until the ordinary methods of treatment had been tried and found inefficient, not deeming the case a favourable one for transfusion.

On June 2nd she was failing, had had nausea and vomiting all night.

June 3rd. Less vomiting. Dr. Minot notified me that she was sinking. In view of this I agreed to operate on the following morning. At 6 o'clock P.M. I took these notes of her condition:—Great emaciation, sallow waxy complexion, extreme prostration; pulse 118,

respiration 30, temperature  $102.8^{\circ}$ . A loud souffle with the first sound of the heart was considered to be anemic. No bruit du diable over the jugular veins. Areas of hepatic and splenic dulness were not enlarged. A drop of blood from her finger was seen to be very deficient in colour; under the microscope, the number of both red and white corpuscles was found to be greatly diminished when compared directly with healthy blood. Urine normal, no albumen by nitric acid test; urophein increased, uroxanthin normal. With microscope no casts, but very few and doubtful disintegrated blood corpuscles.

June 4th. *Operation*, with the assistance of Drs. Minot, Ellis, Lyman, C. P. Putnam, and J. J. Putnam, of Boston, E. J. Forster, of Charlestown, L. Wheeler, of Worcester, and a number of house-officers and students. Dr. J. J. Putnam furnished the blood. Aveling's instrument for immediate transfusion was used. Dr. Putnam, having seated himself upon a stool beside the bed, extended his left arm by the side of the patient's right arm. A fold of skin over the median basilic vein of the patient was then raised, and a transverse incision made: the wound gaped so as to expose the vein to view. The anterior wall of the latter was raised by the forceps, and a V-shaped cut made with the scissors. The afferent nozzle was next taken from the basin, with the forefinger closely applied to its external end to prevent the escape of the warm water; it was inserted into the vessel without difficulty, and entrusted to an assistant. A direct longitudinal incision was then made into Dr. Putnam's median cephalic vein, but the attempt to introduce the efferent nozzle failed, despite persistent efforts, owing, I believe, to the bifurcation of the vessel just below the opening. Dr. C. P. Putnam now offered his arm, and a similar cut was made directly into the left radial vein; the nozzle, however, could not be inserted until a slight transverse incision had been made in the integument. The afferent nozzle had by this time become filled with blood, so that it had to be withdrawn from the patient's vein, cleared of the clots, refilled with water, and reinserted. The rubber tube, likewise filled with warm water, was then affixed to the nozzles, and the process of transfusion inaugurated by the injection of the syringeful of water into the vein, and prosecuted by the repeated filling and emptying of the bulb.

According to the printed directions, sold with the instrument by Messrs. Krohne and Sesemann, of London, the bulb should hold two drachms; on this assumption I based my calculation as to the amount of blood transfused, but a recent measurement of my bulb reveals its capacity to be precisely four drachms, a discrepancy of most vital importance, and one which, in my opinion, contributed largely to bring about the fatal result hereinafter recorded.

The following notes were taken during the operation:—

After sixth evacuation of bulb, pulse 118, some nausea, patient complains of heart-beat and backache. More colour in lips and cheeks.

After eleventh, pulse fuller, now 108.

After thirteenth, pulse fuller, now 104; bulb empties less freely.

After nineteenth, pulse 104.

After twentieth, bulb does not fill readily.

After twenty-second, operation terminated.

Some retching after withdrawal of nozzle.

During the operation, Dr. Ellis applied the stethoscope to the chest several times, but failed to detect any change in the character of the respiration or of the heart-sounds. The anemic souffle was unaffected.

Some difficulty was experienced in stanching the flow of blood from the incised vein. Before I left, the patient expressed herself as feeling much better and stronger than before the operation; she was free from headache or backache.

For the subsequent notes I am indebted to Mr. W. F. Whitney, medical house-officer of the hospital.

10 A.M. Severe rigor, lasting nearly an hour. Extra blankets, brandy, heater to feet, and sulphate of morphia, one-fifth of a grain subcutaneously, afforded relief. Brandy punch was ordered to be given every hour.

10.30 A.M. Temperature 106.3°.

11 A.M. Temperature 106.4°.

11.30 A.M. All oozing from wound had ceased. Pulse 134; temperature 105.3°.

12 M. Has been vomiting constantly for the last hour. The bandage about the wound is found to have become loosened and at least four or five ounces of blood to have escaped into the bed. A ligature is put around the vein. Some vomiting.

1 P.M. Temperature 104.2°.

3.30 P.M. Vomiting ceased at 3 P.M. Patient is now delirious.

4.30 P.M. Comatose; temperature 103°.

June 5th. 8 A.M. Comatose during the night; now conscious, recognising husband. Stimulants. Failed during the day, and died at 7.30 P.M.

*Autopsy* by Dr. Fitz, twenty-six hours after death; the following are his notes:—

*Skin* very pale. A watery blood is pressed from the proximal portion of the incised vein. Right hand moderately edematous.

*Acute Internal Pachymeningitis*.—Inner surface of the dura mater on both sides shows patches of a thin, delicate, translucent false membrane, very readily detached and containing numerous minute hemorrhagic spots.

*Increased Fluid in Pericardium*.—About two ounces of clear yellow fluid in pericardium.

*Anemia*.—The heart, in common with all the other organs of the body, except the spleen, is extremely anemic. The valves and cavities are healthy; no thrombi in the latter, nor are any found in the pulmonary arteries after careful search.

*Spotted Fatty Degeneration of the Heart*.—Almost the entire inner

surface of the left ventricle is spotted with minute yellowish-white opaque points. A section through the walls reveals a similar appearance, but the spots are more thickly clustered, and extend into the substance for at least two-thirds of its thickness. The right ventricle is in a like condition but to a considerably less extent. The microscope shows these spots to be due to fatty degeneration.

*Edema of Lungs.*—Lungs extensively edematous. Kidneys pale. Tubules of cortex indistinctly separated. About one-third of right kidney is atrophied; the surface is depressed, granular, dirty red, and tough. The veins leading from this part are filled with a firm, decolorized, adherent thrombus, similar to that found in one of the uterine veins, both being unmistakably of long standing.

*Retrogression of Uterus.*—Uterus of normal size. Walls cut with greater ease than usual; surface of section opaque. The membrane lining the cavity presents a tripe-like aspect from the intersection of slightly elevated red ridges.

The urine taken from the bladder at the autopsy was analysed by Professor Wood with the following result:—Colour slightly smoky. Reaction slightly acid. Specific gravity, 1012. Much sediment. Urophein diminished. Uroxanthin increased. Urea, chlorides, and uric acid diminished. Sulphates, earthy and alkaline phosphates normal. Albumen about one per cent. Sediment is chiefly bladder epithelium. Considerable pus. Few renal epithelial cells. A few brown, coarsely granular, and hyaline casts. One blood corpuscle seen in six specimens. The casts were chiefly large, but some were of medium and small size. Hematin was present, as shown by brown coagulum of albumen on heating, and also by brown precipitate with sodic tungstate.

*Remarks*—Neither the train of symptoms presented by this patient, nor the pathological changes found post-mortem give a clue to any serious malady other than universal fatty degeneration. The atrophy of one kidney was clearly connected with the thrombosis of the renal and uterine veins, and may unquestionably be referred to the early days of childbed; there was no indication that the functions of the kidneys were impaired. At the autopsy, the most careful search throughout the whole vascular system failed to discover any clot that could awaken a suspicion of embolism such as might be caused by coagulation of blood in transit through the tube. No froth or air-bubbles were found in the vessels.

The fatty degeneration of all the internal organs, and especially of the heart, was the direct result of imperfect nutrition, due to the frequent losses of blood. The diminution in volume of the circulation, which is the first effect of a hemorrhage, is soon corrected by the osmotic extraction of serum from all the tissues of the body, but a corresponding deterioration in the quality of the blood results. This poverty of the blood, nature can rectify, if she is given time. Multiply the large losses of blood at short intervals, however, and she succumbs in the effort to make good the extraordinary waste; not only does nutriment fail, but the circulating medium, by means of which



it is distributed throughout the economy, is wanting. No wonder, then, that the system is poorly nourished, and soon becomes too far reduced to carry on the functions of life. This dependence of fatty degeneration upon frequent losses of blood has been recently pointed out by Gusserow,\* Ponfick,† Perl,‡ and others. Ponfick describes the identical spotted fatty heart—of normal size and sound valves—found at our autopsy, as occurring particularly in women who have lost much blood in childbed. According to him, it is associated with a marked reduction in the number of the red blood corpuscles and in the amount of fibrine. Dropsy is almost constant. Perl succeeded in developing this fatty degeneration of the heart in dogs, by bleeding them a few times to a great extent. When small amounts of blood were abstracted quite frequently, no such result was obtained.

The direct practical bearing of these remarks becomes apparent when we realize that the introduction of blood into the vascular system, when it is already filled to about its natural capacity, is quite a different matter from infusing blood when the vessels are empty and collapsed. In the former case, the vascular tension is increased far beyond the normal; the cavities of the heart are obliged to dilate far beyond their natural limits, and must then contract with more than their habitual force in order to propel the augmented volume of the circulation through the comparatively narrow channels. In short, a severe task is imposed upon the heart, for which, in such instances as the present, it is incapacitated by disease. By transfusion we seek to better the quality of the blood, but this end may generally be compassed by other means, so that we have no right to employ transfusion, with its attendant risks, until the other remedies have failed.

The fatal termination in this case was certainly inevitable, but was in all probability hastened by the shock of the operation and the subsequent hemorrhage, with the attendant mental perturbations, but more particularly by the direct strain to which the heart was subjected by increasing the volume of the circulation.

These views had not taken so definite a shape in my mind before the operation; yet for these very reasons I had designed to restrict the amount transfused to six ounces. In this I was foiled by the erroneous statement of the description bought with my instrument. I transfused eleven ounces instead of six as I supposed, an error—as may be inferred from what has gone before—most prejudicial to the chances of the patient. Recent reflection has convinced me that transfusion is attended by great risk in chronic cases associated with fatty degeneration of the heart, and that this state of the heart is to be suspected in all patients who have been subject to repeated losses of blood. If we decide to employ transfusion, in spite of this unfavourable element, the danger must be as far as possible averted by the introduction of a very small

---

\* "Archiv für Gynaekologie," 1871, page 218.

† "Berliner Klinische Wochenschrift," 1873, No 1.

‡ Virchow's "Archiv," 1874, page 39.

amount of blood at one time, and repeating the process later if required ; or the poor blood of the patient must be allowed to escape from one arm, while the healthy blood is being injected into the other.

Acting upon these convictions I have advised against the operation in several consultations to which I have been summoned during the past summer. On one occasion I was persuaded to go sixty miles to transfuse lamb's blood into the veins of a consumptive. I went after repeated solicitations and a distinct disavowal—on my part—of any belief in the curative agency of transfusion in such diseases. On examining the patient I found, in addition to extensive disease of both lungs, very laboured action of the heart, and obtained the history of much pain and distress in the cardiac region, and a number of fainting turns during the previous month ; the patient was likewise greatly emaciated. I represented to the man the peculiar danger which would attend the transfusion of blood into his veins, and finally persuaded him to renounce the project. A month later, however, a more daring surgeon from New York, a German, successfully transfused six ounces of lamb's blood into the patient. My prognostications of the exceptional risk were fully verified by the unusual symptoms subsequent to the operation. There were "sharp pains throughout the back, chest and limbs" immediately after the operation ; on the next day, again "acute pains in the back ;" on the following morning, "two fainting spells in quick succession," and a pulse of 130 ; on the fourth morning, "palpitation of the heart" for half an hour, and again in the afternoon lasting two hours. Since that date no untoward symptoms have occurred, but the patient has recently published a card in the local journals announcing that his condition has not been improved by the operation, and warning others from trying the experiment.

Aveling's instrument worked well. The slight obstacle to the emptying of the bulb after the thirteenth evacuation I attributed at the time to a tilting upwards of the external end of the afferent nozzle in the hands of my assistant, whereby the oblique internal opening was forced against the wall of the vein and thus closed as by a valve. To this view I still hold. The tardy filling of the bulb after the twentieth evacuation may fairly be set down to the anemic condition produced in the blood-donor after the loss of eleven ounces of blood. The difficulty of obtaining a steady flow from an incised vessel was, I am told, often experienced in the days of frequent blood-letting. I can quite understand, however, that these explanations may not seem satisfactory to those who look with disfavour upon immediate transfusion.

In conclusion let me ask whether the fact that eleven ounces of blood were infused without sensibly modifying the anemic souffle, taken in connexion with the fatty degeneration of the heart, does not throw a little discredit upon the theory that this souffle is due to the impoverished condition of the blood? May not further investigations demonstrate that it is symptomatic of a fatty heart?—*Boston Med. and Surg. Jour.*, January.

## Gynecic Summary.

### *On Acute Parenchymatous Metritis.*

Dr. Alphonse Guérin (*Annales de Gynécologie*, February 2nd, 1875), concludes that the treatment of acute metritis, when of puerperal origin, should consist in a free application of leeches to the hypogastrium, or, where the symptoms are severe, general blood-letting should be resorted to. Purgatives, irrespective of lactation, the most pressing consideration being to save the patient. Injections to prevent the lochia becoming fetid. Mercurial inunction night and morning, poultices being subsequently applied, and opium to allay pain. Preventive treatment in the form of friction and compression to the uterus he considers very important, as also retaining the dorsal decubitus until the uterus has regained its normal dimensions.

---

## Pædiatric Summary.

### *Note on Cremation.*

"Though burning was the ordinary custom of the Romans, yet in some particular cases it was positively forbid, and looked upon as the highest impiety. Thus, infants who died before the breeding of teeth were enclosed unburnt in the ground."

"Terra clauditur infans  
Et minor igne rogi."

*Juvenal Sat. 15.*

The place set apart for the interment of these infants was called *suggrundarium*. "*Romæ antiquæ notitia*," by Basil Kennett.

The form of a *suggrundarium* is engraved by Thom. Bartholinus in his "*Antiquitatum Veteris Puerperii Synopsis*," 1676, p. 167, where also will be found the following quotation from Pliny:—

"Hominem prius quam genito dente cremare mos hominem non est."—*Natur. Histor.*, Lib. vii. cap. 16.—[Ed. O. J.]

---

## BOOKS RECEIVED.

"Clinical Lectures on Diseases Peculiar to Women." By Lombe Atthill, M.D. Third Edition. 1875. Pp. 294.

---

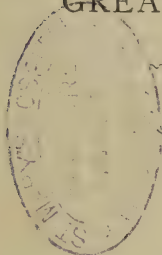
Communications have been received from Dr. Braxton Hicks, Dr. John Brunton, Mr. Cullingworth, Dr. Grosholz, D. A. Henry, Dr. Cordes (Geneva), Dr. Atthill, Dr. Edis, Dr. T. Chambers, and James H.

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

# THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.



No. XXVI.—MAY, 1875.

Original Communications.

## THE PUERPERAL AND PYEMIC PROCESSES.

By Dr. HJALMAR HEIBERG,

Professor of General Pathology and Pathological Anatomy in the  
University of Christiania.

THIS paper contains matter of special interest at the present time. It commences with a brief historical sketch of recent researches and doctrines regarding pyemia; after which the author makes the following remarks:—

After a labour, a surgical lesion with destruction of the continuity of the integuments, or an operation, an individual is exposed to two essentially different forms of after-disease—viz., 1. Simple traumatic disease; and 2, Specific pyemic affection.

1. Simple traumatic diseases are the direct results of the lesion, of the lacerations and contusions, which may be produced by a difficult labour, or (after external violence) of effusion of blood, or of the irritation of ligatures and sutures, immoderate bandaging, &c. In consequence of these a puerperal woman may have endometritis, metritis, parametritis, perimetritis; or if there be severe lacerations and ruptures, diffuse peritonitis, which may cause death in a few days. In a surgical patient, simple traumatic reaction will show itself in the form of erysipelatous redness around the wound; or of a more deeply-seated phlegmonous affec-



tion, with more or less extensive suppuration and perhaps sloughing. The results of thrombosis in the veins must also be counted among the simple traumatic after-diseases. Although such thrombosis is to be regarded as frequently a necessary factor in the arrest of the circulation rather than as a complication, it is often the starting-point not only of local changes, such as edema, phlegmasia alba dolens, &c., but also of secondary embolic affections.

2. The above mentioned diseases must be carefully distinguished from specific pyemic affections. In these, the essential factor is not mechanical irritation or thrombosis, but an injurious material—a *materia peccans*—which acts on the wound, and from it may spread to the surrounding parts, or may even infect the whole organism. If this peccant matter gain access to a wound or the inner surface of the uterus, it may remain localized, and give rise to a traumatic diphtheritis, which may be most correctly regarded, with Buhl, as a superficial necrosis of the base of an ulcer, of granulation tissue, or of the portion of uterine mucous membrane that is left behind, without stating explicitly whether this diphtheritis is identical with the diphtheria specially affecting the throat. If the peccant matter penetrate more deeply, it produces serpiginous necrosis (Jacob Heiberg) or hospital gangrene; or erysipelas ambulans, diphtheritic phlegmon (Hüter), or acute purulent edema. Similar conditions are seen in the uterus, in the form of the so-called *putrescentia uteri*, a necrosis of a greater or less portion of the womb, which must be placed in the same class with hospital gangrene; or there may be specific parametritis, with diffuse puriform edema; and, as a further extension of the disease, perimetritis, or even diffuse peritonitis. It will, however, be more thoroughly shown hereafter, that puerperal peritonitis does not always arise in this manner from a direct transmission of inflammation from the uterus and its appendages.

In other cases the peccant matter proceeds in other ways, often without leaving special traces in the wound, or on the inner surface of the uterus; just as a burglar may leave no footsteps or other traces to show how he has entered.

The infective matter passes from the wound through the serous canals (*Saftkanälchen*) of the connective tissue, into the roots of the lymphatics and the lymphatic vessels, and follows the lymph-current as far as the nearest glands; here it is arrested, not only by the difficulty of the passage, and all the obstructions which it meets, but probably also because the lymphatic glands have a peculiar physiological power of retaining injurious matters. Lymphangitis and swelling of the lymphatic glands may then be developed. The swelling is not only produced by the accumulation of lymph-cells, but also, as G. A. Hansen has pointed out with regard to the glands in typhus, by a swelling of the follicular substance, whereby the lymph-sinus is compressed, the circulation in it impeded, and the noxious material more readily hindered from passing through the gland. The diseased matter may be altogether arrested in the first series of glands, and the only result may be lymphangitis with its various results. In a puerperal woman, simple parametritis, with lymph-thrombosis and swelling of the retro-peritoneal lymphatic glands lying in front of the vertebral column, is the counterpart of specific lymphangitis affecting one of the extremities.

The peccant matter may, on the other hand, pass through one or more series of lymphatic glands, and at last reach the thoracic duct; and, being poured with the contents of this into the subclavian vein, may infect the blood.

In other cases, both surgical and puerperal, the peccant matter passes direct into the veins; and if thromboses be not formed in these, it circulates freely with the blood-current. As a rule, however, it may be said that thrombi form, or are already formed, in the veins, which obstruct the passage of the noxious matter, and perhaps keep it altogether from the blood, so that the effect is only a thrombosis or a phlebitis. If, however, the thrombus break up or portions of it become detached, the embola so formed are infected, and the action of the peccant matter is then seen in the form of pyemia.

All these more local affections, which may yet be fatal (*e.g.* specific peritonitis in a puerperal woman), may be

regarded as abortive pyemia; while a really completely developed pyemia is only produced when the peccant matter circulates freely with the blood. That the origin of the diseases mentioned depends on the same *materies morbi* as pyemia proper, is recognised by many. Any one of them may at any moment end in pyemia; and they occur in greater numbers in surgical wards or in lying-in hospitals, if puerperal fever or pyemia prevail at the same time.

[Two cases are then related to show that certain forms of lymphangitis and perimetritis represent a kind of abortive pyemia, in which it appears as if the process had been limited; but in the period of convalescence a sudden rigor appeared, and completely developed pyemia was developed, probably from the passage into the blood of peccant matter deposited in the lymphatic glands.]

When the deleterious matter has gained access to the blood-current through the lymphatics or veins, it will produce general infection with decomposition of the blood, together with toxic phenomena and changes of nutrition in the organs as the consequence of this, and also scattered circumscribed local destructions of tissue, as a result of direct action. In other words, pyemia or puerperal fever is developed. This is seen on the post-mortem table in four more or less pure forms, which, however, show during life but little difference in symptoms.

1. In one form, there is no special localization.

2. In another form of pyemia and puerperal fever, there is a disposition to localization in the serous membranes lined with epithelium, as also in the synovial membranes and in the endocardium, which anatomically may be classed with serous and synovial membranes.

3. In the third form of pyemia, the peccant matter appears to bear a special affinity for the mucous membranes, especially that of the digestive canal.

4. The last form in which pyemia and puerperal fever may be found on the post-mortem table is the most common: it is that called by Hueter multiple pyemia (*pyæmia multiplex*), and is characterized by the production of abscesses,

arising from the impaction of thrombi in the vessels in various situations.

[Dr. Heiberg describes these forms of pyemia in detail, and proceeds as follows :]

These four forms in which pyemia or puerperal fever is manifested are not always met with in a pure and isolated state. As a rule, there are not only the most various combinations of these forms, but also complications with simple traumatic and irritative processes, and with simple thrombi and embola ; the local effect being dependent on plugging of the vessels and not on infection.

Speaking of bacteria, Dr. Heiberg makes some remarks on the importance of distinguishing between them and granules of débris or fatty drops, and says :

In seeking to follow these micro-organisms in their various courses through the body, we have first to prove their existence in a surgical wound, in the uterine mucous membrane of a puerperal woman, or in a laceration of the perineum or vagina. It is especially the diphtheritic coating or epithelium that must be examined. This is, however, the most difficult part of the examination, inasmuch as the surfaces referred to are commonly covered with detritus. Although in most cases I have obtained only doubtful results in searching for bacteria-colonies, I have in two instances found distinct zooglea-masses, not only lying loose on the surface, but also penetrating into the tissue. This was specially the case in an uterine mucous membrane covered with diphtheritic exudation ; and it was remarkably evident in a semi-gangrenous laceration of the perineum in a puerperal woman, who died two days after delivery from puerperal fever with enlarged spleen and peritonitis. I also found this appearance in a man whose arm had been removed at the shoulder-joint seven days previously on account of sarcoma of the humerus, and in whom considerable hemorrhage from the circumflex artery took place the day before I made the examination. The wound was covered with a diphtheritic deposit ; the lungs contained disintegrated embola, principally formed of spheroidal bacteria, as well as several metastatic sarcomatous tumours with distinct embola of sarcomatous



tissue in the corresponding arterial twigs ; the spleen was somewhat swollen.

Following the bacteria further, I have on many occasions seen them in the larger lymph-spaces, as in the serous membrane of the liver in the case of a woman who died of puerperal fever. Here there was a lymphatic network completely injected with bacteria, which could be easily demonstrated by simply stripping off the peritoneal covering, spreading out the preparation on the object-glass, and treating it with potash. On endeavouring to press out the mass from the vessels, a large number of spheroidal bacteria arranged in a chaplet form were found. In preserving the preparation, a large number of leucin crystals were separated. In another puerperal case, the lymphatic network on the surface of the lungs was found injected in a similar manner, and the injected lymphatics passed onwards along the septa and the vessels and bronchial tubes in the substance of the lung itself. A similar case has been described by Orth.

I also found bacteria in the lymphatic glands, partly diffuse and lying in the lymph-cells, partly in the form of remarkably distinct zooglea-masses, especially in the lymph-sinuses of the retro-peritoneal glands, in a case of purulent peritonitis, where the disease was for a long time localized in the pelvis, finally developing itself, after a course of two months, into ordinary puerperal fever, probably in consequence of the bacteria then passing through the lymphatic glands. I have also had repeated opportunities of noticing bacteria in broken-down venous thrombi in the neighbourhood of wounds, or in the uterine walls in puerperal women.

When the bacteria have passed into the blood by one way or another, they may spread further ; and the heart is the place where we must first expect to find them. Here they may take root, and become further developed. They are comparatively rarely found in the right heart, though it is here that one would first seek them. In a case, however, described by E. Winge (Canstatt's *Jahresbericht*, 1870), there was quite an unusual development of them on the tricuspid valve, as well as in the right ventricle. Their

most frequent seat is then the left heart, where the whole of the endocardium, and especially the valves, are attacked, a malignant ulcerative endocarditis being developed. I have described two such cases of mycosis endocardii in Virchow's *Archiv*, Band lvi.; and a similar case has also been since mentioned by Eberth.

How far this ulcerative endocarditis depends on pyemic infection, cannot yet be determined. If we put aside the question of so-called spontaneous pyemia, it becomes necessary, on the one hand, always to prove the existence of an external lesion, and, on the other hand, to find ulcerative endocarditis, with its peculiar pyemic metastasis and swollen spleen, in valves that have hitherto been healthy, and have not suffered from rheumatism. This is the case as a rule, but in one instance I have had the opportunity of observing malignant ulcerative destruction (with concomitant enlargement of the spleen and multiple abscesses) in valves which presented thickening and retraction in consequence of rheumatism at some former period.

With regard to external lesions, we must not always expect to find the origin of the disease in the uterine mucous membrane, or in a large surgical wound; it may take its rise in a superficial or apparently unimportant injury, or in a bed-sore. Davaine has very appropriately compared the human body covered with its epidermis to a well-corked bottle, into which bacteria cannot enter. If the epidermis be abraded, the result is the same as when the cork is removed from the bottle. Some time ago, I made the post-mortem examination of a patient who had died with malignant endocarditis and distinct pyemic symptoms, where there was reason to assume that infection had taken place through extensive scratches on the lower limbs. [One of the segments of the mitral valve especially presented abundant deposits of bacteria; and Dr. Heiberg suggests that a cardiac valve affected in this way may become a further source of embola, formed of large and small bacteria-colonies.]

Following the bacteria in their further course, I have met with them in small abscesses of the lungs, in the form of masses of zooglea, in a puerperal woman, who died of mul-

tiple pyemia, and in whom were found an offensive puriform fluid, tinged with blood, along with a limited grey diphtheritic infiltration on the inner surface of the uterus, and broken-down thrombi in the venous plexus. As the preparation had not been hardened, it could not be ascertained how far the masses of zooglea lay in the small arterial twigs. In this case there was no affection of the right heart, and the embolon must have therefore come direct from the uterine veins.

Dr. Heiberg says:—I have repeatedly examined the yellow spots and miliary abscesses in the muscular substance of the heart. It is especially necessary to make a distinction between the small yellowish spots, which to the naked eye are more like parts that have undergone fatty degeneration, and abscesses proper. In the latter I have not found bacteria-colonies, while in the yellowish spots I have repeatedly seen distinct bacteria, partly in the form of zooglea, apparently lying free in the connective tissue, partly as closely-packed granules in the cells of the muscular tissue, the transverse striæ of which were obliterated and replaced by granules which presented, in most instances, the characters of bacteria. I also found bacteria in one case in large embolic thrombi in broken-down infarcts of the spleen. In the kidneys, especially after hardening in spirit, miliary abscesses are found both in the cortical portion and in the pyramids. Here the abscess surrounds an arterial twig plugged with spheroidal bacteria, as may be proved both by longitudinal and by transverse section. In some cases the purulent deposit lies within the urinary tubules, the stroma remaining almost normal; while, in other cases of metastatic renal abscess, the stroma also is affected. Masses of zooglea and chains of bacteria are also found in peritoneal and pleural exudations, sometimes constituting a more important portion than the pus-cells and fibrin.

Bacteria having thus been traced through the veins and the lymph-spaces to the heart, and thence into metastatic deposit in various organs and in the renal tubules, the question arises, what signification is to be attached to them?

In the little epidemic of pyemia and puerperal fever which forms the groundwork of this communication, it may be said that the presence of bacteria was proved in most cases ; and the cases may be placed side by side with those of pyemia described by Klebs as occurring in the Franco-German war. Besides this, similar observations have been recently made by Hueter, Waldeyer, von Recklinghausen, and others ; and if it be at the same time remembered, that Virchow probably had these masses of zooglea and bacteria before him when he spoke of diphtheritic masses in pyemia and puerperal fever, it cannot be denied that bacteria are very frequently found in cases of pyemia and puerperal fever.

It is not, however, as yet decided whether they are to be regarded as only accidental phenomena, or whether they really represent the peccant matter—if not directly, still, in any case, by producing a peculiar decomposition of the blood or of the tissue-juices with which they come into contact. Davaine's experiments appear to have clearly established the noxious influence of bacteria in *mal de rate* ; and the experiments of Klebs, Eberth, and others, with regard to pyemia, have made it probable, and I myself believe, that such cases as ulcerative endocarditis indicate that the bacteria have had a pretty active share in the process of disease. Nevertheless it would be premature to declare them to be the only causes of pyemia and puerperal fever, although perhaps future researches may make this more probable. According to Panum, putrid poison is not decomposed by boiling and steaming, and is soluble in water ; properties which cannot be ascribed to bacteria, even if we follow Rindfleisch, when he says that the boiling of water is not sufficient to destroy bacteria. Moreover, we sometimes meet with a well-marked case of multiple pyemia, where there is a closed abscess, without any external open lesion. I especially remember having made a post-mortem examination of a patient affected with disease of the vertebral articulations, and who had multiple pyemia. Although it is probable that healthy uninjured mucous membrane may serve as the point of invasion for bacteria, it cannot be denied that it



would be a forced explanation to assume that infection had taken place in this way in a man who had a large purulent focus, notwithstanding that his pyemia indeed occurred at a time when an epidemic of pyemia and puerperal fever prevailed.

[Dr. Heiberg merely notices, without discussing, the question whether bacteria are formed in the body or introduced from without, but he indicates that he is not in favour of the doctrine of spontaneous generation.] As to the specificity of bacteria, much may be said on both sides of the question. In favour of specificity, there is the epidemic occurrence of the disease; against it we have the sporadic cases and the limited epidemics of puerperal fever, which can sometimes be traced to bed-sores or to dissections by students.

How are bacteria—when they are present—transmitted to a puerperal woman or to a wounded person? Is it through the air, or directly by means of the finger, the syringe, or dirty surgical instruments? These are questions of the greatest importance to the practical surgeon and to the obstetrician. The observations made during the recent war have especially opened our eyes to the evils that may be caused by the employment of foul instruments, or of dressings that have already been used; and some have even recommended that all dressings, and even instruments, that have been employed in a pyemic case should be committed to the flames. Greater and less epidemics of puerperal fever have, too, been observed, in which each single case could be traced with tolerable certainty to one and the same midwife; or, again, of two midwives practising in the same town, one has been quite free from the disease, while the other can scarcely attend a single patient without infecting her. While it must be assumed that a large proportion of cases of pyemia and puerperal fever arise in this way, there are, on the other hand, single cases, and even epidemics, which, in spite of most careful research, cannot be traced to direct infection, and regarding which, for the present, we are obliged to assume that communication has taken place through the air.

We cannot assume, as a special source of bacteria, self-infection arising from decomposition of the peculiar dis-

charges of puerperal women. The bacteria must be introduced from without; but they may find a more or less favourable nidus for development in portions of placenta or decidual membrane that may be left in the uterus.

In all cases it is of the greatest importance to observe cleanliness, or to impede the entrance of bacteria by every possible means. A modification of Lister's treatment may, without doubt, be adopted by the obstetrician, who should not only take the greatest care that his fingers and instruments are clean, but should, both during and after labour, endeavour to prevent the entrance of air charged with bacteria. These precautions are, indeed, observed in many lying-in institutions.

I will next say a few words on so-called spontaneous pyemia, in which the disease occurs without any apparent external lesion. Taking into consideration the characters of the protoplasm, and of the ameboid cells which frequently wander from the mucous membrane among the epithelial cells, there is *à priori* no reason to suppose that the bacteria may not be taken up by the epithelium, and may thence penetrate into the mucous membrane and spread through the lymph-spaces. There are, indeed, certain forms of puerperal fever which favour the assumption of the introduction of infective material through sound mucous membrane. In lying-in institutions especially, cases are met with in which rigor appears before the completion of labour, or so closely after it that we must assume that infection has taken place through the manual examination made before birth. A case of this kind is among those on which this paper is founded. A woman, aged twenty-five, was admitted into the lying-in hospital on April 17th, and was confined at 10 A.M. on the 20th; but at 7 A.M. on the 19th she had had a rigor, and she died at 6 P.M. on the 21st, thirty-two hours after confinement. On post-mortem examination, there were found purulent edema of the subserous membrane of the uterus, puriform collections in the lymphatic vessels of the uterine walls, diffuse peritonitis, acute softening of both ovaries, with an abscess as large as a pea in the broken-down rudiments of the right ovary. The spleen was

enlarged. The inner wall of the uterus was coated with bloody fluid, and there was distinct diphtheritic infiltration at the point corresponding to the attachment of the placenta. There was a deep laceration of the perineum, with a dirty semi-sloughing base ; and in the right wall of the vagina was a large greyish-yellow congested lymphatic vessel, distinctly communicating by several roots with the laceration. On microscopic examination, bacteria-colonies were found in the wound and in the lymph-spaces, on the purulent edema around the uterus, and in the peritoneal exudation. Although the presence of bacteria in the perineal laceration and in the lymph-spaces of the vagina may point to infection from this source, still the occurrence of rigor before confinement proves an earlier infection, probably through the mucous membrane of the os and cervix uteri, the disease being then communicated to the subserous membrane and the peritoneum.

[The paper is concluded with some further remarks on the possibility of the introduction of bacteria through other mucous membranes ; among others, through that of the nose.]

---

## REPOSITION OF THE PROLAPSED FUNIS UMBILICALIS.

By J. BRAXTON HICKS, M.D., F.R.S.,

Physician-Accoucheur to, and Lecturer on, Midwifery and the Diseases of  
Women and Children at Guy's Hospital, &c.

DR. BRUNTON has, in your last issue, in a very practical paper on "Replacement of the Funis Umbilicalis," given a just preference for the postural mode of treatment, as compared with the various plans he has enumerated. It is evident he has inadvertently overlooked a plan which I described in the paper I read many years since at the Obstetrical Society, on "Combined External and Internal Version," as having put in successful practice. The inclusion of this description in another paper, has, I think, caused it to be generally overlooked. Knowing that it only requires trial to make it appreciated, I take this opportunity of calling the

attention of your readers to it. Those who have tried it have, like myself, found it an easy and successful plan.

The patient being on the left side in the usual obstetric position, one hand is placed over the lower abdomen, the other is introduced into the vagina. The funis is then to be passed within the os, and the head being lifted up, it is to be passed by its side, and then above it. The external hand should have already during the act made out the position of the head, and now should press it downward towards the os. That it really does so the internal hand will readily tell. While the head is pressed down from without, the internal hand is withdrawn slowly, taking care that the funis does not follow. The external hand continues its pressure over the period of two or three pains; when the uterus has fairly settled round the head the patient may be left, for the funis is effectually prevented from again being prolapsed. Thus it will be seen that the internal hand does the work of gravitation, both in the first step, as also in the second of the operation, and to my mind it is more reliable and definite. The only difference in favour of postural treatment is the gravitating upwards of the cord; but as far as my experience goes, the pushing up of the head effectually permits the restoration of the funis into the uterine cavity. The main obstacles to this restoration in cases of prolapsus of the cord are a very rigid os, and a uterus firmly set round the child. To restore the funis under these conditions is manifestly dangerous to the life of the child. It is evident, however, that it is not to these cases Dr. Brunton refers, although he does not mention them, for it is clear that postural treatment cannot act under these conditions. The postural treatment is excellent, but I think the plan above described will be found to have all the advantages without the drawbacks attending the awkwardness and disturbing nature of the position referred to.



## PROLAPSUS OF THE FUNIS UMBILICALIS.

By PATRICK JAMIESON, M.A., L.R.C.S.E.

IN your issue of last month, Dr. Brunton strongly advocates the adoption of the "postural" treatment in prolapsed funis. He describes, fairly enough, our obstetric resources in this emergency previous to the introduction of the postural method, and that these resources were so limited, causing much anxiety on the part of the attendant, no one will deny.

There seems to be an impression, that the value of the postural method in cephalic presentations is not very well known amongst the profession generally, or if so, its merits are not sufficiently recognised. Should such be the case, the sooner our experience of this resource, in an emergency so fatal to the life of the infant, is laid before the profession the better.

In the following cases which I narrate, the benefit accruing from early interference is demonstrated, for I demur very strongly to the axiom laid down, that it is wise and prudent to let alone the funis when the membranes are unruptured. If any considerable portion of the cord be felt at the os uteri and bulging the membranes into the vagina, it is very unlikely it will slip aside *in utero* and allow the cranium to take up its position in front of it. On the contrary, we should expect when the membranes are ruptured a larger amount of funis to protrude; while on the other hand, the reduction of the prolapse is much more easy of accomplishment when aided by the support which the unruptured membranes afford.

On 24th November, 1867, I was called to attend a primipara, who, I was informed, had been in labour for twelve hours. The indications of labour being very slight, I was doubtful of proposing a digital examination, but having found the value of knowing the exact state of parts at an early stage, fortunately in this case I overcame my hesitation. The os uteri was dilated to the size of a florin: the membranes were entire; the presentation was natural, but com-

plex in as far as a large knuckle of cord filled and protruded from the os—probably four inches of it, doubled up as indicated. The fetal head pressed upon the funis, which pulsed but feebly during the pains.

At once I adopted the postural treatment.

Placing the woman on her knees and elbows, the latter carried outwards, the forearms flexed with the palms of the hands resting on the subclavicular spaces, the side of the head then rested naturally on the pillow. Gently insinuating my hand into the vagina, and gathering the cord in my fingers, a process greatly aided by the unruptured membranes, I was successful, in the interval of a pain, in causing the funis to return within the uterine cavity. To prevent a recurrence of the prolapse, I maintained my hand *in situ* during a few pains, and at each uterine contraction felt the head press against the slightly dilated os without any attempt of the cord to protrude or intervene. I withdrew my hand and, directing the woman to maintain her position till my return, left her for two hours.

At the expiry of that time, I found the os slowly dilating and no protrusion of the cord, therefore I allowed my patient to get out of bed and walk about.

The first stage of labour was not completed till twelve hours after that time, and in three hours more the labour terminated in the birth of a living and healthy child, the funis being of the ordinary length.

In this case there is little doubt the child would have been lost had not the accident been early discovered, and the postural method adopted. Long before version could have been attempted, the pressure of the fetal cranium would have arrested circulation in the funis, since even already the pulsation had become feeble.

On the 13th December, 1867, I was called to a pluripara. I found the first stage of labour almost completed. The membranes had been ruptured, and about six inches of the cord lay prolapsed in the vagina, pulseless and flaccid. Almost hopeless as, under these circumstances, it was to expect life in the fetus, I resolved, since the pains were strong, to give it at least a chance.

Adopting the postural treatment, the prolapsed cord was easily reduced at the first attempt, and the labour was completed within half an hour; but the child—a well-developed male—was dead.

Subsequently, this woman again became pregnant, and I impressed upon her the propriety, nay, necessity of giving intimation at the very commencement of her labour.

This was done, but the pains had come on suddenly and with considerable violence. When first seen the os uteri was dilated to the size of rather more than half-a-crown; the membranes had been ruptured; the vertex presented, and there was prolapse of the cord to the extent of four inches—the cord was pulsating.

Postural treatment was at once adopted: the protrusion was returned with considerable difficulty, but was retained within the uterus. An hour subsequently, the labour terminated in the birth of a living child—healthy and vigorous.

## *Reports of Hospital Practice.*

### ST. MARY'S HOSPITAL, MANCHESTER.

#### RETROVERSION OF THE GRAVID UTERUS—RETENTION OF URINE—PYEMIA—RECOVERY.

*Under the care of* MR. CULLINGWORTH.

MARY B., aged thirty, a married woman, was admitted February 12th, 1875, having been sent in by a surgeon, who supposed she was in labour. She had already been pregnant five times previously, and had given birth to four children at term. The last menstruation occurred two months before Christmas last, so that she had probably gone a little beyond the third month of pregnancy. She dates her present illness from the first week of the new year, when she began for the first time to have pain during micturition. From that time she says her abdomen was unusually large, and continually

increasing, and she was perpetually harassed by a desire to pass water, though only a very few drops would pass at one time. On the 9th of February (three days before admission), she passed bloody urine, and continued to do so to the time of her admission.

When received into the hospital, she was in very great suffering, and presented the series of symptoms often classed together under the name of typhoid symptoms; the tongue was dry and brown, thirst intense, pulse small, weak, and rapid. The abdomen was enormously distended, the distension reaching quite to the ensiform cartilage; bloody urine was dribbling from the urethra. The patient had a large protrusion of piles. The vulva and vagina were inflamed and tender, and a distinct tumour (the body of the uterus), smooth and rounded, could be felt between the rectum and vagina.

A gum-elastic male catheter was passed, and 124 fluid ounces of very bloody urine were withdrawn, the flow lasting an hour and three minutes. The abdominal distension disappeared, and the patient felt such relief as she had been quite a stranger to during the past five weeks. After evacuating the bladder, a vaginal examination was again made, and the retroversion distinctly felt.

*February 13th.*—Urine still contains so much blood that it becomes perfectly solid on the application of heat; sp. gr. 1022. The nurse directed to continue the use of the catheter every two hours, and the following mixture ordered:—  
℞ Acid. gallici, gr. 80; liq. opii sed. ℥ 40; aq. cinnamomi, ℥ viij; capt. ℥ j qq. quartâ horâ.

*14th.*—Urine less bloody, so that it no longer becomes solid on boiling; sp. gr. 1010. Tongue cleaning; pulse 120; temperature 98°.

*15th.*—Urine purulent and highly offensive; sp. gr. 1010. Tongue clean; pulse 110. Uterus found to have regained its normal position; rectum loaded with scybala. Ordered to discontinue the mixture, and to take freely a decoction of *triticum repens*; simple enema, and catheterization at night only.



16th.—Much better, asking for a meat diet.

18th.—Shivered a little yesterday, and complains this morning of being very thirsty; looks anxious and ill; pulse 120; temperature (11.0 A.M.)  $102^{\circ}4$ ; vomiting. There is a broad central strip of the tongue which is dry and red. Ordered an effervescing mixture.

19th.—Very irritable and petulant; complains of much thirst and nausea; pulse, 130; temperature (11.30 A.M.),  $101^{\circ}8$ ; tongue dry and brown down centre.

20th.—Slept well; expression still painful; yellowish appearance of skin and *conjunctivæ*; general urinous odour; pulse, 120; temperature,  $101^{\circ}5$ ; tongue less dry.

22nd.—General aspect more satisfactory; yellowness persistent; urine clear, of sp. gr. 1007, and containing a very small quantity of albumen; no vomiting; thirst diminished; pulse, 104; temperature,  $100^{\circ}8$ ; tongue still rather dry down its centre.

25th.—Urine pale and abundant (56 ounces in 24 hours); sp. gr. 1007, with a trace of albumen; pulse 84; temperature (taken separately in each axilla),  $97^{\circ}2$ ; tongue in same condition. Ordered a mixture containing potassium chlorate with acid.

27th.—Catheterism discontinued.

March 1st.—Urine again slightly purulent and offensive; pulse, 84; temperature *in recto*,  $98^{\circ}4$ . Ordered catheterization once daily.

6th.—Urine clear; tongue of good colour, with still a little tendency to central dryness; appetite good. Ordered use of catheter to be discontinued, and to be out of bed for a little time daily.

13th.—Dismissed cured.

April 19th.—Attended the out-patient room, looking stout and well.

---

RETROVERSION OF GRAVID UTERUS—RETENTION  
OF URINE—RECOVERY.

*Under the care of MR. HESLOP, and reported, with his  
permission, by MR. CULLINGWORTH.*

ELLEN K., aged thirty-three, a married woman, was admitted February 17th, 1875. She has had five previous pregnancies, three of which lasted the full term ; and she last ceased to menstruate on the 1st November, 1874, whence it follows that she is most likely in the fourth month of pregnancy.

Twelve days ago she found that the urine was not passing naturally ; the desire was constant, and the flow not continuous, but in little gushes or spurts. After three days, namely, on the 9th February, she began to suffer a good deal of pain, and to become much distended. On the 11th February, a medical man saw her, and drew off two pints of urine. He called once daily from that time to pass the catheter.

On being admitted at 4 P.M. on the 17th, she was in considerable suffering. The urine had been withdrawn at 9 P.M. on the previous day, and again in small quantity at 11 A.M. on the morning of the day of her removal to the hospital. Vaginal examination revealed a retroversion of the uterus. An elastic male catheter was passed into the bladder, and thirty ounces of strongly smelling urine drawn off, without sediment, and free from the presence of blood.

18th.—The patient was again catheterized at ten o'clock last evening, and since that has passed urine voluntarily. This morning the position of the uterus is normal. Ordered an enema, and catheterization twice daily.

19th.—Urine free from odour ; appetite good ; tongue clean.

25th.—Has suffered occasionally a little pain in the abdomen. Uterus retains its normal inclination, and can scarcely be reached *per vaginam*. Appetite good. No further catheterization.

March 13th.—Dismissed cured.

## General Correspondence.

---

### DR. CHADWICK'S CASE OF TRANSFUSION.

*To the Editor of the Obstetrical Journal.*

HAVING read in the last number of your Journal, p. 66, a case of immediate transfusion, by Dr. Chadwick, of Boston, United States of America, in which it is stated that the bulb of Dr. Aveling's apparatus, supplied by us, contained four instead of two drachms, will you allow us to correct a misconception of the meaning in our printed directions which we supply with the apparatus? We do not state therein that the bulb holds two drachms, but that squeezing the bulb forces two drachms of water into the afferent vein. That our statement is correct, may be proved by a few experiments carried out according to the printed directions. We have never altered the size of the bulb since we made the first instrument for Dr. Aveling, and all of them being made in the same mould, there cannot be a material difference in their capacity, except a few minims caused by a slightly varying thickness of the rubber. Although the bulb holds four drachms, when compressed as directed only two drachms are displaced. We beg to remind the operator of the absolute necessity of testing for himself the instrument he intends to use and to measure the amount he expels with each compression of the bulb. Various factors combine to produce different results. Besides the varying thickness of the rubber, the manner of holding the bulb when squeezing it, and the size of the tips of the fingers and thumb of the operator, may cause a more or less quantity of fluid to be expelled for which the instrument maker should not be held responsible.

We are, Sir, yours, &c.,

KROHNE AND SESEMANN.

---

---

# THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

MAY, 1875.

---

## CRIMINAL RESPONSIBILITY IN PUERPERAL FEVER.

A SHORT time since we drew the attention of our readers to the case of Elizabeth Ingram, the Coventry midwife who, on the charge of manslaughter, was committed to take her trial at the Warwick Assizes. Having then reviewed the evidence upon which the charge was founded, it only remains now to add that the prisoner was acquitted on the plea of her having attended the patient with the belief that she had the sanction of a medical man in doing so. Since this case another very similar, but of still greater interest, has occurred. It may be thus briefly related. At the Salford Hundred Assizes, before Mr. Justice Field, and a special jury, Elizabeth Marsden was indicted, that on the 16th February last, in the borough of Salford, she did feloniously kill and slay one Mary Ellen Goodier, and that, on the 19th February last, in the borough of Salford, she did feloniously kill and slay one Ann Mills. To both indictments the prisoner pleaded "Not Guilty." From the evidence it appears, that on the 17th January the prisoner attended Catherine Carroll, who died from puerperal fever about the 23rd of the same month. Mr. Orchard, a surgeon, told Elizabeth Marsden that her patient had died of puerperal fever, and warned her not to attend any other case for two or three months. He also informed her that he had been called in to see two or three other cases of puerperal fever in which she had acted as midwife. On February 11th, Mr. White, surgeon, attended Mary Ellen Goodier in puerperal



fever. She too had been attended by the prisoner, and he also advised the midwife to burn her clothes, and discontinue practice for a month. After this, Elizabeth Marsden attended Ann Mills in her confinement, who also had puerperal fever, and died on the 19th February. At the conclusion of the trial, which lasted nearly five hours, the jury found the prisoner guilty, and the judge approving their verdict, addressed her in the following words:—

“ You appear to have been a respectable woman, but it is a very serious thing to cause death in this way, and it is a matter which the law cannot pass over, because death was caused by negligence after, what I am bound to say, was a very plain warning, which I think you ought to have attended to. Under the circumstances, I shall pass as light a sentence as I dare to do, which is imprisonment for six months.”

It is scarcely possible to exaggerate the importance of this case, for the law which applies to the midwife must in like manner affect the obstetrician. It is easy to imagine a case in which the law meted out to the midwife might be enforced with great injustice. Suppose A, a medical man, has a case of puerperal fever in his practice which proves fatal. B, another medical man, also sees the case, and warns A not to attend another midwifery patient for two months. A believes by changing his clothes, and using other precautions, he can so disinfect himself as to render any chance of his communicating the disease impossible, and with this belief he attends another woman in her confinement. This patient also dies of puerperal fever. The fever, however, has not been communicated by him, but is autogenetic in its origin. What is to prevent A from being indicted for manslaughter, and imprisoned six months? Everybody must now admit the possibility of such a case as this occurring. A, however certain he might feel of the fact, could not prove the autogenetic character of his second case, popular opinion would certainly be against him, and a jury would probably give a verdict in accordance with it. The case of Elizabeth Marsden is one which our Obstetrical societies might profitably discuss and pronounce opinions upon. It is not a little remarkable that at this time, when Medicine is

earnestly inquiring what puerperal fever really is, and shrinking from speaking positively about it, Law is able to decide dogmatically all disputed points, and award without hesitation the amount of punishment incurred. Considering the present imperfect state of our knowledge, and the still greater deficiency of the midwife's information, we think the ends of justice would have been sufficiently attained had Elizabeth Marsden been solemnly warned by the Judge. She had received, as in the case of Elizabeth Ingram, no official notice from the Coroner, and she surely might fairly be granted discretional power in estimating the value and force of warnings proceeding from private practitioners. If eventually it be decided that midwives shall be liable to be prohibited under certain circumstances from practising for a given time (and we think this would be a salutary arrangement), the power should not be given to any one holding a medical diploma, but should be vested in the Coroner, Health Officer, or some other public functionary. The liability incurred in carrying disease from one patient to another is a very wide medical question. Should the State take up this subject in relation to the influence which medical men may have in conveying contagion, and undertake to decide, *e.g.*, how long and under what conditions a physician who has seen a case of small-pox shall remain in quarantine before seeing another patient, it will, we think, prove the most difficult task it has ever undertaken, involving the making of laws, for the just construction of which no sufficient scientific data exist, and necessitating compensations from the Imperial funds, or other costly arrangements which would scarcely be appreciated by the general public. As far as puerperal fever is concerned, we look forward to seeing its propagation prevented and its isolation insured more by the extension of obstetric knowledge than by the enactment of restrictions; at the same time public safety demands that the power should exist of staying the ignorant and wilful death-dealing course of a contagion-spreading practitioner.

---

## Notices and Reviews of Books.

---

*Contributions to the Mechanism of Natural and Morbid Parturition, including that of Placenta Prævia, with an Appendix.* By J. MATTHEWS DUNCAN, President of the Obstetrical Society. Edinburgh, A. and C. Black, 1875. Pp. 468.

ALL earnest students in Obstetrics will be pleased to have the opportunity of possessing, in a collected form, the numerous excellent essays which Dr. Duncan has, from time to time, published on the Mechanism of Parturition. They form a portly volume of twenty-five chapters, with an Appendix, containing the Address in Obstetric Medicine delivered at the Norwich Meeting of the Medical Association in August (not February, as in the note), 1874. We cannot too strongly recommend those of our readers, who are not already acquainted with these Contributions, to avail themselves as early as possible of the laborious experiments and statistics, so full of practical suggestions, which Dr. Duncan has presented to Obstetricians. The volume, besides explanatory woodcuts, contains three coloured lithographs, illustrating the separation and expulsion of the placenta.

---

*Clinical Lectures on Diseases peculiar to Women.* By LOMBE ATTHILL, M.D. University, Dublin. Third Edition. revised and enlarged. Dublin, Fannin and Co., 1875, Pp. 294.

This useful little volume has very deservedly now arrived at a third edition, and its author has not lost the opportunity afforded him of correcting the errors and supplying the omissions of former issues. As a handy text-book of the diseases of women for general practitioners, it has neither peer nor rival. Its information is trustworthy and practical, and written in a style terse, unaffected, and intelligible.

*Compendium of Children's Diseases; A Handbook for Practitioners and Students.* By Dr. JOHANN STEINER. Translated from the second German Edition, by LAWSON TAIT, F.R.C.S. London. J. and A. Churchill, 1874. Pp. 408.

Mr. Lawson Tait has done good service to the English Medical public by giving them, in their own language, Dr. Steiner's work on the diseases of children, which has obtained such well-merited popularity in Germany. The translation is easy and accurate, and will doubtless find favour not only with those practitioners who make Pediatrics their peculiar study, but also with all who have (and who has not ?) to treat infantile disorders.

---

## Abstracts of Societies' Proceedings.

---

### OBSTETRICAL SOCIETY OF LONDON.

*Meeting, April 7th, 1875.*

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., *President,*  
*in the Chair.*

*On the Relation of Puerperal Fever to the Infective Diseases  
and Pyemia.*

Mr. T. SPENCER WELLS opened the subject with the following address:—In opening a discussion this evening on the relation of puerperal fever to the infective diseases which are among the most numerous class of cases treated by the physician and general practitioner, and to the varied forms of traumatic fever which, under the head of pyemia, fall more frequently under the care of the operating surgeon than of the obstetrician, I wish particularly to impress upon all who honour me by their attention, that my object is rather to elicit than to impart information; to call forth the stores of knowledge now latent in the Society, to ask for the results of your observation, rather than to attempt to add to the knowledge of the Fellows by any contribution of my own.

It has been the custom of the Society in whose room we meet to stimulate the Fellows to enrich the *Transactions* by papers of original research and sterling value; to make this one of the chief objects of the Society; and, if not directly to discourage, certainly not to encourage, full or exhaustive discussions at the meetings. I have



long felt, and have publicly expressed my conviction, that this course might be amended with great advantage to all medical societies, and to the advancement of medical science and practice; and I had some small share, in the earliest of the preliminary meetings of this Society—I may say even before the Society was constituted—in determining that in our *Transactions* there should be a permanent record not only of the papers read before the Society, but of the discussions to which the papers gave rise. This custom has ever since been honoured in the observance, and the example is now being followed by other societies. If I am not greatly mistaken, the discussion on Pyemia in the seventh volume of the *Transactions of the Clinical Society* will be referred to hereafter with more interest and profit than any of the papers in that or in any of the earlier volumes of that Society's *Transactions*; and the discussion on Cancer in the twenty-fifth volume of the *Transactions of the Pathological Society* was most certainly as great an addition to the value of that volume as one could well imagine, until last night many of us heard the opening address of Dr. Bastian on a subject closely allied to some of the questions which I have to bring before you this evening, and the admirable speech of Dr. Sanderson in commencing the discussion, both full of deep thought, and eminently characteristic of the tendencies of our age and our nation to reject any theory which is not supported by facts, and to bring the highest developments of science to our help in the needs of daily life. These discussions lead to the almost painful reflection, how greatly would the value of the earlier volumes of the *Medico-Chirurgical Transactions* have been increased, if we had now not only a copy of the papers read, but a report of the comments they called forth from Baillie and Halford; from Cline, Cooper, and Abernethy; from Travers, Brodie, and Lawrence; from Bright and Addison. The marble busts of these great men of the past now surround us; but the workers of the present and the future can find no record of what they have said here, and can only regret that, while their form and features are preserved by the sculptor, their thoughts have not been embalmed by the reporter and the printer.

“Who of us can tell  
What he had been, had Cadmus never taught  
To man the magic that embalms the thought?”

As I have reason to believe that the discussion this evening will be fully and accurately reported, and will be preserved in our *Transactions*, I must now ask you to pardon these few prefatory remarks, and also to forgive me if I venture to express the hope that, as the subject of puerperal fever is not of less interest than that of cancer, is closely allied with pyemia and with the relation of bacteria to contagious and infectious diseases, it will be debated with as much ability and as complete freedom from any other than purely scientific and truthful feeling, as have characterized the discussions at the Clinical and Pathological Societies.

If you permit me to consider what I have so far said as introductory, and to commence the subject of puerperal fever now, I hope I shall not encroach upon your time beyond the fifteen minutes within which limit I have tried to condense what I wish to say.

In order that some definite direction might be taken in this inquiry, attention has been already publicly requested to six leading questions. The first, second, and third are so closely allied, that I will, if you please, read them together now.

1. Is there any form of continued fever, communicated by contagion or infection, and occurring in connexion with childbirth, which is distinctly caused by a special morbid poison, and as definite in its progress and the local lesions associated with it as typhus or typhoid, scarlet fever, measles, or small-pox?

2. May all forms of puerperal fever be referred to attacks of some infective continued fever—as scarlet fever or measles—occurring in connexion with childbirth, on the one hand; or, on the other, to some form of surgical fever, or to erysipelas, caused by or associated with changes in the uterus and neighbouring parts following the process of childbirth?

3. If all cases of contagious and infectious diseases which occur under other conditions than that of childbirth are set aside, does there remain any such disease as puerperal fever?

In framing these questions, after searching for an accurate definition of the term puerperal fever, or for some short description of this as distinguished from other forms of continued fever, I have taken, as the most accurate and comprehensive, the definition from the Nomenclature of Diseases, drawn up by a Committee appointed by the London College of Physicians (for which definition I believe the Committee are indebted to Dr. Arthur Farre). It is this: “A continued fever, communicable by contagion, occurring in connexion with childbirth, and often associated with extensive local lesions, especially of the uterine system.” To this definition this very important *note* is added: “In returning cases of puerperal fever, the more important local lesions, such as peritonitis, effusions into serous and synovial cavities, phlebitis, and diffuse suppuration, should be specified.”

Here, then, we are led, on the authority of the most distinguished obstetric teacher of his day, supported by a committee appointed by the Royal College of Physicians of London, to the conclusion that in puerperal fever we have a contagious continued fever *often* associated with the important local lesions just enumerated; not *always*, but *often*. You may then, according to this definition, have this contagious fever without these local lesions. The poison may be so potent, or the dose so large, that it may kill before there is time for the development of the local lesion; or the dose may be so small, or the poison so feeble, that it only produces some transient elevation of temperature, some greater rate in pulse and respiration, some increased action of skin, kidneys, and bowels, and

the morbid material is eliminated before any local lesion is established. But I must ask you to say if, in your experience, you ever saw such a case which could not, on careful inquiry, be traced to exposure of the patient to some one or other of the contagious or infectious fevers—to scarlet fever or diphtheria—to measles or small-pox? I need not remind you how these diseases are intensified or modified by the puerperal condition; and I proceed to ask if, in any case where puerperal fever could not be proved to be really scarlet fever, diphtheria, measles, or small-pox occurring in connexion with childbirth, it was not a traumatic or surgical fever, erysipelas, pyemia, or septicemia; the local lesions associated with the fever assuming rather a primary than a secondary importance in the chain of sequence. Time does not permit me to enter on the very important practical question as to the sole dependence of erysipelas upon a specific morbid poison. Whether this peculiar inflammation of the skin and cellular tissue, tending to spread indefinitely, and preceded or accompanied by fever, can arise from the spontaneous generation in the human body of a poison communicable by contagion; or, whether a poison capable of producing erysipelas exists at all times in varying quantity somewhere, ready to increase and multiply under favourable conditions; or, whether (as some believe) erysipelas may arise independently of any poisonous influence from without, we need not inquire now. The important fact for us now is, that erysipelas often attacks the parts concerned in the process of childbirth, and that the fever which accompanies it, intensified by the puerperal condition, is a very fatal form of one of the diseases confounded together under the term puerperal fever. Set aside the infectious fevers and erysipelas occurring in connexion with childbirth; and then we come to the local lesions associated with puerperal fever, and we ask what relations this fever and the local lesions bear to each other. Bruises or tears of the genital canal or perineum; inflammation, and the production of pyrogenic liquids or solids, which may contain both bacteria and some poisonous material or particles which have the power of impregnation; diphtheritic exudation on the mucous membrane of the uterus and vagina, especially on the place of the separated placenta, and plugging of the lymphatic vessels with granular masses or colonies of bacteria or spheroids; the rapid development and growth of plant-life, and the disturbance of function and alteration of structure which must follow; does all this arise under perfect sanitary conditions, spontaneously, or from mere chemical decomposition, or only when some poisonous agent is introduced from without—the seed of some plant sown in a fruitful soil?

Again, supposing inflammation is set up in the uterus, in its veins and lymphatics; that the albuminoid secretion known as the lochial discharge contains pus; that pus, or putrid material, or organic germs, are found in the lymphatic vessels of the uterus and the subperitoneal cellular tissue, with diffuse peritonitis; that the blood in the

uterine veins clots, softens, breaks up, is the seat of chemical and vital change, is detained in or near the pelvis, or is carried away to distant parts, or alters the composition and properties of all the blood in the body; that we have purulent infection, or pyemia—putrid infection, or septicemia; can all this arise in a healthy woman, placed in favourable conditions, if she be not exposed to some morbid poison? Is puerperal fever ever a simple traumatic fever modified by this puerperal condition, or does it always and necessarily depend on the action of a morbid poison? Or let me put the question in another form. Did you ever see a case of puerperal fever which was not really either a case of scarlet fever or measles, or some such infectious or contagious fever, or erysipelas? or a traumatic fever caused by the bruising or tearing of the parts concerned in childbirth, and the changes in the blood-vessels, blood, and lymphatics, following the injury? If you have seen such a case, then let us know something about the fever, its period of latency, its course and duration, and its termination; and especially tell us something about the poison which has been the cause of the fever.

Time does not allow me to do more than put the question. I await your reply, and pass on to the fourth, fifth, and sixth heads of our subject, which I will also read together. They are these.

4. Assuming that a form of continued fever—communicable by inoculation, contagion, or infection—does frequently occur in connexion with childbirth, how can its spread in private and in hospital practice be most certainly prevented or checked?

5. What relation have bacteria and allied organic forms to the pyemic process in the puerperal state?

6. What is the value of antiseptics in the prevention and treatment of puerperal fever?

And, to show the close connexion of these with the previous questions, I must remind you that even those who admit that a tear of the perineum, or a bruise of the vulva, by leading to inflammatory exudations around the vagina or uterus, or to clotting of blood in the veins, and consequent changes in the whole of the blood in the body, is quite sufficient to account for fever, which, aggravated by the puerperal condition, may lead to all the local lesions specified in the note to the official definition of puerperal fever—even those believers in the spontaneous or local origin of the fever, seeing the hundreds of cases where the injury is observed without the fever for every one where the fever is observed, and the prevalence of the fever in certain seasons and districts, and in the practice of certain surgeons or midwives,—still freely admit that it is only under some endemic or epidemic condition, or as a result of contagion or infection, that the ordinary wounds, or bruises, or tears, inevitable during the process of parturition, and commonly free from any serious consequence, become in exceptional cases so deadly.

It is especially in this direction that the knowledge of our country Fellows may be of the greatest value to us. In the practice of a large



hospital, or in private practice in a large city or a thickly populated district, it is impossible to say that a patient may not have been exposed to some contagious or infectious disease. But, when isolated cases arise in private practice in the country, where any source of poisoning can be readily traced, the accurate record of such cases, their origin and course, and their arrest or extension, may be of incalculable value. A country surgeon attends a man who has erysipelas after a broken arm. He also attends a healthy woman in an isolated cottage in a natural labour. There is no puerperal fever in the district, yet this woman dies of puerperal fever, and so do others attended about the same time by the same surgeon. These women, in all probability, would have recovered in the ordinary course, had not erysipelas occurred in the man who broke his arm. Such a history as this would have tenfold weight, as being free from numerous sources of fallacy and doubt attending any similar history in a large city.

And, again, we cannot consider the mode of preventing the spread of puerperal fever without examining into the part which bacteria and other organic forms may play either as poisons or as carriers of poison. Here I must ask you to allow me to refer to a paper which I read twelve years ago at Cambridge, on the Causes of Excessive Mortality after Surgical Operations. It may be found in the second volume of the *Medical Times and Gazette* for 1864. In that paper, I gave some account of Pasteur's researches on fermentation, on the organized corpuscles in the air, on spontaneous generation, and on putrefaction; sketching rapidly the results of some of his researches, to show the influence of the germs of the lowest organisms present in the atmosphere, especially bacteria and vibrios, upon animal bodies in health and disease, and on our tissues during life and after death, and especially upon the development of epidemic and contagious diseases. I also showed, from the observations of Angus Smith, Chalvet, Eiselt, and Réveil, that germs may be often found in the air of crowded rooms and hospital-wards, which only require favourable conditions for their rapid development.

Now that the influence of bacteria is beginning to assume a more general importance in pathological investigation, I must ask you to listen to the account published twelve years ago, of what I even then thought Pasteur's discoveries were leading us to:—"Carrying on the analogy between puerperal fever and purulent infection in the various forms which contribute so large a share to the excessive mortality after surgical operations, and applying the knowledge for which we are indebted to Pasteur of the presence in the atmosphere of organic germs which will grow, develop, and multiply, under favourable conditions, it is easy to understand that some germs find their most appropriate nutriment in the secretions from wounds, or in pus, and that they so modify it as to convert it into a poison when absorbed; or that the germs after development, multiplication, and death may form a putrid infecting matter; or that they may enter the blood and

develop themselves, effecting in the process deadly changes in the circulating fluid. That these low forms of animal life may seriously affect the blood of the higher orders of animals is clearly proved by the recent researches of Davaine, who has furnished us with the first well established example of a disease of the blood due to the presence of inferior beings which are capable of development and multiplication in the torrent of the circulation. These creatures (bacteria) differ from the whole class of infusoria which form in putrefied matter, as they disappear completely as soon as putrefaction of the blood commences. The bacteria are rapid consumers of oxygen, and when they exist in the blood they absorb the greater portion of the oxygen furnished by respiration, and thus hinder the combustion of all the effete and used-up substances which ought to be eliminated from the body." I may also say that, in the same paper, I gave some account of the relation of bacteria to splenic apoplexy in sheep; and referred to demonstrations of the presence of living germs in the air capable of reproducing contagious diseases, by Lemaire finding the achorion in the air which had passed over a scalp affected with favus, and by the experiments of Kennedy and Salisbury on the production of measles by the inoculation or inhalation of fungi given off from mouldy straw or linseed-meal. And then, in concluding that paper, after recording proofs—1. That the injection of a certain quantity of pus into the blood produces pyemia and affections characterized by multiple abscesses; 2. That the injection of putrid matter produces septicemia, or putrid infection, characterized by the symptoms of typhoid gastro-enteritis; 3. That the injection into the blood of the exudative materials in contagious diseases, as in glanders, produces the general contagious affections; and that, in all these cases, the introduction of the foreign substance or poison into the blood must be regarded as the origin of the disease—I went on to give some account of Polli's antiseptic treatment of these various forms of disease by sulphurous acid and the alkaline and earthy sulphites, showing how they arrest or prevent fermentation and putrefaction, and how it might not only be expected that the living fluids and tissues charged with the sulphites would resist the action of morbid poisons, and that there were proofs that when the sulphites had been taken, they really altered the actions of pus upon the blood of a living animal, as well as that of putrid matters injected into the blood, and that of a virus distinctly contagious and not putrid.

This was in 1864. It was two years afterwards—towards the end of 1866—that Lister began to treat cases antiseptically in the Glasgow Infirmary, using carbolic acid rather than sulphurous acid or the sulphites, but with the express purpose of destroying the organic germs present in the air, or in any of the liquids or solid substances about the patient; or of protecting any wounded or injured part from the contact or development of the germs. How he has gone on gradually perfecting the details of the antiseptic system, I

need not describe to you ; and the results are too well known to require more than the most passing allusion to the prevention of surgical fever, of pyemia and septicemia, the checking the spread of erysipelas after its importation into a ward, the lessening of mortality after both the greater and the more trifling operations, the saving of limbs after compound fractures, the healing of large burns, ulcers, or abscesses, and the general freedom of hospital wards from noxious odours and matters, and from the introduction of poison from the dead-house or dissecting-room, while the hospital atmosphere is not only purified for the patients, but for the surgeons and nurses.

If all this have been gained since 1866 in Glasgow and Edinburgh, and in other British and foreign hospitals, where the example has been more or less closely followed ; if traumatic fever and pyemia can be kept out of a surgical hospital, why should not puerperal fever be kept out of a lying-in hospital, or be prevented from spreading, if it have been accidentally imported ? There has been a great outcry against lying-in hospitals of late ; but I trust this Society may be able to guide the feeling rather in the direction of freeing them from puerperal fever than of destroying them. What has been done by reducing the size of a surgical hospital in Edinburgh and in London, the size of the wards, the number of beds in each ward, the number of attendants, and enforcing the utmost care in protecting the patient from any contagious or infectious influence, and in securing the greatest possible cleanliness in all things about her ; in reducing the mortality after one of the most serious operations to which a human body can be subjected and still live,—is known to those who have watched the progress of ovariectomy. It is for you to say whether a similar enforcement of obedience to the laws of sanitary science, a more exact observation of the origin and progress of the many different conditions which have been classed together under the one head puerperal fever, and of the treatment called for by each condition ; and a careful attention to the details of the antiseptic system, as it is brought more completely into accord with our daily increasing knowledge of the natural history of the lowest forms of organic life, shall or shall not be your rule and guide henceforth in your daily practice. What I have said in the very hasty and imperfect sketch it has been possible for me to offer in the few minutes you have so kindly accorded to me may soon pass out of your thoughts. But I am fully persuaded that there are many here to-night who will be able to assure us that such a diminution of unnecessary or excessive mortality after childbirth may be hoped for as may relieve you and your successors from much anxiety and sorrow, and, while making your own lives more useful and happy, may raise still higher in national esteem the noble profession to which we should all be proud to belong.

Dr. LEISHMAN (Glasgow) : In availing myself of the opportunity which the courteous invitation has permitted to me of taking part in



this discussion—for I presume there can be little doubt that the remarks of to-night will more or less take the form of a discussion—I am a little taken aback in your having selected me as the first speaker after the distinguished essayist of this evening. I confess, however, that I have a peculiar satisfaction in being able to speak to-night on this subject. I may say I have in some measure a personal satisfaction in speaking, upon various grounds. I think I shall have the support and sympathy of every person present whose duty it has been to teach the subject of obstetrics, and still more, perhaps, I may have the support of those who have had occasion to write upon this particular department of obstetrics, when I say that, when in their lectures they approach the subject of puerperal fever, they have felt that they were approaching a subject with which they were in a great measure incompetent to deal. It is not for me to explain, nor would I presume for a moment to indicate, the reasons of this in all their bearings; but I can well understand, and from experience I can assert, that the difference of opinion which has at all times existed on this subject has been the main reason of this confusion. I have a strong personal feeling in regard to this subject, because I have, to some limited extent, disseminated views which I now feel to have been erroneous; and I am, therefore, glad to have this opportunity, before such a distinguished body of my fellow practitioners, of disclaiming, and in a sense abjuring, certain errors which I have had some share in disseminating among the juniors of the profession. I have regretted every day since I wrote on the subject that I had not more thoroughly investigated the evidence of the pyemic source of puerperal fever; for the more I have done so, the more convinced have I become that, in a large proportion at all events of these cases, there is strong evidence to lead us to believe that they have their origin in pyemic or septicemic infection. I am not prepared to go the length which I believe many writers in Germany and some in this country have gone, in supposing that we are permitted to accept this pyemic theory as the solution of all our difficulties; that by admitting pyemia, or septicemia, or ichorrhemia, or whatever you choose to call it, as the cause of puerperal fever, we get rid of all our difficulties; and that for what we have hitherto considered *chaos*, we may in future read *cosmos*. I do not believe that it is so; for there are points of difficulty which must arise in the mind of every man of experience which have yet to be solved. I admit, and I should like to admit with all possible emphasis on an occasion like the present, that I believe the pyemic or septicemic origin of many cases of puerperal fever has been fully and thoroughly established; but I am not at all sure that this will account for all the cases that come before us in practice. There are cases in which a patient in the puerperal state unfortunately becomes the subject of diseases of specific origin. I may mention as an illustration scarlatina. A patient becomes infected with the specific poison of scarlatina. There is nothing in practice which we dread so much, and the result unfortunately proves



that our dread is well founded. But, in the later history of such cases, I have had a difficulty in discovering any difference between the cases which we may suppose to have a specific origin and those which have proceeded from a specific poison. Again, there is another class of cases in which it would appear as if the original symptoms were more those of a local inflammation, be it metritis or peritonitis, localized or general ; and in all these instances, whatever the initiatory symptoms may have been, in so far as my experience enables me to form an opinion, I have again a difficulty in separating those cases, as far as the final symptoms are concerned, from those in which puerperal fever is dependent on pyemia or septicemia. These I take the liberty of presenting to the Society merely as the difficulties which have suggested themselves to my mind, having naturally, and as a matter of necessity, had the subject under consideration for a long time. I well know that in what I have written on the subject I have stated views which I am not prepared to support ; I therefore began by frankly withdrawing something of what I had previously written ; and perhaps what I now state may enable those members of the Society, if there be any such, who have done me the honour of perusing what I have published, to correct these views. There are other points which suggest difficulties. We have frequently been informed that these diseases are likely to be engendered by decomposing animal matters, from whatever source the decomposition may originally arise. It has been very commonly asserted, and rules in fact have been framed, bearing upon the question in those countries more particularly in which puerperal fever is most common, that students who dissect are liable to convey this infection. I have not the slightest idea of calling in question the accuracy of this assertion ; but what I should like to point out is this, that if this were so certain and so frequent a method of communicating the disease as some would suppose, we should have far more puerperal fever in the practice of students who dissect. Then, again, if we are to assume this question of pyemia as identical with puerperal fever, we know what pyemia is in a surgical hospital. I presume it was the prevalence of pyemia in surgical hospitals which led the late Sir James Simpson to write his celebrated papers on hospitals. Pyemia in a surgical hospital is a dreadful scourge. It may be conveyed by the surgeon ; it may be conveyed by dressers, by nurses, by anybody ; and beyond doubt it may be conveyed, as Professor Lister has clearly shown, through the atmosphere. I consider that that is a point which now-a-days is demonstrated. Yet in surgical hospitals, pyemia is not such a scourge as puerperal fever is in lying-in hospitals. If you have the disease once existing in a lying-in hospital, you have it communicated with a frequency, with a fearful fatality in point of result, which it is appalling to contemplate ; so much so, indeed, that I think I scarcely exaggerate matters when I say that, in some Continental towns, the condition of a woman entering a lying-in hospital for attendance and care during the puerperal period, is something

like this : that she must be content to incur the risk of a patient who takes typhus fever in the discharge of this purely physiological function. Now, this identity of pyemia and septicemia with puerperal fever may be established within certain limits, or it may not ; but it appears to me that there is an intensity in the affection, that there is a peculiarity in the conditions, or there may be (as has often been said) in the woman in the puerperal state, a peculiar condition, which renders her specially liable to impressions about which we know very little, that may account for all this. These are points which, in connexion with those that have been formulated so kindly and ably for us by Mr. Spencer Wells, it appears to me should be taken under the consideration of the Society. I am unwilling to take up the time of the Society longer. There are many points on which I should like to speak ; but I should be sorry to carry the forbearance of the Society even to the limits in point of time which you have laid down. I cannot, however, sit down without congratulating the Society as a body on the fact that this important subject, perhaps one of the most important of the present day, has been in this prominent manner brought under the notice of the Society, in the temperate and dispassionate manner that it has been to-night, rather inviting discussion than stating broad and distinct views with which we must agree, or from what we must be prepared absolutely to dissent. And I would almost venture to predict that the result of the discussion which has been inaugurated to-night, arguing from the interest which it seems to have awakened in all quarters, will be a result which will be for good, for abiding good, in the future history of obstetrics.

Dr. NEWMAN (Stamford) : In rising to speak, I may say that I meet the requirements of your courteous invitation in two respects. I am a country practitioner, and I have come from some distance—I cannot say with the express object of being present to-night—but, at all events, I am glad that the opportunity should offer itself to me of listening to the discussion this evening, on a subject of so much importance to us all. You will pardon me if, in speaking on the matter, I may seem to speak too egotistically. I simply say that, after some twenty years of tolerably hard country work, first in a village, then in a country town, in general practice and hospital practice, I have come to certain conclusions which may or may not be correct, and which I should have a difficulty in following out one by one, giving you the arguments that have brought me to those conclusions. In taking the propositions which Mr. Spencer Wells has formulated for us this evening, I may say, in the first instance, that I should hold strongly that there is no such thing as a definite puerperal fever, so called. In the next place, I should say, with equal decision, that in a large number of cases of puerperal fever—I have traced it in my own personal experience, and have known it in the practice of other gentlemen with whom I happen to have been associated—there has been a distant, it may be yet a definite, link in the occurrence of possible transmission to the patient of some definite infecting poison.

I cannot give a much more striking instance of it than this. Some years ago I saw a lady who was exceedingly ill with puerperal fever. Two days after my visit she died. The opportunity was afforded me of close inquiry, I might almost say exhaustive inquiry, into the history of the few days or few weeks that preceded her delivery. It turned out that there had been a visit paid by her to the house of a neighbour in the same village, and a child in the house was at that moment suffering from scarlet fever. She knew nothing of it: she simply knew that the child was ill. She paid no attention to it, and, until the inquiry was pushed as closely as it could be, there had been no idea in the mind of the patient, or in the mind of the medical attendant (for he came from a distance), or of any one connected with her, that that could have been the cause of the disease that led to the fatal issue. I should take that simply as a type, and say that my impression is that, in a large number of instances, of some of which I have notes, and of some none, the condition in question has its origin in direct or indirect communication with some infective process. There is another set of cases—and I am again drawing on my memory for a very clear one that I happen to have seen—where I believe that a local inflammatory mischief may unquestionably kill, by its production of a definite pyemic condition, as certainly as it would kill in the instance of a surgical operation followed by pyemia. I was asked some time ago to see a woman, who had been suffering for two months. She was supposed to have been three or four or five months pregnant, and she had been suffering for two months from a certain amount of hemorrhage. She had lost so much blood, and her general condition was so bad, that one felt that the only thing to offer her a reasonable chance was at once to empty the uterus. This was done by her own attendant at my suggestion, in the first instance by the dilatation of the os uteri, then by manual removal of the fetus or ovum, and the secundines. Within three days she had rigor; and when I saw her subsequently she had all the symptoms of pyemia. She had an abscess in the shoulder-joint, an abscess in the wrist, and not a few collections of matter in different portions of the body. That again I should take as a type of another set of cases, where I believe a morbid process, which we are satisfied to call puerperal fever for want of a better term, had its origin in a direct local lesion. That lesion may have been produced by enforced dilatation, and the forcible removal of the retained material; but still there the condition was. I have further to say—and here I am referring to what has fallen from the preceding speaker—that one has very much to bear in mind that even in our present state, with fairly cared-for houses and the rest, there is not uncommonly such a condition of sewer-air permeating the houses, that the general state of a parturient woman before her delivery is below par; hence the greater tendency on her part to absorb—if I may use the word—infective material, however it may be presented to her. I would further add that there can be very little question that the activity of the vital



processes would certainly seem to be associated from the very earliest condition of pregnancy to its close,—that the history of every pregnant woman must be understood by a no far-fetched analogy, to have a material influence upon the question before us. Given a woman in whom all the processes, nervous, vital, circulatory, mental, if you like, are materially excited or altered in their condition of reasonable health; and I should maintain that in that phase alone we have much to look for in the way of an explanation of the reasons why poisons that run a different course, or at all events a slower course, under more ordinary states of everyday life, run, when they have to deal with a parturient woman, a course of far more severity and far greater rapidity, and, unhappily, far greater fatality. One more remark, and I have done. It seems that one should take into consideration, at all events that one should not hastily dismiss, the mental conditions which not uncommonly associate themselves with pregnancy. On this ground it happened to me some years ago to tabulate—I never did anything more than tabulate—a certain number of cases of puerperal fever that had occurred to my own knowledge. I think I may say that in a good number of them there were, at all events, the elements of distinct mental disturbance. I do not refer to mental disturbance in the sense of insanity, but in the sense of distressing circumstances, of the condition of pregnancy being a result of seduction, and many other reasons which will occur to gentlemen present. These do unquestionably seem to me to play a material part, at all events, in predisposing the system to the virulent development of septic poisons, however they happen to be introduced. I have to apologize to the Society for the very imperfect manner in which I have spoken. I began by saying that I would simply give my own personal impressions. I ask the Fellows to be good enough to accept them, and to give to them the very small value that they actually possess. I had no intention, when I thought of coming to the Society, to join in any way in the discussion; and I may almost say that, if I had not had a direct personal invitation, I should hardly have dared to do so.

Dr. BRAXTON HICKS: I presume that by the discussion in which we are engaged we shall not be able to clear up all the ambiguities and uncertainties with which this subject is surrounded. I take it that the principal advantage of such meetings is that the current opinion of observers may be brought out, and thus a certain impetus is given to the progress of knowledge, which would not have taken place, had each speaker waited to mature his opinions. Hence it follows that opinions expressed under such circumstances have more or less the disadvantage of imperfection, though it may be that the hints thrown out by the various speakers will stimulate thought in others, and, indeed, in themselves. The difficulties of this subject have been somewhat retarded by two means. First, most of the older observations were made in hospitals, and thus, if we admit that the disease is contagious, the character of the cases in each epidemic



is similar. Thus, some have said puerperal fever was erysipelas, diphtheria, &c. Another retardation arose from the very means by which information was obtained—namely, from the post-mortem room. For, according as one appearance was more prominent, so it was considered the essence of the complaint; thus peritonitis, enteritis, phlebitis, &c., were considered the real disease, instead of being looked to as effects. Not that I would underrate the value of these investigations, carried on as they were at much personal risk, but to point out that these opinions prevented the observer from looking in the real direction—namely, to the clinical facts, derived not only from hospital, but from all the variable conditions of home attendance. However, the general post-mortem appearances having now been ascertained and well known, it is to the clinical study of the disease that I would now urge the attention of obstetricians, leaving the inquiry as to the exact nature of the poison as a separate one, simultaneously carried on, but still separately. By this means we shall proceed with more distinctness of purpose. I have already in a paper called a “Contribution to the Knowledge of Puerperal Diseases,” endeavoured to show the clinical aspect of the conditions classed under the general head of “Puerperal Fever,” taken from the various circumstances of home attendance in all classes of the community. I would not again detain the attention of this Society, by alluding to the facts there shown, did I not consider them to have a great bearing on the subject of the paper of this evening, and that from further inquiries, the results there set forth may be taken as a fair average of practice as seen in consulting practice. There is no doubt that they represent the severer cases; but I must here point out an error into which many fall in estimating the extent of the influence of circumstances on the puerperal woman by the death-rate. It is not by the death-rate we can judge; I should say, broadly, that where one dies, three or more are retarded in their recovery by either a more or less mild state of fever, or by the secondary effects well known to us as cellulitis, phlegmasia, &c. Not till these also are calculated, can we recognise fully any influence brought to bear on the puerperal woman. Now, the cases brought forward in that paper were those of simple labour: I did not include any about which I had made no inquiry as to the surroundings, either those which existed before or after. I found that, out of 89, 68 had been connected in some kind or another with animal poisons, more than three-fourths. Of these 68, more than half—namely 37, had been connected with scarlatina in one way or another. Amongst the remainder, erysipelas, diphtheria, and offensive state of the discharges were prominent; in the remaining 21 I could trace no definite history in regard to zymotic diseases; but it would be difficult to say how many might not have been influenced by them, seeing it is hard to tell in many cases of zymotic diseases whence the source of infection had come. Some undoubtedly had been exposed to mental depression or excitement, were in low condition of health, or otherwise in

conditions not favourable to recovery from any great disturbance. But of the first class it may be asked, Are these really cases of puerperal fever? Are they not properly cases of puerperal scarlatina, diphtheria, erysipelas, and so on? Let us, then, for the moment exclude them; though in doing so we are excluding three-fourths of what have been received as cases of puerperal fever; at any rate, with the exception of the well-marked scarlatina cases. Let us examine the remaining 21. Shall we exclude or include those of them which have been brought on by sudden mental disturbances, as fright, annoyance, and anxiety? They probably amount to a fourth. If we exclude them, we shall be excluding cases like those which have been called "puerperal fever;" and yet, unless these influences generate the fever, we must exclude them. Again, four of the 15 left were ill before labour set in, and these can scarcely be looked upon as cases of puerperal fever. Some of the yet remaining 11 possibly may have had an influence from a zymotic disease, as I have already hinted. Some were of traumatic origin, not toxemic; and, allowing three for these, we reduce the total of unexplained origin to eight. Now, I would ask, where is the proof of the existence of a separate entity such as is ordinarily understood by puerperal fever? and close upon this result, I maintain, will be arrived at if you take any hundred of cases. But when you come to examine the symptoms belonging to the various classes, you will find that they all belong to the same class; and if you, as I did for a moment, exclude those cases influenced by zymotic diseases, you are met by this difficulty: that those without any specific symptom, as rash, &c., showed symptoms most typical of the so-called puerperal fever; that the specific symptoms were in all grades of proportions in the several cases; and that, generally speaking, the less the specific signs showed themselves, the more tendency there was to malignancy. This fact is well shown in hospitals where, erysipelas being the primary cause, the disease which followed would be more like malignant puerperal fever, and less like erysipelas. In the paper alluded to, many cases may be found showing this. I may venture to repeat one. A woman was taken with the most malignant form of puerperal fever, and died about the third day. I could find no history of scarlet fever previously; but, in a few days, two of her children had malignant scarlet fever and died. Since my paper, I saw the following case: a lady had been delivered twelve hours, when feverish symptoms commenced; a pile already inflamed became very tender and painful; from this part a blush spread, something like erythema, but without any defined edge, and spread over her back. About the third day, arthritic pains and swelling commenced, with delirium; these symptoms increased in intensity, and she died in great agony on the fifth day. As the symptoms began so soon after delivery, I could not help suspecting that it had its origin from without, as the symptoms arising from decomposing secretions commence from the third to the fifth day. I could not find

that she had been exposed to any exanthem. The medical man had not seen a case for two or three months. The nurse was apparently free. I, however, told the medical man I thought he would have evidence in the children. In about a week, the eldest child had scarlet fever, and rapidly died. The second was then attacked, and shortly died. Subsequently the baby was attacked, but recovered. In neither case was there any specific sign of scarlatina. Numerous cases I have seen where scarlatina was in the house, and the mother had puerperal fever without any rash. In some of the other cases where there are signs of a zymotic disease, they would have been overlooked had not care been taken, so masked were they by the general condition. But some have rejoined, "I have attended cases of scarlatina and delivered women constantly, and that without ill results." I would answer, Even so. But this is no proof that the influence of scarlatina is not detrimental. It is not every woman that is exposed to scarlatina who contracts it; on the contrary, most have already had it, and thus are less liable to catch it. It would be but few married women who would be susceptible to it. It would be an interesting point to know whether those are seriously affected by scarlatina who have had it in former years. I have not sufficient information as yet. But, as I have said before, the death-rate must not be the criterion of influence. We must watch the patient for a month. It is seldom that we shall find a bad getting-up in one who has been exposed to scarlatina just before or during her labour. An apparent contradiction to the influence of scarlatina has been instanced by the fact that sometimes, during an epidemic of scarlatina in a town, the lying-in hospital is free from puerperal fever. This is no proof. The only proof reliable is to be able to say that frequently cases of scarlatina have been introduced into lying-in wards, and no ill effect followed. The same answer applies to erysipelas. But there is another point which cannot be left out of consideration—viz., that violent mental emotions also are followed by symptoms precisely similar to those which follow zymotic influence, or the existence of putrid discharges. In fact, looking over the whole cases, we are, at least at present, unable to distinguish as a class the one from the other, excepting where the specific symptoms are superadded. To what conclusions, therefore, are we led? In some cases, no doubt, some other medium must be added, such as decomposing "sepsis," or the living bacteria, or some material which, mixing with the discharges in the uterine cavity, are absorbed into the system. It is difficult, in our present state of knowledge, to assign the proper value of this influence. That decomposing matter does cause these symptoms can readily be proved, but whether it acts through living or decomposing material, there is no evidence sufficient to permit us to argue. One thing seems to militate against the notion that it is the bacteria which, in many cases, accompany the absorption of offensive discharges—namely, that if we wash out the uterus, the symptoms very rapidly subside; in twenty-four hours I have seen



them pass mainly off. If living growths were going on, one would scarcely expect so rapid a subsidence. Whatever may be the exact nature of the poison, in our present knowledge I do not consider we are justified in basing any line of practice upon it; certainly not in allowing our clinical inquiries to receive any bias. That the puerperal woman is, by means of these various factors, brought into a state which we recognise as blood deterioration or disturbance, so that either she dies rapidly, or that, if this event be postponed, the processes required for repair after labour are so perverted as to be accompanied by inflammation extending to the peritoneum, or producing effusions which often tend to suppuration; or to the uterine veins, producing plastic plugging and its consequences; or the blood assumes a tendency to coagulate, and thus fibrinous deposits form in the vessels; in fact, to all the secondary troubles well known to us all, as the results of the primary blood disturbance, but considered formerly as the principal condition. Such is the liability of the puerperal women to these deteriorating influences, that I think we may generally trace nearly every ill-getting up to some depressing or disturbing influence, if we take the trouble to trace it. Whether the blood-conditions set up in the puerperal women are similar to the so-called pyemia, such as is observed in men and non-pregnant women, is not quite certain. That they seem to differ rather in intensity than in quality, I think is generally admitted. We can readily understand the intensification when we consider the altered condition of the blood in pregnant women, and the ease with which the nervous system is perturbed; intensified by the large surface which is exposed, and the greater facilities for absorption which exist in the anatomy of the uterus, compared with the conditions found in the male. The position of the question appears to me this: admitting that various circumstances can set up a malignant fever in the puerperal woman, has this fever so set up a permanent character capable of being communicated to other pregnant women? Looking to the cases as they occur in hospitals, there seems some reason to think there may be such a condition. Looking to private practice, one would also say so too, did we not see that apparently, when it spread to non-puerperal persons, it became reconverted to the zymotic form from which it had sprung. No doubt many difficulties attend this point of the inquiry; and to it attention should be particularly directed. However, without going so far as this, one may fairly say that a zymotic disease, if not modified in its true nature, is altered as to the usual character of the symptoms, assuming more the kinds which have been generally called "malignant" in the non-puerperal person; and this tendency to change its character is more noticeable the nearer the patient has approached the full term of pregnancy; whether this depends on the changed condition of the system, or the greater patency of the lymph spaces and veins, so as to increase the quantity absorbed, is not very clear. Seeing the many sources of contagion that surround all of us, I think that the notion



that the aggregation of a number of puerperal cases can set up, *ab initio*, a puerperal fever has no resting place. The aggregation unquestionably increases the number of chances of the introduction of some zymotic disease, while the exposure of a number to the disease will necessarily increase the number of persons affected. All aggregations do this. The same rule applies to surgical wards, with regard to erysipelas. While, therefore, I agree that aggregation tends to spread the disease, I am doubtful whether it sets it up, if cleanliness be carried out properly. But when once an exanthem has been introduced, then its eradication is by no means easy; and the constant recurrence of cases, without complete purification, may fairly, to my mind, justify many of the remarks on what was called, perhaps not quite correctly, "Hospitalism." There is only one other point to which I wish to allude: this is respecting the contagious nature of the conditions grouped together as puerperal fever. That the majority are contagious to puerperal women I have no doubt; whether all are so, I am uncertain. I am inclined to think that those forms derived from the zymotic diseases are the most so; those from the self-generated kinds the least so. Some few seem not at all contagious. I remember two students attending a woman with puerperal fever for some days; during this time, they attended some twenty cases of midwifery. None of these were ill afterwards. A second instance occurred. Before the nature of the case had been made out, upwards of twenty were attended without ill result. However, I wish I could say this of all. I have had lists not unfrequently given me by practitioners, where the effects from a smaller series were very differently shown, and yet I saw, a short time since, a most malignant form coming on apparently from mental distress. A coachman's wife, just after labour, heard that her husband had to leave his place. She was much affected, became violently maniacal for a few hours, and then subsided into the usual condition of malignant puerperal fever, of which she died in a few days. The medical man attending her told me he had to attend another in a day or two afterwards, and she died in a rapid manner. But, surrounded as we all are by contagium, it is very difficult to say how far any case is free from zymotic influence.

Mr. JONATHAN HUTCHINSON: I, of course, have nothing to do with obstetrics, or with puerperal fever, but I believe that the subject is one which has its analogies in general surgical practice, and one of the most interesting points in Mr. Spencer Wells's address was the manner in which he traced that analogy. If I correctly understood his views, he seemed to place puerperal fever almost exactly in the position in which we place the several kinds of maladies from which our patients suffer after operations. He would say that there is no such thing as a specific poison which can produce any fever that should be known as "puerperal fever," but that a number of maladies induced by various causes have been grouped together under that name, and I thought that he very ably classified these according to the several

causes which produced them. I hope the time will come when he will extend his unwillingness to use such a vague term as "puerperal fever" to a term which, I think, he accidentally used—"surgical fever." For, if puerperal fever be absurd, I contend that surgical fever still more leads us aside from the consideration of the true causes of the malady. If I may be permitted, I should like to say a few words on the great importance of attempting to define as precisely as we can the terms we use. I think we should much more easily get to conclusions, and find a much greater unanimity of opinion, if we did take these several terms, septicemia, erysipelas, specific disease, specific fevers, and try to attach to them some definite meanings. Although it may seem presumptuous, I will state what my own opinions upon these subjects are, with great deference to the Society, and hoping to hear my views criticised and set right, if they seem to be erroneous. I will say, with regard to erysipelas, that it is a disease which is of great importance in reference to puerperal fever, since we have the most of proof concerning it that the contagion from it is one which is potent in the induction of the local inflammation, which produces puerperal fever. I express, in the most unqualified terms, my belief that erysipelas is not a specific fever, that it is only a local form of inflammation, that this local form of inflammation may vary in intensity, may vary in duration, may be induced by many different causes, may undoubtedly be produced by contagion from the secretions of an erysipelatous patient, but may also be produced by other causes; and that the pyrexial symptoms and general disturbance are secondary to the local inflammation, and are proportionate to the local erysipelatous action that exists. I have expressed this opinion on many occasions. It is the opinion, I believe, originally stated by Mr. Higginbottom of Nottingham, and I regret that the majority of our systematic works still, without defending the position of erysipelas to rank as a specific disease, so define it. Authorities still assert respecting it, that it has a stage of incubation, and this I wish definitely to deny. It has a stage of development; certainly there is, it is true, a day or two during which the patient may be ill before the redness appears; the disease wants a little time to develop, but there is no true stage of incubation. When we know that it arises from contagion it will develop in a day or two of the virus being applied, within twenty-four hours, and that is fatal to its claim to rank as a disease due to a specific poison. Then we never see it prevail symmetrically in the two halves of the body, as it certainly would do if it were due to the introduction of a specific poison, which would develop in the blood in the same manner, as we know the specific poisons of small-pox, scarlet fever, and measles do. I assert next that erysipelas may be checked at any stage; that appropriate treatment will stop it at a very early stage in a manner which would be utterly impossible if you were dealing with a specific fever. Let me add, that this will apply to the next stages of those forms of inflammation of the uterus, which is due to erysipelatous

contagion, that they are not to be ranked as specific fevers, but as things which may be put an end to by appropriate treatment at an early stage. Dr. Braxton Hicks has just insisted, no doubt as the result of practical experience, on the importance of washing out the uterus; so that the thing is curable, and does not run through any definite stages. Having made this assertion respecting erysipelas, that it is to be defined as a local inflammation, which has special peculiarities that are capable of pretty easy definition, but having no true analogy to specific fevers, I must attempt to give some meaning to the term septicemia. Now, for that we have two meanings afloat in the professional mind. One is that it is due to some poison which is introduced into the patient's blood from without; that the poison then grows, spreads, germinates in the blood, and produces the symptoms. Now, what I would like to suggest is this, that the term septicemia ought to be applied to the results of poisoning of blood induced by the inflammation of the patient's own tissues. I admit that every now and then in connexion with a "poisoned" wound the phenomena of septicemia follow. To illustrate what I mean, I will mention two cases. I had to amputate the finger of a professional friend on account of gangrene, the result of acute inflammation, owing to a "poisoned wound." He had scratched his finger on a piece of carious bone, in an operation on the hip-joint. Acute inflammation followed, which passed into gangrene, and I had to remove the finger at the metacarpal joint. He was very ill at the time. This might be called septicemia, the finger having been poisoned and inflamed. My own belief is that the character of the poison which was introduced was of little or no importance; that there was something special in the state of the patient's health, perhaps something a little irritating in the secretion. The gangrene was the result of inflammation, and the constitutional symptoms were the result of the blood circulating through the finger, and back again from a portion of tissue which had passed into gangrene. I once amputated a foot on account of gangrene in a case in which I knew that the man's femoral artery was obliterated. The gangrene was continually spreading, and we were obliged to remove the part. Fearing gangrene, and knowing that there was no femoral artery, I amputated just below the knee. The leg was in a perfect state of health in that part; but within twenty-four hours of the amputation, in connexion, no doubt, with a deficient supply of blood, the stump passed into gangrene, which spread rapidly to the thigh; it all became dusky, and the patient was sick; the pulse rose to 160; he was feverish, had a dry tongue, and in fact, was in a condition evidencing acute septicemia. He was being poisoned, according to the way in which I would like to use this term, by the absorption of fluids from his gangrenous limb. The blood was going through it, and back again, and thus every time the body became contaminated by the circulation; and the man would have died if we had not adopted other measures. Believing that the gangrene was the source of poisoning,



I amputated again, at once, below the hip-joint, high up, at a part where the circulation was better. He was extremely ill at the time, but, contrary to my expectation, he made a good recovery. I quote the case as proving that the removal of the source of the poison was efficient in removing the septicemic symptoms. I may also mention the case of a gentleman who had insured his life in the Accidental Insurance Office. He had slipped in the street, and dislocated his thumb (a compound dislocation), and he died some six or eight days afterwards, with symptoms of septicemia, vomiting, a rapid pulse, dry tongue, shivering. His hand was passing into a state of gangrene. There was a long legal dispute as to whether he died of the accident, or of something in connexion with his general health. I mention this case in connexion with the others as an illustration of what I believe, that it was gangrene of the hand that poisoned the patient. I mentioned just now the case of a medical man who poisoned his finger, in whom gangrene followed, and some of the symptoms of septicemic poisoning. I wanted to confute the prevalent idea that some morbid matter is taken into the blood from without in every case in which septicemic symptoms are present, and that that matter is the cause of the symptoms. The next case in which I had to remove a finger was one almost exactly analogous. It was the case of a poor seamstress who had no poisoned wound whatever. A prick of a needle on the finger had led to gangrene, and she had a similar train of symptoms. We very often get symptoms of septicemia in cases in which there has been no possibility of their being produced by morbid poison; and it yet is quite impossible to distinguish between the cases in which gangrene follows an injury in a more or less unhealthy person without any introduction of morbid poison, and the cases in which inflammation follows when there has been some possible introduction of a small amount of such poison. I admit the influence of morbid matter as an irritant in setting up inflammation; but what I wish to hint is, that I believe the stage of gangrenous inflammation of the part is an essential stage of septicemia, and that it is quite possible for pyemia to occur without any local poison introduced from without whatever. So much, then, for septicemia. I think we should do well to keep it apart from the term pyemia, and to believe that in septicemia there is no phlebitis. In reference to the far more important subject of pyemia, I think we should give a little more attention to the opinions of our forefathers; and I believe that no one could get more information on this subject from any papers by modern writers than from the original paper which drew attention to the subject, by Mr. Arnott, written some forty-five years ago. Next in importance to the paper of Mr. Arnott, I rank the papers of Dr. Robert Lee: a series of admirable essays on puerperal fever, a perfect mine of correct information upon that subject. If we studied those papers better (in the *Medico-Chirurgical Transactions*), I feel certain that we should not have afloat the very erroneous opinions respecting pyemia, which have gained admission into some



of our most important text-books, very much to the regret of many. The great heresy that at present prevails respecting pyemia (at least, according to my belief), is, that it is possible for pyemia to be caused by the introduction of a specific poison from without into the blood. I no more believe this, than I believe that septicemia is so caused. Pyemia, I hold, is produced by an inflammation of the patient's own tissues. What we call pyemia, in more typical cases, is due to phlebitis. The old-fashioned notion is quite true, that it is a poisoning of blood by inflammation of the veins. I regretted that Mr. Spencer Wells was a little vague upon that point. He spoke of coagulation of the blood, and the breaking-up of the coagula. Now, in spite of experimenters upon the lower animals telling us of the difficulty of making the veins inflame, I hold that any one who goes into the post-mortem room, and examines the veins in cases of pyemia, will have no doubt that the veins do inflame. Nor am I in the least confuted by the assertion that we have cases of so-called phlebitis without pyemia; that there is coagulation of blood in the veins, solid masses formed in varicose veins, and yet in such cases we do not fear pyemia. Of course, we do not. The things are totally different; but that there is a suppurative and gangrenous phlebitis in which the veins become full of pus, in which purolymph adheres to the ulcerated lining membranes of the veins, I hold to be thoroughly established by the records of surgery, and by daily experience. It is not the breaking down of the blood clot; it is the ulcerative and suppurative inflammation of the vein itself. I could produce illustrations to-night, showing this in various parts in the most typical cases of pyemia. I do not say that the term pyemia is to be restricted to these cases. I think it is quite possible that there are other cases in which multiple abscesses are formed, in which there is no phlebitis; but, I believe, we shall proceed on a safe basis of classification if we at any rate accept these most definite forms of pyemia, which are due to inflammation of the veins as by far the most important group in connexion with the disease under discussion.

Dr. RICHARDSON: The view I would start with in regard to puerperal fever rests with the condition of the woman after delivery. She is at that time physiologically in a peculiar position. First, her blood is in a peculiar condition. That colloidal fluid which, separated in a solid form, we call fibrin, is sometimes in excess, from three to six or seven, and (as I once found on a direct analysis) eight parts in a thousand; so that the blood is at this moment in a trembling equilibrium, ready on the slightest possible disturbance to precipitate it. Then there is a diminution of salts in the blood, again a condition favourable to the precipitation of the colloidal fibrin. Then this woman has been for a time supplying to the child a mass of blood from her own body which has now stopped, so that practically she is in the condition of a person who has lost a limb, a considerable portion of the body. Then she is in a nervous condition, she has been

supplying from her own potential energy that element which has been shown in the movements of the fetus, and now that has stopped, and she is suffering from that nervous reaction which comes on when that motion is suddenly arrested ; she is, therefore, in the exact condition for a series of changes, which must necessarily be febrile in character ; and I think we must bear in mind this position of women physiologically, before we enter upon the subject at all. To these facts we must add those which we have reason to learn in regard to hereditary qualities. We must accept the fact that there are a considerable number of women who are hereditarily predisposed to particular diseases, and, I think, we cannot except puerperal fever from this position. Then we come to consider the woman in this state, and we ask the series of questions which Mr. Spencer Wells has placed before us. Taking these questions one by one, I should say that, from experiment as well as observation, I should be unable to declare that there was any such thing as a special poison belonging to the puerperal state, or a special poison creating puerperal fever. It is quite true that, puerperal fever once started, there is a poison formed which will apparently communicate the disease ; but there are certain varied forms of this poison, though, perhaps, all these poisons have one common meaning if we could look at them fully ; but there are so many poisons derived from other sources, which seem to have the power of producing the disease, that we cannot look upon this special poison as we do on the poison of small-pox or scarlet fever ; neither can we say, in respect of this first question, that there are such local lesions as would lead us in the dissecting room in any case to say, this was a case of puerperal fever, as we should say, this was a case of scarlet fever, or of typhoid, or of typhus fever. In fact, I recollect making a post-mortem examination some years ago, in a case of what was called puerperal fever, on a lady of some distinction in the metropolis. There were four eminent members of the profession present, and they were none of them agreed on this very point ; one said it was a simple case of peritonitis. We then began to discuss whether, supposing this case was going before a jury, and we had to give our opinions, we could formulate in any way a series of pathological changes, which would indicate the cause of death as due to puerperal fever. Or to put it in another way, supposing one of our Fellows had said, "This is a case of puerperal fever ; will you make a post-mortem examination ?" would any of us know precisely what we were going to find as distinguishing that diagnosis ? I should give a negative answer, therefore, to the question of Mr. Wells, both as regards the special poison and as regards the special characteristic pathology of the disease. In regard to the second question, as to whether any or all of the forms of puerperal fever may be referred to an attack of infective continued fever, as scarlet fever, and so on, I should also offer that a negative. Under the term puerperal fever, as I have been obliged to recognise it, from being frequently summoned to see cases which are so called, I should

say that I have seen four distinct forms of fever which clearly may bear this name. For instance, there is the pure, simple surgical fever, as we may call it, following upon the delivery of the child. I presume there is no such thing (at any rate the exceptions are very rare) as a case of delivery which is not followed by some slight febrile state ; that is necessitated by the changed physiological conditions, by the increased tension of the vessels which must occur from the removal of so much blood in the uterine circulation, and by the irritation that has occurred in the breast previous to the secretion of milk. There is always this simple surgical fever similar to that which occurs after a surgeon has removed a limb ; this, in fact, is a fever of resistance, as I have called it in a paper that I have written on the subject. Now I am bound to say that I have seen some cases of so-called puerperal fever which were nothing more than exaggerated forms of this fever, which it may be had become exceedingly severe, in which we have had true inflammatory fever, and apparent indications of danger from that source ; nay, in one case I have seen a fatal result from the formation of a plug of fibrin in the heart. In that case there was a simple rise of temperature of 4 degs. following the confinement, then a sudden collapse. An exceedingly rich blood, rich in fibrin, had been so modified by that simple rise in temperature, and by the increased friction of the blood passing through the heart, that a deposition of fibrin had taken place in the auricula of the right auricle, that had increased, layers of fibrin had been laid down, layer upon layer, as in aneurism, and ultimately a complete bag of fibrin was formed ; there was an extension of that into the pulmonary artery, a loosening of the whole, and then death ; yet no trace of the disease in any part of the patient, not in the uterus, not in any organ of the body, except that there was an incidental plugging up of the right side of the heart. We see such cases occurring in simple instances of pneumonia and some other trifling forms of inflammatory diseases. An attack of erysipelas, with a slight blush in the leg, I have known to terminate in a few hours in death in the same way. Then I have seen another form of puerperal fever (many will recal similar cases) of what may be called a remittent character, with slight symptoms or extreme symptoms of jaundice ; twice I have seen this coming on with high fever in the puerperal state. In a case in the neighbourhood of London this occurred. It was a pure case of bilious remittent fever, occurring in a neighbourhood where there was no puerperal fever about, and ending fatally in a few hours, and with all the conditions of yellow fever. I have seen cases ending fatally with this remittent bilious character pertaining to them in other instances. Then there is another class of cases such as Mr. Hutchinson referred to, where there is a true introduction into the body of the patient of matter derived probably from the uterine sinuses, and where we get one of the true distinctive forms of septicemic poisoning—a form in which the material becomes dark, and death takes place from deficient oxidation, and a form where there is a separation



of fibrin coming on rapidly, and terminating by stopping the circulation of the blood through the right side of the heart. These cases seem to me to be of that class mentioned by many speakers here, where there has been some local injury, the exposure of cellular tissue, the rupture or exposure of a vein, the formation of a modified secretion, the absorption of that, and death from what Mr. Hutchinson correctly called poisoning immediately from the patient herself. Then, lastly, there is another source of puerperal cases, where, without doubt, a poison appears to be carried into the body from without; and here comes the singular part of this poisoning, that the poison may seem to belong to any one of the poisonous diseases, so long as it reaches the patient, and that it may not in the strict sense of the word, so far as we know, be a poisonous secretion at all—it is poisonous in the individual sense, but not poisonous as coming from any person who has been poisoned. We have instances of scarlet fever, apparently provoking the puerperal condition in this way; we have instances of erysipelas, and I can adduce examples of all kinds. We have examples of poison derived from other puerperal cases, and we have that extraordinary instance, recorded with great faithfulness and honesty by Mr. Huntley of Jarrow-on-Tyne, where he shows, by a series of conclusive arguments, that a secretion from his own hand was the cause, three times successively, after intervals of seven months' practice, of conveying poisonous matter to the patients, and so inducing puerperal fever. These are true cases of septicemic puerperal fever, where we must assume that the poisonous matter is carried from the hand, or by some other channel, into the uterine surface, where it meets with a favourable reception for absorption, and so is conveyed into the body, producing all these phenomena which we have experimentally induced by inoculation. With regard to this second question, I should say there is no special form of puerperal fever, no one particular type of the disease, which we can say bears that name, but that there are several varied forms of the disease terminating much in the same way, but having distinct characteristics; that some are contagious, some not contagious; that some spring from what you may call a natural condition of the patient, others from an injury of the patient, others from the introduction of morbid material. This again bears on the third question, "If all cases of contagious and infectious disease which occur under other conditions than that of childbirth are set aside, does there remain any such disease as puerperal fever?" To that I should answer that, barring that natural febrile state which follows upon confinement, as surgical fever follows upon injury, there is no such thing as puerperal fever, and in that I should agree with Dr. Braxton Hicks. As regards the fourth question put forward by Mr. Spencer Wells, the means of preventing the spread of the disease, I have nothing to add to what was said by Dr. Braxton Hicks as to the effect of washing, and the general measure of removing the source of poison and isolating the patient. Then I am brought to the fifth question, relating to the presence of



bacteria and allied organic forms in the puerperal state ; that, of course, only relates to the pure septicemous condition, the last of puerperal fevers to which I have drawn attention. On this matter, as is well-known to most of you, I have held for many years—more than twenty-five years—to the purely physical side of the question, and my opinion is in no way changed by the various views which have been more recently announced as to the effect of organic germs and bacteria in the production of these diseases. I am at this moment still more firmly adherent to the physical view, from any recent researches, which have shown me the probable mode of action of these septicemous poisons, that they all act after the manner of those inorganic bodies, such as black oxide of platinum, manganese, and other bodies of that class, and, in infinitely minute quantities, have the power of eliminating oxygen from the blood, and preventing combination of oxygen with the blood. I take it, that the whole of these poisons act in that simple physical manner, and that, as regards the presence of organic germs and bacteria, they are matters of coincidence ; that where the conditions are favourable for those organic forms to grow and multiply, there they grow and multiply, but that this is entirely coincident ; that is to say, the further development of the organic form which can be perceived has no more to do with the production of the disease, than the presence of the maggot in the decomposing meat has to do with the decomposition of that meat. To put the point in a simple way, I remember a point in my child-life which will place the position well before the Society. There is a time in country districts, which is well-known, when sheep fall in the field ; they may not be dead, but they are found to be infested by carrion crows, which sometimes injure them severely, picking out their eyes and doing them a deal of mischief. In my school-days I remember that we had a debate, whether the fall of the sheep was due or not to the presence of these carrion crows. They certainly did a deal of mischief, and my school-fellows would go out with stones and sticks to drive them away ; others thought that it was the heaviness of the fleece that caused the sheep to fall, and we used to put the sheep straight on their legs, to run about again. Now it seems just the same physical cause at work producing the falling down of a woman with this disease from the influence of septicemous poisoning, and these products which are found are coincidences, beautiful pieces of natural history, influences which, perhaps, are not directly favourable to the progress of cure when once they are developed, but they are purely matters of natural history, removed altogether from the true action of septicemous poison. I have but a few other remarks to make. My impression is, that in the course of time, we shall arrive at the discovery of certain agents which will immediately stop the action of septicemous poisons, by their direct physical effect upon the blood, and their influences in holding oxygen in combination with the blood. I have recently referred, in another Society, to the effect of quinine in this respect ; but that is a bungling crude method of deal-

ing with an agent that will act in such proportions as the ten-thousandth or the hundred-thousandth part of a grain, so as to produce disturbance within the organism. So, dealing with this matter of antiseptics, I should say that, if antiseptics, as they are called, that is bodies which prevent putrefaction, are advanced as a means of curing these particular diseases arising from septicemous poisons, their action is not because they are antiseptics (because other agents which are not antiseptics possess a similar property) but for the simple reason that they act on a given principle, and many of them act altogether in accord physically, and I might almost add chemically, in neutralizing the specific action of these poisonous agents; I mean antiseptics do not act by destroying germs or organic forms, but they act definitely, by interfering with the poisonous action of the septicemous material which produces the fatal disease. I predict that in ten years hence, in this Society, we shall see a means of preventing these diseases from septicemous poisonings as clearly as we now see the means of producing them, by the introduction of these poisons in the form of inoculated matter in small-pox by vaccination.

The discussion was adjourned.

## OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, November 18th, 1874.*

DR. MATTHEWS DUNCAN, *President, in the Chair.*

*On the Risks run by Obstetricians.*

By Dr. CAIRNS.

It is not my intention to enter into detail on all the risks incurred by that class of medical practitioners above mentioned.

I might, *e.g.*, allude to the risk run by the obstetrician of carrying away on his person a swarm of pulices, pediculi, and cimices—parasites which, however interesting they may be to the student of natural history, are by no means pleasant companions to those who are infested with them—especially the latter, which through the medium of the obstetrician may infest his whole house, baffling almost every means for their extermination. These and other risks, however, I pass over; I confine myself exclusively to the risk run of contracting certain diseases, more especially syphilitic disease—a disease from which, considering the dreadful effects which it produces both on the party immediately affected and on his offspring, we may all well pray God evermore to preserve us. It is certainly a matter of deep congratulation that this disease is but rarely contracted by accoucheurs on their attendance in confinement cases, but it occasionally is, and perhaps more frequently than is generally believed,

owing to reasons for concealment which I need not mention, but which must readily occur to every one present. The case which I purpose laying before you is as follows:—About nine months ago a medical man was confined to bed for about a month from what his medical attendants considered to be a case of blood-poisoning, but which he himself considered to be a case of scarlatina, caught from a little patient whom he had recently been attending for a very virulent attack of that malady. Be this as it may, the case presented all the usual symptoms of scarlet fever, followed after convalescence by desquamation of the cuticle and the formation of nearly a dozen successive abscesses in the neck. Two days after he was able to resume duty, he was asked by one of his pupils to go and examine a midwifery case in which he thought the presentation was abnormal owing to the long duration of the labour. He made only one examination, and found the presentation quite natural, and that the retarded labour was due to the fact that this was the patient's first labour, and that she herself was above thirty-six years of age. The case terminated favourably. When my friend made his vaginal examination he had unfortunately a raw surface under the extremity of the nail of his right index finger; but before leaving, he, as usual, washed his hands thoroughly with soap and water, and afterwards allowed a stream of cold water to run over his hands from the sink water-pipe. Well, only a few days after this, he was seized with a most violent pain under the nail of the finger above mentioned, and two days after the occurrence, of the pain there appeared extruding from under the nail a small, dark, soft excrescence, which increased so rapidly in size that in the course of four days more it had attained the form and dimensions of an ordinary-sized filbert. The pain was so excessive that it was deemed advisable to wrench off the nail and apply strong nitric acid, both of which were accordingly done. The excrescence in three days sloughed away, but by that time—*i.e.*, two weeks only after the vaginal examination had been made—there appeared a copper-coloured eruption which covered the entire body, it being most marked on the face, trunk, and upper extremities. In the course of a few days after the appearance of the eruption the eyes became so much affected as to occasion intense pain, and greatly impaired vision, loss of hearing, loss of smell, falling out of the hair, and most intense periostitic pains, all of which still continue in a greater or less degree. Sore throat also has existed ever since the contraction of the primary sore, and still continues. The primary sore is not yet healed. A portion of the distal phalanx has come away, but there still remains the largest portion to come, but, not being yet quite detached, it cannot of course be removed.

The student who attended throughout the whole case most fortunately escaped all injury. This may lead to the supposition that the patient after all was not suffering from the disease to which the foregoing remarks point; but this objection is easily met by the fact, that he had no abrasion of skin, and moreover by the two follow-

ing facts, that the child died in a few days covered from head to foot with a syphilitic eruption, and that the parents candidly confessed that they were both suffering from venereal disease.

I now conclude with a few remarks, 1st, As regards the character of the primary sore. In this case it was neither indurated at the base nor umbilicated, and yet followed by secondary symptoms, and these of a very violent character.

2nd. The primary sore occurred at a much earlier period after contact with the poison than generally happens.

3rd. The secondary symptoms showed themselves very much earlier than they usually do. Was this owing to the natural susceptibility of the patient to the poison, or to his being less able to resist its action owing to his illness immediately before the contraction of this disease? In my opinion, both causes may have operated, but I leave my superiors to determine.

The most important points for consideration, however, are, Should a woman, when about to be confined, be compelled, under the pain of a heavy penalty, to inform the accoucheur of her own accord that she is suffering from venereal disease? I think she ought.

For, consider the moral effects of the contraction of this disease upon the poor practitioner. If he should happen to be a virtuous man—and so far as my knowledge extends most medical men are—or should he happen to be cursed with a morbidly sensitive disposition, the mere fact of his having contracted such a disease is extremely apt to render him a lunatic or a suicide, more especially if he is married, and still begetting children. But should such a direful result as this happily not follow, he is still doomed to suffer much, very much, cruel wrong. He loses his patients—generally the best ones; he goes to the country to be out of the way for a time; but on his transit by railway, the moment his face is seen every passenger in his compartment hurries off to another, anxious to be away from the presence of such a pestilential being; wishes lodgings when happily he reaches his destination, but no one will admit him with that small-pox like face; is obliged to return home so downcast and dispirited, and so utterly undone in body, that he sighs for relief from this earth altogether.

Such was the case with the poor patient referred to.

Good patients lost—his character assailed and maligned—his feelings tortured and his presence shunned, no marvel that he ardently longed for death as he did from his bodily and mental sufferings—sufferings occasioned by a wretch who raised him in the dead hour of night to attend her, knowing well as she did that she had no fee to give to her medical attendant, except that of the loathsome disease under which she was labouring—a fee which would rather not have been received. I might go on enlarging on the baneful effects of this disease contracted by obstetricians, but I think I have said enough to entitle them to the deepest sympathy and praise for the gratuitous services which they render in many



cases, considering the terrible risks which they run in so doing. And now, in conclusion, what means should be used by accoucheurs in order to avoid contracting this disease?

These means I shall simply state, without entering into details.

1st. Never attend a case of labour if there is any abrasion of skin on the right hand.

2nd. If any abrasion exists on the right hand, place the patient in the appropriate position, and use the left hand.

3rd. If not an ambidexter, have the right index finger in all cases covered with gutta-percha tissue soldered together by chloroform, which tissue entirely protects the finger, and does not affect its sensitiveness.

4th. In all cases, after each examination, wash the right hand or left, as the case may be, with a solution of carbolic acid or any other good antiseptic.

5th. If, after all these appliances, a suspicious sore should occur, have the nail wrenched off immediately and caustic applied to the matrix.

*Meeting, January 27th, 1875.*

*Fetal Montrosity (Phoco-Melos).*

By Dr. MACDOUGALL, of Galashiels.

*(Communicated by the President.)*

Early on the morning of the 12th September, 1873, I was requested to visit H. J., an unmarried girl, who was said to have become suddenly ill. I found her relatives anxious and alarmed, and totally unsuspecting of her condition. The membranes having suddenly ruptured, the bedding on which she lay was soaked with amniotic fluid. External examination revealed enlargement of the uterus corresponding to a seven-months' pregnancy, while the finger inside detected an os but slightly dilated, with a highly situated and somewhat obscure presentation. As yet she had no uterine pains.

Six hours after, I again saw her; the os had become somewhat softer and the presentation more distinct. Its softness and general contour gave the impression of a breech, which, however, external manipulation did not verify. As matters progressed and a more complete examination could be made, a small and peculiarly soft head could be diagnosed. Once clear of the os, the child was rapidly expelled. The moulding at the perineal orifice was a short process. Before withdrawing it from under the clothes, some of its peculiarities were evident, notably the shortening of the lower extremities. It cried feebly when born, and lived for some minutes. Not anencephalous, but having no bony development in the vault of the cranium, apparently wanting thighs and upper arms, the mon-

strosity can hardly be regarded as a common one. In this instance, as in all the anencephalous cases I have seen, the liquor amnii was in very great excess.

---

*Hair in the Female Bladder.*

By Dr. MACDOUGALL, of Galashiels.

(Communicated by the President.)

In April, 1874, I saw an elderly lady of corpulent habit, who was complaining of symptoms of gravel. The history she gave was that, until within the last two weeks, at no previous time had she had any urinary ailments. The first symptom was frequency of micturition, attended by very considerable pain. This pain she referred to the lower part of the abdomen. It certainly, she stated, was *not* aggravated by emptying the bladder.

Of full habit of body, living freely, and having comparatively little exercise, I was not surprised at such a history as she gave, and prescribed some antilithic remedies. No relief followed, the pain became more intense, and the calls to urinate more frequent. Now, however, she stated that the pain was greatest near the vulva. Digital examination enabled me to detect, lodged in the urethra, and in part projecting from its lower orifice, the mass of hair now exhibited. It was easily seized and removed, and with its removal all irritation ceased.

How it arrived in the bladder is matter of difficult explanation. Apparently ovarian in its origin, its transposition is curious. She had never suffered pelvic pain or uneasiness, and had no recollection of passing at any time a more than ordinary amount of urine, such as sometimes occurs when an ovarian cyst opens into the bladder.

---

*Fibroid Polypus.*

By Dr. MACDOUGALL, of Galashiels.

(Communicated by the President.)

On the evening of the 16th of June last, I was hurriedly summoned to visit Mrs. G., a married woman, aged forty, who was said to be very ill. I found her pale and exsanguine, with a cold clammy skin and feeble pulse, restless and anxious, exhibiting in a marked manner all the bad symptoms of hemorrhage. I was informed that she had prolapse of the womb, that it had come down some hours before, and that the bleeding had been profuse ever since. Between her thighs lay a large pear-shaped tumour, with a soft velvety surface, bathed in blood. No os uteri was visible, and the finger introduced into the vagina could detect high up the cervix, through which the pedicle of the tumour passed. The diagnosis was simple, the treatment more debateable. My difficulty lay between the use of the *écraseur* or the ligature. Ultimately I determined to tie it; and, having the tumour

drawn well down, I passed a fine whipcord ligature round the pedicle just within the os. The traction upon the tumour was still maintained, and, to my dismay, in tightening the ligature, it *divided the pedicle*. Instinctively I grasped the cervix, dreading severe hemorrhage would occur. My alarm, however, was groundless. After waiting some little time, during which the cervix retracted almost beyond reach, I carefully plugged the vagina, leaving instructions that watch should be kept lest the bleeding recurred. Happily it did not, and the woman, though weak and debilitated, made an excellent recovery.

Examination by the uterine sound some days after gave only a length of four inches. The growth itself weighed *thirteen ounces*, and at the spot where the pedicle was divided, the open mouths of some large vessels showed how great its vascular supply had been.

One or two points in this case are of interest. She had been the subject of the tumour for some years; the constant reddish watery discharge, a discharge which at her menstrual periods became excessive, and the bearing-down sensation, almost her only troubles. Six months before I saw her, it had first become prolapsed, and a medical man who visited her, mistaking it for the uterus, had, with much trouble to himself and pain to the patient, reduced it. A pessary was then recommended, but its use I suspect was not persevered in. The second occasion upon which it appeared externally, I saw and removed it. The effect of the ligature was happier than one had expected. My primary intention was, after in this way securing the vessels, to have removed the tumour with the knife. The fault I think lay in the continued traction upon the pedicle. The ligature once in position, had the parts been relaxed, an accident, which in some ways might have proved disastrous, would have been less likely to happen.

---

#### *Case of Central Rupture of the Perineum.*

By Dr. WILSON, of Alloa.

(Communicated by the President.)

I was asked, about 10 A.M. on the forenoon of Friday the 3rd April, 1874, to attend Mrs. N., aged twenty-six, in her first confinement. On visiting her shortly afterwards, I learned that she had had more or less severe pains in the abdomen since 3 A.M., and, on examination per vaginam, found the os uteri hardly admitted the point of the finger, while the vaginal surface was free from secretion. I ordered her a dose of castor-oil, and asked the person in attendance to send for me when she thought it necessary. I was sent for about 4 P.M., and on my arrival found the pains very constant and severe, the membranes ruptured, and the head pressing strongly against the perineum, with a rigid vulva. At once I supported the perineum with my hand, and for nearly an hour continued to do so, the vulva

in the interval becoming greatly relaxed. The head was now so far protruded, that I expected its exit each pain, but suddenly the perineum became thinned, gave way, and the head, receding from the vulva, escaped through the laceration, followed immediately by the body. I separated the child, passed the fingers through the vulva, and in a short time removed the placenta by the natural way. On examining the parts, the fourchette and sphincter ani were found entire.

In the evening, I wrote Dr. Matthews Duncan asking his advice as to what should be done; but after thinking over the case, I resolved to bring the parts into apposition by means of the quilled suture, and, with the assistance of my friend, Dr. Duncanson, did so next morning.

The ordinary precautions in such cases were taken, and everything seemed to be going on well, till the Tuesday morning, when, to my great disappointment, the wound was found to be partly gaping; the patient, having been restless during the night, caused the looped end of the wires to slip from off the quill.

The surface of the lacerated wound being in a sloughy state, I ordered poultices containing carbolic acid to be applied for a few days; then stuffed the wound daily with lint, soaked in carbolic oil, and in three weeks after the day of her confinement the wound was entirely healed, the perineum being very slightly puckered.

## OBSTETRICAL SOCIETY OF DUBLIN.

*Meeting, March 13th, 1875.*

LOMBE ATTHILL, M.D., *President, in the Chair.*

### *A Case of Extra-uterine Gestation.*

By JOHN DENHAM, M.D.

MR. PRESIDENT AND GENTLEMEN,—The rarity of this untoward and too-often fatal disease, or accident, as we may please to call it, must plead my apology for going a little into the history, the treatment, and results of such cases.

Riotanus, so far as we know, was the first writer who, in 1650, reported a case of extra-uterine gestation. Polinus, however, gives the history of a case, although not recognised as such at the time, as early as the year 1581, which was spontaneously relieved by an abscess in the left hypochondrium. He relates that a woman who had had ten children, conceived again, and at the full period was delivered through an abscess in the left hypochondrium, whence a well-grown infant was extracted, which survived one year and a half. The mother, however, died on the third day in great agony.

Four varieties of extra-uterine gestation are enumerated—viz., ovarian, Fallopian tube, ventral, and interstitial. The ovarian form



must result when the vesicle has been impregnated in its natural situation, without having been detached by the fimbriated extremity of the Fallopian tube from the ovary. In the two first recorded cases of this kind it was found that a fetus of about two months' growth had fallen into the cavity of the abdomen, the ovary in each case having a rupture on its most depending part.

The reason of the frequency of the second variety—namely, the Fallopian tube gestation—is very obvious. When we call to mind the narrowness of the tube, and how susceptible of inflammation all such highly-organized structures are, our wonder will be, not that such cases should occur, but that they do not occur much more frequently.

The third variety, called abdominal or true ventral gestation, takes place when the fecundated ovum, instead of being conducted into the uterus along the Fallopian tube, drops into the cavity of the abdomen and is there developed. In this case the placenta becomes attached to the intestines, to the mesentery, to the stomach, or any part covered with peritoneum; while the uterus, the ovaries, and the Fallopian tubes may be, and generally are, in a natural and healthy condition. In one of the cases recorded it is stated that the child lay with its head downwards close to the *lumbar vertebræ*, the feet being situated under the stomach and colon, while the rest of the body was covered with omentum; the cord was round the neck of the fetus, which was well formed and at its full period of gestation. The placenta was adhering to the lower part of the stomach, to a portion of the colon, and to the gastro-epiploic vessels. The external surface of the uterus and its internal were quite whole, as were also the ovaries and the Fallopian tubes.

The fourth variety, termed interstitial, of rare occurrence, is that form of uterine gestation where the impregnated ovum has made its way along the canal of the Fallopian tube into the muscular coat of the uterus. It does not lie under the peritoneal coat of the uterus, or between the muscular coat and the lining membrane, but between the layers of the muscular fibres. Of seven cases of this kind recorded by M. Brischat, five occurred on the left side.

Another variety mentioned is that of uncertain locality, but from a journal of the published records of cases given under this class, it appears to me probable that many, if not all, of them may be considered as fairly belonging to some of the other varieties, and not worthy of a separate classification. To any who may wish still further to investigate this important and interesting subject, I would beg to refer them, among other works, to the first edition of Davis's "*Obstetric Medicine*," where he has collected and given the history of about 90 cases, which he arranges in the following order:—Ovarian, 16 in number, all of them terminated fatally, and most of them at an early period after conception. Fallopian tube pregnancies, 40 in number, most of them fatal in the early months of gestation, but some survived; one lived 3 years, one 6, one 8, one 14, and

one 17 years. All the cases of ventral gestation recorded—8 or 9 in number—have, without a single exception, terminated fatally within the period of natural gestation, and the greater number during the earlier weeks.

Among the cases recorded in Davis's fourth and last division of extra-uterine conceptions—namely, those of uncertain locality, 24 in number—there are many cases of recovery, and many of deep interest. Permit me to mention a few of them:—1st. A case of recovery after 9 years' gestation, by the escape of the fetus through an abscess which opened near the umbilicus. 2nd. Recovery after a twin extra-uterine conception, and the enjoyment of health for 7 years subsequent to the cessation of life in the child. One case where the woman survived 27 years after an extra-uterine conception, one 28 years, and one 43 years. Again, he records a case of 2 extra-uterine gestations in the same woman conceived at different times or dates. Also a case of natural uterine gestation which supervened on an extra-uterine conception; and lastly, a case of recovery where the bones of the fetus were removed from the rectum by means of a lithotomy forceps. I had a similar case when master of the hospital. Our Dublin hospital reports, on the subject under consideration, are very meagre, but I cannot resist alluding to a case of deep interest, from the pen of a master mind, so graphically described that the author is photographed in the picture he himself draws. The late Professor Montgomery, in his very valuable work on the "Signs and Symptoms of Pregnancy," speaking on this subject, says:—"The writer had once an opportunity of examining a very remarkable case, which presented a combination that could hardly fail to be attended with infinite doubt. A woman was received into Cork-street Fever Hospital in 1828, with considerable enlargement of the abdomen. Her history, as far as it could be learned, was that, 8 years before, she had been in labour, which, after continuing for two days, suddenly ceased, and the child, as she expressed herself, rose up into her stomach. No delivery followed. After remaining in bad health for about two years, she again experienced the symptoms of pregnancy, and gave birth to a child that did not survive: but the former child still remained in the cavity of the belly, and during its continuance there she bore three children, the last of whom lived. Ultimately a fistulous opening formed near the umbilicus, which was enlarged, and the original child removed. It was in a state of wonderful preservation, measured 22 inches in length, and had attached to it about two feet of the umbilical cord." Dr. Montgomery adds:—"A case of a somewhat similar kind has been recorded by Dr. Steigertahl, in which the woman lived in good health to the age of ninety-four, with a full-grown fetus in the abdominal cavity for the last 46 years of her life, during which she bore two other children."

And now, Mr. President and gentlemen, I shall give to you, as briefly as possible, the history and termination of, I believe, the first

—and I sincerely hope it may be the last—case of extra-uterine gestation that it has been my lot to attend. On Sunday, 2nd of last August, I was called to visit a lady in Rathmines, at or about 12 o'clock in the day. I found her pallid and nearly pulseless, with incessant vomiting, and complaining of great pain over the abdomen. I learned from my friend, Dr. Mitchell, who had been in close attendance since 5 o'clock in the morning, that she had been suddenly attacked with severe pain in the abdomen. About 11 o'clock on the previous night, having walked home in perfect health, after spending the evening with some friends in the neighbourhood, she described the pain as if she had been stabbed with a knife, while at the same time a feeling of weakness and sinking suddenly came on. He found her suffering severe pain, which she referred to as rather below the region of the stomach, with nausea and occasional vomiting. Her pulse did not indicate much prostration. He looked upon the case as one of great gastric irritation, and gave her at once 20 minims of Battley's sedative, with 2 or 3 drops of hydrocyanic acid in a draught, and left her a mixture with small doses of Battley and hydrocyanic acid, to be taken every two hours, gave small quantities of brandy and soda-water, and directed hot epithems for the abdomen. He remained a couple of hours, and she appeared relieved, when he left. He said he would return at 10 o'clock, but a messenger came at 9, saying she was no better.

He found her a little weaker, and the stomach still sick ; ordered her iced champagne, and to continue the medicines.

Dr. Nicholas White, the coroner, came in, as a friend, to see her, and was of opinion that nothing better could be done for her, and to pursue the treatment for some time longer ; but while he and Dr. Mitchell were consulting in the next room, they were called in to see her, and found her in such an extreme state of prostration, bordering on collapse, that they at once took alarm, and suggested that immediate assistance should be sent for. I came about 12 o'clock, but in the interim she had rallied.

There is little to add, so far as treatment is concerned. I recommended that she should take a mixture with bicarb. soda and prussic acid, alternately with a pill containing watery ext. of opium and calomel, the iced champagne to be continued, with iced chicken-jelly, and beef-tea injections to be given, with wine or brandy in them. I saw her again in the middle of the day with Dr. Mitchell, and again at night, when she appeared to have rallied, to be in less pain, and to have longer intervals between the fits of vomiting. At an early morning visit on the following day, we found that the pain, the collapse, and vomiting, had all returned, and, in fact, that she was rapidly sinking. She died at or about 11 o'clock. We gave it as our opinion to the family that some ulcerated point in the stomach or breast had given way, and that effusion of the contents had taken place into the cavity of the abdomen, causing collapse and probably acute inflammation.

Our opinion and view of the case were confirmed by Dr. Banks, who had been sent for both on Sunday and early on Monday morning again, but unfortunately could not attend in time to see the patient alive. He arrived at the house immediately after death, and on hearing the symptoms, gave it as his opinion that perforation and effusion had taken place. My friend, Dr. Egan, kindly consented to assist me in making the post-mortem examination on the following day, but at the hour appointed I was unfortunately engaged with a midwifery case, and all the trouble of the autopsy fell on Dr. Egan and Dr. Nicholas White, who had been unremitting in his attention, both as a friend and a physician, during the entire illness. Before reading Dr. Egan's very admirable report of the post-mortem examination, I may mention that I ascertained that our patient had been married for several years; that she had been for some time in India; that she had given birth to a fine healthy boy, five years before, after which she fell into bad health, and had had several miscarriages, the last a twin case, which had occurred about five months ago; and that she considered herself to be, at the time of her death, about two months pregnant.

"August 3rd, 1874.—This day, as requested by Dr. Denham, I made a post-mortem examination of Mrs. S. C., aged thirty-five years, 48 hours after death. The body was that of a beautifully-formed woman, well nourished, moderately covered with subcutaneous fat, and of marble whiteness. There were not any external marks, save those the result of medical treatment; the face pallid and composed; the pupils contracted; a brownish fluid was issuing from the mouth and nose; the abdomen greatly distended.

"The thoracic viscera were perfectly healthy. On opening the abdomen, a rush of air and bloody serum followed the knife. It contained nearly a gallon of bloody serum and coagulum—the latter entirely filling the true pelvis and beyond; the liver and stomach were quite healthy—the latter greatly inflated; in fine, the whole contents of the cavity were exceptionally sound; the blood-vessels were unbroken. On removing the coagulum before described from its position, the source of hemorrhage was found to have been a ruptured 'tumour,' situated within the folds of the left broad ligament of the uterus (which was itself somewhat enlarged) in the track of the Fallopian tube, about two inches and a half from its uterine attachment. It was ovoid in shape, two inches and a quarter in its longest axis, which ran parallel to the tube by one inch and a fourth in its shorter transverse. It was irregularly lobulated on the surface, purple-brown in colour, and quite firm to the finger. It was embedded in coagula, and had evidently burst at its distal end, tearing through peritoneal and other investments.

"The head was not examined. The pelvic viscera were removed for further examination."

And now, what are the lessons to be learned from such a case as this? I fear they are few, and not of much practical value. It



certainly points to the importance of close and careful diagnosis, and also to a guarded prognosis, for the lull in all the urgent symptoms at my evening visit tempted me to hold out a stronger hope of recovery than the result warranted.

Let us for a moment suppose that the real nature of the case had been diagnosed at the moment of the rupture, could anything more have been done? In my opinion, nothing. Let us go back a little and suppose that we had been able to diagnose the nature of the case before the rupture had taken place, could we have done anything to prevent the final and fatal issue? I believe nothing. Again, let us suppose that the case goes on to, or near to, the full period of gestation, and that the child is alive, would we be justified in having recourse to the Cesarean section in the hope of saving the life of either mother or child, or both? On this point I reserve my opinion until called on to reply.

---

*On Protracted Labour, Hour-glass Contraction, Hemorrhage, and the Introduction of the hand into the Uterus.*

By S. NICOLLS, M.D.

Having seen in *The Dublin Journal of Medical Science*, for May, 1874, a discussion on the relative merits of the solution or the solid perchloride of iron, in which the introduction of the hand into the uterus incidentally occurs, and an invitation to practitioners is given to state the result of their experience, I am induced to give the result of my long experience regarding the introduction of the hand into the uterus, and also what led to my adoption generally of that proceeding. For some six or seven years after I got my appointment here and engaged in midwifery practice through the country, I was frequently much embarrassed and severely taxed as to time and bodily fatigue, by a complication in labour, which is frequently met with in the country, owing to the very laborious lives led by the wives of small landholders and labouring men, such as carrying on their back, in creels, turf from the bog, manure from their cabins to the fields, and stones from the fields to be broken for road-making. With those heavy burdens they are obliged to stoop forward, which causes the abdomen and uterus to be more prominent and pendulous than they should be naturally. The complication I allude to is a partial suspension of labour, so that the natural expulsive force is interfered with, and for some two, three, or four days, the woman is worn out with sickening, torturing, unavailing labour. I have frequently been kept two or three days waiting most anxiously for the descent of the uterus and the expansion of the os, so that I could use the forceps, and end the patient's suffering and my own anxiety and fatigue. I observed that the liquor amnii gradually escaped, and the uterus became narrowed and lengthened, so as to assume a long oval shape, instead of the compact globular form it should have. I also found

that this kind of cases was followed by hour-glass contraction, which added much to the poor woman's torture and suffering. On very many occasions, my hands have been for days nearly powerless, from the long and severe pressure they were subjected to in overcoming hour-glass contraction. Many and many a time, when driving home at night, have I anxiously thought and reflected on the cause of this complication, until, after seven years' meditation, I fortunately hit it off, and for the last twenty-five years I have had comparatively mental and bodily comfort. The result of my reflections was that I became satisfied that the suspension of labour was caused by the head of the child pressing the cervix uteri on the arch of the pubis; thus the descent of the uterus and the expansion of the os were completely arrested. After a time the waters commenced to escape, and the uterus, not being able to contract from the fundus downwards, contracted circularly on the fetus, until the legs became extended and the uterus moulded itself on the body of the child as completely as the shell of a walnut on the kernel. This I am satisfied of; for, on innumerable occasions after extracting the head, I had equal difficulty in getting down the shoulders, and the same in extracting the hips. Then the placenta was retained by hour-glass contraction. Now matters are entirely changed, and I get along generally with expedition, comparative ease to myself, and safety to my patients. In the last twenty-five years, my practice has been first to ascertain if the uterus retains its natural globular form; if so, I wait quietly and let Nature take its course; but should I find that the uterus has become elongated, and that the pains are sickening and unavailing, I give an opiate draught, and commence to dilate. I am very soon able to apply the long forceps, and comparatively with little trouble I can extract the child, previous to which I give a full dose of ergot, to insure a good contraction. Then, as soon as the funis is secured and divided, I introduce the hand promptly and remove the placenta, whilst the vagina and uterus are open and yielding. The ergot insures the contraction of the uterus, so that as I withdraw the hand and placenta the uterine contraction is immediate. I then apply the binder, make the woman comfortable, see that there is no loss of blood, and my trouble is over. They generally complain of after-pains, but this is a sure sign there is no relaxation, and, therefore, no hemorrhage. I advise that the patient be kept very low, and no oil or other purgative given. I have observed that women who get food and purgatives are liable to have the milk and other secretions interrupted, and sore breasts and even puerperal peritonitis follow. Now, as to the introduction of the hand, twenty-five years' experience has satisfied me that, as the President, Dr. Evory Kennedy, stated during the discussion, it is as harmless as the head or breech of the child. It is not the hand, but the vast amount of violent injury inflicted in forcing the hand through the contracted vagina and uterus, which excites inflammation and causes that most fatal disease, puerperal peritonitis. I am so satisfied with the ad-

vantage of promptly introducing the hand and removing the placenta, that I invariably resort to it with my most respectable patients. I could give many sad cases, to which I have been called, arising from waiting two hours for the placenta to be expelled.

---

## REGULATIONS FOR MIDWIVES IN DENMARK, SWEDEN, AND NORWAY.

### MIDWIVES IN DENMARK.

(*From Udsigt over Medicinalvæsenets administrative Ordning i Danmark i 1873.* Ved Dr. E. Hornemann. *Hygieniske Meddelelser og Betragtninger*, vol. iii.)

To provide the country with midwives, married women who have borne children, and who must (as a rule) not be more than thirty years old, are received and instructed as pupils in the Royal Lying-in Hospital, in Copenhagen. They are admitted by the managing body of the institution on the recommendation of the Superintendent Accoucheur (the Professor of Obstetrics in the University), which is essentially based on the judgment of the local officials and authorities (clergy, physicians, and police) as to the qualifications of the persons commended, as well as on the existing demand for midwives in various parts of the kingdom. The Lying-in Hospital and Orphan Asylum are under the Ministry of Religion and Instruction, and are managed by a special directing body; but, beyond this, the regulation of midwives for the country is entirely in the hands of the Royal Sanitary College.

The pupils of the midwives' school must themselves pay the cost of nine months' maintenance in the institution, and also twenty-five rix dollars (a little less than 3*l.*) to the funds of the institution, for instruction. Pupils in Copenhagen may, however, reside at their homes in the town; but, during the course, they must be present in the institution on alternate days to give assistance.

The extent of instruction is indicated in the "Midwives' Book;" but instrumental midwifery is not taught, nor is it allowed to be practised by any midwife. On the other hand they learn, and may practise in cases of necessity, turning, and manual traction, with operations on the placenta and umbilical cord. Instruction is also given in the hospital (in the evening) in reading, writing, &c.

An examination is held at the end of the course by an examining Committee, composed of a member of the Sanitary College, the Town-Physician in Copenhagen, and the Professor of Obstetrics. Candidates are placed in a first or a second class, or are rejected; and a certificate of instruction (lærebreve) is prepared by the examiners. With the certificate is given the code of instructions which, among other things, fixes the limit of the midwife's practice among the people. This certificate is given her on her departure from the

institution. It gives her the right of practice and of appointment as a district midwife in the different parts of the country, and as a reserve-midwife in that part from which she came as a pupil to the school in Copenhagen. Those only who have undergone the examination have a right to practise as midwives.

In 1871 there were (see *Dansk Lægestatistik*, 1872), in Copenhagen, 76 midwives; in the provinces, 869. Of the latter, 737 were district or reserve-midwives with appointments, and were becoming established almost every year in districts or towns, rarely in single parishes; 132 were in private practice.

The average proportion of midwives to the population in the provinces is 1 in 1838.

In Copenhagen none of the midwives have fixed appointments; but individuals are employed in the service of the commune.

The midwives are subject to the control of the local magistrates and district physician, and are under the superintendence of the Sanitary College. They are obliged to make special reports of labours, which reports, as well as their instruments, are inspected yearly by the public physicians. These, especially the *physici*,\* must hold special conferences with the midwives, to learn the extent of knowledge which they possess, to judge of their fitness, and occasionally to assist them by explanations.

Midwives may be suspended from practice when they suffer from infectious diseases; and the suspension is not removed until they have recovered and have undergone disinfection. They are also suspended when cases of puerperal fever occur in their practice. The duty of suspension and of fixing its duration, as well as of giving the necessary directions for disinfection, lie with the chief civil officer of the district (*amtmand*) and the district medical officer, or *physikus*.

When midwives act contrary to their instructions, or commit other gross errors, they are prosecuted and punished with fine, &c. In the worst case the certificate is cancelled, and they are forbidden to practise.

#### MIDWIVES IN SWEDEN.

*From Ofversigt öfver det civila medicinalväsendet i Sverige.*

Af. Dr. Axel Jäderholm. *Ibid.*

The occupation of midwife is dependent on licence; and the practice of midwifery is forbidden to all other persons under pain of fine, both to the person acting and to the person by whom she has been called in, unless in case of necessity, or where a physician or an examined midwife cannot be obtained. Instruction is given free of

---

\* The *physici* are superintendent medical officers, through whom the district physicians report to the Sanitary College. In 1872 there were eleven *physici* in Denmark.



expense at various institutions supported by the State; a portion of the pupils, so-called free-learners (*frilärningar*) are even supported from the public funds. [This is especially applicable to the institutions in Stockholm and Göteborg; in Lund, the arrangement is somewhat different]. The course lasts at least nine months, divided into two terms. The instruction is given from a text-book adopted by the Sanitary College, and embraces "all that relates to midwifery proper and the management of infants, together with bleeding, cupping, the application of leeches, and vaccination." At the end of the course, twice in each year, an examination is held, and licences in midwifery are issued. After the administration of an oath, the successful candidate is legalized as a midwife. Legalized midwives who show sufficient intelligence may proceed to attend a course of instrumental midwifery; the fact of passing a good examination in this is certified on the licence, and entitles the holder to practise operative midwifery in conformity with the instructions laid down in the authorized handbook.

A midwife has the right—

1. To free practice within the limits assigned for her calling. In the country districts she is generally appointed by the commune, with certain privileges granted her by the public vote.

2. To remuneration, which in the country is supplied from a tax imposed by public vote.

3. To the practice of vaccination.

*Responsibility.*—1. The midwife is under the supreme superintendence and disciplinary power of the Sanitary College, which, under certain circumstances affecting her conduct, can prevent her from practising her calling in future. If she be accused before a court of justice of want of skill, the Sanitary College must always be heard before sentence is pronounced.

2. The midwife is under the direction of the town, provincial, or district physician, as well as under the teachers of midwifery in the towns where educational institutions exist. She may be occasionally subjected to examination by her superiors. On settling in any neighbourhood, she must report herself to the nearest superior officer.

3. The regulation regarding midwives issued in 1856, contains many details concerning the practice of the calling. It is here only necessary to point out, that whenever a midwife uses instruments in labour, she must send a written report to the nearest superior officer (district physician, &c.), and thence to the Sanitary College.

4. A midwife is bound to give, without charge, a certificate of death in every case in which a puerperal woman or a newly-born child dies without being attended by a medical man.

In 1869 the number of midwives was 1840, unequally distributed. There is no obligation on the country communes to appoint examined midwives.

## MIDWIVES IN NORWAY.

*From Medicinalvæsenet i Norge.* Ved Director O. Sandberg. *Ibid.*

Midwives are instructed in the schools of Christiania and Bergen, in the Lying-in institutions, of which there are special examining boards, consisting of three physicians. There are about 600 examined midwives in Norway. Every woman has the right to assist at a labour, but she must call in the aid of a physician, or an authorized midwife, if she observes any unusual or dangerous circumstance during the birth.

## Obstetric Summary.

*On the Temperature of Puerperal Eclampsia.*

Dr. Bourneville (in *Archives de Tocologie*, April, 1875), relates the particulars of four cases of eclampsia, from which he concludes that—

1st. In eclampsia, the temperature increases from the commencement to the end of the attack.

2nd. In the interval between the fits, the temperature maintains itself at an elevated degree, and at the time of the convulsions it increases slightly.

3rd. Finally, if the eclampsia is likely to terminate fatally, the temperature continues to increase and attains a very high degree; if on the contrary the fits cease, and if the coma diminishes or disappears, the temperature goes down gradually and returns to the normal standard.

He lays great stress upon differentiating the various kinds of uremia from puerperal eclampsia, and states that from observations of over thirty cases, a *diminution* of the temperature occurs at the commencement in uremia, and an *elevation* in puerperal eclampsia. As the case progresses towards a fatal issue, the temperature in uremia descends very low, even below the normal standard, whereas in puerperal eclampsia it rises to a very high degree.

*The Corpus Luteum of Pregnancy.*

Dr. Albert Puech (in *Gazette Obstétricale*, March, 1875), in a paper on the subject, concludes that the corpus luteum of pregnancy, and that of menstruation are two products entirely identical in their origin and their histological structure, but differing essentially in their duration, and the different phases of their evolution. Whilst the second attains rapidly its height, and decreases very rapidly, the first appears to increase until the third month of pregnancy, then decreases slowly so as to present still at the ninth month the half of its maximum diameter. This fact proves undeniably the suspension of spontaneous ovulation during the whole duration of gestation.

*Cystic Myomata of the Uterus.*

Dr. Spiegelberg writes on the diagnosis of cystic myomata of the uterus, and their intra-peritoneal enucleation, and describes a new method of operation (*Archiv für Gynæc.*, Band vi. Heft 3, 1874). A patient, aged twenty-eight, who was supposed to be suffering from ovarian disease, was operated upon. The ovaries were found unconnected with the tumour, which sprang from the posterior aspect of the uterus from about one inch below the fundus to the insertion of the vagina and reaching more than four inches above the umbilicus; it was covered by the uterine peritoneum and the posterior layers of the broad ligaments. On puncturing it was found to be a cystic tumour of the uterus. Half a bucketful of dark yellow fluid which coagulated at once and completely was removed; the growth was then drawn out, ligatured, and cut off with the knife. As hemorrhage continued, the remaining portion of the uterine walls was enucleated, leaving a deep hollow the size of the fist. This large cavity was closed by eighteen silk sutures passed through the peritoneal surface, and the hemorrhage was arrested. The ends of the sutures were carried out at the abdominal wound and a drainage tube passed through the retro-uterine pouch into the vagina; the abdominal wound was then closed. The patient died suddenly on the sixteenth day, most probably from pulmonary embolism; no autopsy was permitted. The diagnosis of the fibro-cystic tumour of the uterus by physical examination alone is almost impossible. They are generally taken for ovarian tumours, and their true nature is only discovered when ovariectomy is performed. The exploratory puncture is the only certain diagnostic test, as the character of the fluid, its rapid and complete coagulation reveals its nature. Spiegelberg designates this operation, enucleation and bringing together the peritoneal covering of the cyst with sutures, and thinks it more favourable than the removal of a portion or the whole of the uterus. The author speaks very favourably of Marion Sims' system of vaginal drainage in ovariectomy.

---

*Multiple Fibroid Tumours of the Uterus inducing Labour and Interfering with Parturition—Softening of the Tumours—Spontaneous Accouchement—Expulsion of two Tumours.*

M. le Dr. Charrier (*Annales de Gynécologie*, February, 1875) relates the particulars of a case of this nature.

Patient aged twenty-six, fourth pregnancy, previous labours normal. At the commencement of present pregnancy patient suffered much from loss of appetite, vomiting, profound anemia, profuse leucorrhea, and subsequently hemorrhage. On February 26th, a tumour, the size of an ostrich's egg, was found protruding from the vulva; hard, with no point of softening. It was impossible to make out the base of implantation of the tumour on the os uteri. Efforts at expulsion were constant.

Enema of chloral and laudanum was given and the patient watched. Symptoms of softening of the tumour with extremely fetid discharge ensued. Delivery occurred on March 22nd, a living seven months' fetus being expelled. The tumour was removed with scissors.

Another, the size of a turkey's egg, was expelled spontaneously the following day, and a third one the following week. The patient made a good recovery. The child died within twelve hours.

---

#### *On the Physiological Action of Chloral and Chloroform combined.*

Monsieur Verrier (*Gazette Obstétricale*, March 5th, 1875) demonstrates the reciprocal action of these two drugs in a case of tedious labour, where both were employed. He concludes that chloral acts like morphia, opposite to chloroform—*i.e.*, that it is a soporific like opium, and not an anesthetic like ether or chloroform. But the two effects—soporific and anesthetic—join so as to make two quantities of the same nature. They act in the same way, and it is a question whether this combination is not without danger.

---

#### *On Chloroform in Natural Deliveries.*

Professor Pajot (*Annales de Gynécologie*, January 15th, 1875), in reviewing Dr. C. J. Campbell's work, concludes that no practitioner would hesitate to use chloroform, with all necessary precautions, in cases of painful operations, or abnormal, though spontaneous deliveries, unless there be indications to the contrary. Some imagine that its dangers and inconveniences exceed its advantages.

Partial anesthesia, he regards as useless, as it is harmless and of no scientific value.

---

## Gynecic Summary.

### *Hypertrophy of the Anterior Lip of the Uterus.*

Dr. Blox (*Le Mouvement Médical*, Dec. 26th) reports on a case where an accoucheur, Dr. Calmeil, was called to a labour by a midwife, to a woman in labour already twenty seven hours. A fleshy tumour, consistent and depressible, protruded from the vulva; the fetal head was placed above and in the position left occipito-iliac, and delivery was effected. The tumour was the size of a fetal head, and hung from a voluminous pedicle, following which with the forefinger, the anterior lip of the os uteri was reached. The placenta had come away entire; the uterus had commenced to contract. Dr. Calmeil placed a wax thread around the pedicle, which he tied tighter daily. On the fifth day this pedicle was only the thickness of the little finger. In a few days the pedicle was cut through. The



tumour thus removed was solid, covered on its external surface with a strongly adherent mucous membrane, and formed of dense fibres traversed by numerous vessels. Dr. Blox had observed fifteen years ago an analogous case in a young woman, who had been long in labour when he was called in. A tumour like that described hung from the vulva: the forceps terminated the labour, and the tumour retired into the vagina, where it became so atrophied that in a month it was barely possible to recognise an hypertrophy of the anterior lip of the os uteri. Dr. Verneuil mentioned the case of a woman who had died from total obstruction of the vagina by a soft tumour which was found to be the anterior lip of the os uteri.—*Med. Press, &c.*

---

*On Lesion and Disease in the Chronic Affections of the Uterine System.*

Dr. Emille Tillot (in *Annales de Gynécologie*, March, 1875), in an elaborate series of articles on this subject, comes to the following conclusions:—

1. Chronic diseases are only chronic in virtue of a diathesis or of a spontaneous alteration of blood.

2. All the affections of the uterus which develop themselves independently of the puerperal state, and a certain number of those which develop themselves during this period are chronic, or tend to become so; they are in consequence amenable to the laws which preside over the chronicity of other affections of the organism.

3. The diatheses which play the greatest part in the causation of chronic affections of the uterus are, in the order of their frequency, the strumous, rheumatic, herpetic, syphilitic, &c.

4. After diathesis, the most frequent cause of chronicity of uterine affections is the morbid and spontaneous 'deglobulisation' of the blood.

5. That which authors call special diathesis, predisposition, idiosyncrasy, appears to enter into the two great orders of causes.

6. Pregnancy, parturition, and other determining causes are powerful aids to the production and maintenance of uterine affections, but these may break out and become chronic in the absence of every occasional cause.

7. In certain rare cases the affection is primarily local, whether preceded or not by parturition.

8. In the majority of circumstances, the lesions are only the secondary symptoms occurring under the influence of a general condition.

9. The chlorosis which exists in a patient affected with disease of the uterus, generally increases and declares itself beforehand; in certain cases it is consecutive to the lesion, the original cause is in a diathesis.

10. Finally, in chronic affections of the uterus, the lesion is in the uterus, and the disease in the organism.

## Pædiatric Summary.

### *Cirrhosis of the Liver in Infants.*

Dr. J. Cazalis (in *Le Progrès Médical*, March 20, 1875) records two instances of this disease. In the case of a girl, aged seven years, there was ascites, edema of the lower extremities, and albuminuria. Paracentesis abdominis was performed, and  $5\frac{1}{2}$  litres of serous fluid withdrawn, more fluid exuding subsequently from the puncture. Death ensued a month afterwards. At the autopsy, marked cirrhosis of the liver, enlargement of the spleen, and slight fatty degeneration of the kidneys was observed together with evidence of peritonitis.

In the second case, a girl, aged nine years, became suddenly very ill and feverish; vomiting, and epistaxis with dyspnea and ascites ensued. Paracentesis abdominis was performed, and four litres of albuminous fluid drawn off, but the patient succumbed a fortnight afterwards. The liver was exceedingly atrophied, being no larger than an adult fist. There was no history of alcoholism or syphilis.

## News.

### *Laws of Foreign Countries Relating to Midwives.*

On Friday, the 16th April, Walter H. James, Esq., M.P., moved in the House of Commons for a return of the laws of Foreign Countries in respect of the instruction, examination, licensing, registration, and supervision of midwives. Being an unopposed return, no mention was made of it by the press, but the information required will be collected and presented in due course. Mr. James has evidently taken up the midwife question in good earnest, and by this application has shown that he intends to make himself fully conversant with the subject before urging Parliamentary action in the form of a Bill.

### *Execution of an Abortionist.*

Alfred Thomas Heap, who was convicted at the Liverpool Spring Assizes for the wilful murder of Margaret M'Kivett, at Manchester, on the 16th March, was executed at Kirkdale Gaol on the 19th April. We have been favoured with the following notes of the post-mortem examination of the deceased, made by Mr. Thomas Jones, M.B., Pathological Registrar at the Manchester Royal Infirmary:—

There were no external marks of violence. She had apparently been dead some time, as there was evidence of commencing decomposition. There was acute inflammation of the lining membrane of the abdomen and of the surface of the intestines. There was a

quantity of pus-like fluid in the pelvis, and the outer surface of the womb was covered with lymph; when I removed the womb, I found on the external surface two very small punctures, one on the anterior surface, and one on the posterior. I passed a fine probe through the punctures and found that they communicated with the inside of the womb. The surface of the womb was  $4\frac{1}{2}$  inches long, and the width nearly 4 inches. I afterwards weighed the womb and found it to weigh 7 ounces; the wall was about  $\frac{1}{2}$  inch thick. There was no doubt that the womb had been lately impregnated, and lately emptied. I think she might have been in the fifth or sixth month of her pregnancy. There was clotted blood in the womb and in the vagina. The lining membrane of the womb was breaking up, the natural result of confinement or abortion. The stomach and intestines were healthy. There was a quantity of greenish fluid in the stomach. The left lung was adherent, indicating old inflammation; otherwise the lungs were healthy. The heart and other organs were healthy.

At the deceased's own house I carefully examined the contents of three chamber utensils: one contained a large quantity of greenish vomited matter, such as I found in the stomach; one contained urine and some pieces of paper with blood on them; and the other contained perhaps half-a-pound of salt, and there was salt in the room. There was also a towel with several oval spots of blood on it, which might have been produced by using the towel as a vaginal plug. There were a quantity of other clothes recently washed.

The breasts were enlarged, the nipples prominent and surrounded by a dark circle, indicating that the deceased had been pregnant.

There was no doubt that the punctures had caused acute inflammation of the peritoneum, which was the cause of death. The steel instrument produced by the inspector might have caused the punctures; there is no legitimate use in our profession for such an instrument.

Lymph was found on the surface of the bowels, but no adhesions.

An energetic attempt was made to obtain for the miserable man exemption from the extreme penalty of the law, but without avail. He had for some time been carrying on a wicked trade as an abortionist, and had before been convicted for procuring abortion.

---

Communications have been received from Dr. Braxton Hicks, Mr. Cullingworth, Dr. P. Jamieson, Dr. Morley, Dr. Lowsley, Mr. Lowndes, Dr. A. Henry, Dr. Edis, Dr. Carter, Dr. Orchard, and Dr. John Brunton.

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.

THE  
OBSTETRICAL JOURNAL  
OF  
GREAT BRITAIN AND IRELAND.

~~~~~  
No. XXVII.—JUNE, 1875.  
~~~~~

Original Communications.

---

INFLUENCE OF POSTURE ON WOMEN.

By J. H. AVELING, M.D.

Physician to the Chelsea Hospital for Women, &c.

CHAPTER III.

HYPEREMIC DISORDERS OF THE PELVIC ORGANS  
PRODUCED BY POSTURE.

*(Continued from p. 630, vol. ii.)*

HAVING already pointed out in a general way the large vascular development which exists in the female pelvic organs, it will be readily understood how proportionately they must be liable to all those morbid changes which have their origin in abnormal blood supply. It having also been shown how the amount of blood circulating in the vessels may be increased and diminished by postural influences, our task will now be to consider this subject still further, and endeavour in the first place to determine how far gravitation may produce the hyperemic disturbances so frequently met with in the pelvic viscera; and secondly, briefly to enumerate these, and indicate their appropriate postural treatment.

Since the time of Harvey, those who have devoted most  
No. XXVII.—VOL. III.



time to the study of hemostatics have, as a rule, paid the greater part of their attention to the influence of the heart's action upon the blood. Chemists and pathologists have also with infinite pains investigated its composition and morbid conditions. The ways in which it is affected by the potent and ever-acting power—gravitation, have, however, it must be acknowledged, not been observed with a corresponding amount of minuteness and scientific accuracy; nor have many disorders, the origin of which may with certainty be attributed to gravitatory factors, had these essential elements in their etiology sufficiently recognised.

The pelvic disorders of hyperemic origin fairly attributable to posture are chiefly the following:—varix, hemorrhage, hematoma, thrombosis, hypersecretion, edema, hypertrophy, ulceration, and hyperesthesia. These will now each be considered in their relations to the various organs and parts affected.

### I. *Hyperemic Disorders.*

(a.) *Hyperemy*.—Synonyms: congestion, chronic inflammation, catarrh, irritation, plethora, engorgement, turgescence, determination of blood, fluxion.

Andral was the first to use the word hyperemy, or rather its Latin equivalent *hyperæmia*; and he defined it as “a lesion of the circulation, in which the quantity of blood in the capillary system is preternaturally increased.” Trousseau has defined it as “a non-inflammatory collection of blood in a part.” This is perhaps the best general definition of hyperemy which can be given, and it will be more readily accepted and live all the longer, inasmuch as it does not enter into particulars.

Hyperemy is an abnormal condition every one can understand. Its existence has been recognised and admitted by medical writers in all ages, and every year its importance in pelvic pathology appears to be receiving more consideration. It is no new idea to attribute the origin of many disorders to excess of blood in the vessels, for every one must be familiar with the celebrated maxim of Stahl—“*plethora omnium morbo rem mater.*” Irritation, not long ago, held an im-

portant position in the causation of disease, but this term has been superseded by inflammation, which in like manner seems destined to be supplanted in a large degree by hyperemy.

Hyperemy may be divided into active and passive.

(b.) *Active hyperemy* includes all fluxions (the increased affluxions of arterial blood), whether of physiological, traumatic, chemical or morbid origin.

(c.) *Passive hyperemy* (the retarded efflux of venous blood) may be subdivided into hypostatic, mechanical, and hypocardiac; the first being due to gravitation, the second to obstruction, and the third to diminished cardiac action.

Although the active forms of hyperemy are capable of being intensified and protracted by gravitation and position, they cannot be said to be caused by them. The passive forms, on the other hand, whilst they are liable to be aggravated and perpetuated by the active, are almost entirely caused by postural influences. It is with passive hyperemy, therefore, that we have most to do, and it will consequently be necessary to consider carefully those forms of it which may with certainty have their origin attributed to posture.

(d.) *Hypostatic hyperemy* may be defined as a dilated condition of the veins, and venous capillaries produced or perpetuated in dependent parts by the distending influence of gravitating blood. A good example of it is given by Isidor Bourdon, who writes:—"One evening in the summer the heat was so intense that, returning home with the intention of studying, I was forced to throw myself for some moments on a couch to recover that perfect calmness indispensable to serious thought. In this position, as favourable to meditation as repose, I gave myself up to some reflections on physiology, when I perceived that the nostril corresponding to the side on which I reposed ceased almost entirely to permit the passage of air, and that respiration became more and more difficult. I turned then mechanically on the opposite side, and soon after the nostril at first obstructed became free by degrees, whilst the other, being now the lower, was affected and narrowed in its turn. This option of

contracting the two nostrils, a contraction which coincided precisely with the inclination of the body to either side, soon fixed my attention. I then turned myself on my back, and I observed in this last attitude that the nostrils remained entirely free." These interesting observations show the influence of blood gravitating in the venous capillaries. The same effect occurring in the larger veins has already been alluded to as being readily seen at the back of the hand when the arm is dependent.

(e.) *Mechanical hyperemy* occurs as a postural result next in frequency to hypostatic. It may be caused either by obstruction produced in the displaced veins of organs gravitating out of their normal positions, or by organs or tumours falling hither and thither, and directly compressing the veins which convey blood from the pelvic viscera. In both the result is the same—an excess of blood in the dilated vessels upon the capillary side of the obstruction. In this form more than any other the veins are liable to become greatly distended, as the heart continues to pump the blood into the obstructed vein, which has become little better than a blind appendix.

(f.) *Hypocardiac hyperemy* is caused by diminution of the heart's action ; and this, as has already been noticed, may be produced by posture. Whenever the body is at rest, the heart beats less frequently, and contracts less powerfully. If the resting position be such as to favour the production of hypostatic hyperemy, this condition must necessarily be increased by the slackened impetus of the blood current, for it is chiefly upon the arterial vis a tergo that the venous stream now depends for its momentum.

Postural hyperemy of the pelvic organs and tissues may therefore be produced by any one or more of the three passive forms here mentioned. To determine in what proportion they are concerned in causing hyperemic disorder, and to understand the peculiar influence which each or all may have upon the pelvic organs individually or collectively, will demand of the practitioner patient and careful investigation.

*2. The Vulva.*

As a general rule, it may be accepted that the venous circulation of the most dependent part of the body is always more or less retarded. This being the case, it would naturally be expected that the vulva, occupying as it does the lowest portion of the trunk, must be particularly liable to become hyperemic, and the seat of various consequent disorders. The vascular supply to the external genital parts is unusually large, some of them possessing the faculty of erectility. They are also subject to reiterated active hyperemic influences. Those, therefore, who sit the greater portion of their lives must expose themselves to many disorders which women more actively employed escape. Those also who adopt the dorsal reclining position during the greater part of the day are equally liable to be affected by disorders following a distended condition of the blood-vessels. They are perhaps even more subject to consequent mischief, as they are deprived of the remedial counterpressure of the surface upon which the parts of those sitting rest.

(a.) *Varix*.—Varicosity may be looked upon as exaggerated hyperemy. The veins being habitually distended with blood become dilated, gradually increase in size and at length assume the peculiar and well-known convoluted appearance. The pudenda are not unfrequently affected with varix. Its origin is generally mechanical, and its most frequent cause is the gravitation of the pregnant uterus upon the larger venous trunks compressing them and thereby preventing the return of the blood to the pelvis. It is however sometimes unconnected with pregnancy and may result from mechanical hyperemy produced by tumours or dislocated organs, or by impeded venous reflux of a hypostatic character.

This affection although apparently simple and inoffensive, may from the serious consequences which are liable to ensue, become one of very grave importance. No pains should therefore be spared in endeavouring to prevent its occurrence. Women when pregnant or suffering from large abdominal tumours should not remain too long in an upright posture.



Fresh air and moderate exercise are necessary for the health, but when at rest they should recline or lie down.

(b.) *Hemorrhage*.—Bleeding from the vulva is usually caused by the rupture of varicose veins. In some cases this accident is determined very easily, by a slight fall or sudden effort, but is most generally occasioned by the passage of the child's head in parturition. In whatever way it may happen it is always a very serious accident, the hemorrhage being often so great as to threaten and in some cases destroy life.

To check loss of blood from the vulva, the hips should be raised considerably above the level of the rest of the body, for the sanguineous fluid in varicose veins is peculiarly under the influence of gravitation, and will run hither and thither in obedience to it, and in accordance with the varied positions of the body.

(c.) *Hematoma*.—Vulvar hemorrhage instead of escaping externally, sometimes finds a home in the lower cellular tissue of the labia, and produces tumours varying in size from a hazel nut to a cocoa nut. Its etiology is similar to that just described under the heading "hemorrhage."

After the effusion is completed and the tumour fully developed, postural treatment will of course be of little avail, but if it be detected early the dimensions of the clot may be materially curtailed by elevating the hips of the patient.

(d.) *Hypersecretion*.—Vulvar leucorrhea, or an increased flow of mucus from the pudenda is a normal and not unfrequent result of the active hyperemy produced by sexual excitement. This is of an intermittent character. The flow of mucus produced by passive hyperemy is more or less continuous, and results very commonly from sitting too long in the upright posture. Hypersecretion may be looked upon as one of the best signs by which inflammation and hyperemy may be differentiated, as in the former secretion is impeded, and in the latter augmented. Passive hyperemy from whatever postural cause, produces increased action in the vulvar glands and follicles, and occasions, if not an actual escape of mucus, an undue moisture of the external genitals.

The lateral reclining and recumbent postures will be found to diminish materially vulvar hypersecretion.

(e.) *Edema*.—A dropsical tumefaction of the labia is not uncommon. It is generally due to the mechanical forms of hyperemy, and more especially to venous compression produced by gravitatory descent of the uterus during pregnancy.

It is capable of being greatly relieved by postural treatment. The patient should be recommended to assume, and remain in, as far as is practically convenient, such a position as will favour the removal of the obstructing body from its point of contact with the compressed vein.

(f.) *Hyperesthesia*.—Pruritis of the vulva is an exceedingly distressing affection. It is undoubtedly often of hyperemic origin, and any of the passive forms may produce it. Exacerbations occur when any active hyperemy temporarily invades the part, and the irritation is much increased, and perhaps sometimes originated by the mucous secretions, resulting from vascular distension.

To the same causes also may be attributed the unnatural sexual excitement which sometimes torments unhappy patients. The clitoris and neighbouring parts when hyperemic become super-sensitive and perpetually annoying.

When these symptoms exist all postural sources of hyperemy should be carefully avoided. Dorsal reclamation and soft feather beds which allow the pelvis to sink below the level of the rest of the body, should be especially eschewed.

(g.) *Hypertrophy*.—Hypernutrition is the natural consequence of increased blood supply. The labia, *nymphæ*, and clitoris are the parts of the vulva most frequently hypertrophied. These affections, however, are of rare occurrence, and in this country seldom attain such importance as to attract attention or demand treatment.

(h.) *Ulceration*.—When a mucous membrane becomes highly hyperemic, it is apt to become softened. Its epithelium from hemorrhagic and serous effusions or other causes then loosens and is removed, leaving a denuded surface which often has little tendency to heal and is liable to provoke and keep up active hyperemy in the part in which it is situated. At the orifices of the vagina and urethra these ulcerations cause great local pain and constitutional disturbance, some-

times they assume the form of fissures, and at others irregular proliferation takes place, resulting in a troublesome crop of abraded vegetations.

Postural treatment will in these cases be found a valuable adjunct to other suitable remedies.

### 3. *The Urethra.*

The hyperemic disorders of the urethra being constantly subject to exacerbations from the passage of urine, seldom escape attention for any lengthened period. The amount of discomfort also, which they produce is so great that patients cannot long endure it, and are therefore speedily induced to seek relief.

(a.) *Varix*.—The numerous veins which surround the urethral tube are liable to become enlarged and tortuous when the reflux of their contents is impeded. At the meatus more especially, where the vessels have no controlling counter-pressure, vascular tumours frequently develop which bleed on the slightest provocation and cause great distress.

The postural treatment must in these, as in many other instances, be either prophylactic, or palliative. Alone it can scarcely be expected to be frequently radically curative. The upright posture always augments the vascular fulness and material alleviation is produced by recumbency.

(b.) *Hemorrhage*.—Escape of blood from vascular urethral tumours is sometimes very severe, the slightest touch determining it, its control may be assisted by placing the patient on her back with the hips raised.

(c.) *Hematoma*.—Blood effused into the peri-urethral tissue, which may be caused by the rupture of a varicose vein, may assume considerable importance in consequence of its compressing the urethra and causing the retention of urine. Strictly speaking, however, this perhaps ought to be considered further on under pelvic hematoma.

(d.) *Edema*.—The urethral mucous membrane is occasionally liable to become swollen by serous infiltration, and may thus have its excretory functions seriously impeded. The influence of position in relieving this condition ought not to be overlooked.

(e.) *Hyperesthesia*.—The internal surface of the urethra, and more particularly its orifice, sometimes becomes intensely sensitive, and the act of micturition is in consequence dreaded and postponed as long as possible. Posture has a marked influence in increasing and diminishing this pain, it being almost invariably worst at the end of the day, and relieved in the morning by the night's recumbency. The intensest pain is sometimes felt in the little highly hyperemic tumours already referred to. But all this undue sensitiveness may be much relieved by postural treatment, and by it appropriate local remedies may be much aided.

(f.) *Hypertrophy*.—The urethral mucous membrane is subject to enlargement, not only by infiltration and vascular distension, but by proliferation of its proper tissues resulting from passive hyperemy. In this condition it is liable, as has already been stated, to become prolapsed and appear at the meatus in the form of a ring-shaped tumour. Rest in the recumbent posture will assist other appropriate treatment.

#### 4. *The Bladder.*

This organ is subject to all the ordinary disorders which have their origin in passive hyperemy, and they are also capable of being much relieved by posture, more especially by dorsal recumbency with the shoulders placed lower than the hips. This position will be found to diminish the distension of the vesical veins and sensibly relieve all irritation and hyperesthesia.

#### 5. *The Vagina.*

The extreme vascularity of the vagina causes it to suffer frequently from hyperemic disturbances, and the large venous plexuses which surround it are more particularly liable to be affected by postural influences. When the veins become obstructed by pregnancy or other mechanical causes, the walls of the vagina assume a purple hue, and are sometimes spotted with dark patches of extravasated blood. It should be remembered that as the veins and arteries run side by side, the same mechanical obstruction which retards the



venous circulation must also impede the arterial, and as a consequence a form of hyperemy similar to the hypocardiac must generally co-exist. When the force of the affluent blood is diminished its refluxion must be proportionately lessened.

(a.) *Varix*.—A prolonged hyperemic condition of the vaginal veins leads gradually to their dilatation, and they may at length be seen and felt projecting beneath the surface of the mucous membrane. This unnatural and dangerous state of the vaginal veins is generally due to the prolonged maintenance of the upright posture during pregnancy, but it may have a hypostatic origin. If pregnant women would avoid this serious complication they must never for any length of time stand, or sit erect in a chair. They may walk at appropriate intervals, but when at rest they should, as a rule, recline or lie down.

(b.) *Hemorrhage*.—The formation of varicose veins in the vaginal passage, besides acting as a mechanical impediment to the progress of labour, is a source of great danger from the violent bleedings which may occur. Fatal hemorrhage from ruptured varices in the vagina is not very rare. When it occurs, postural treatment will be found of great service. By raising the hips and lowering the shoulders the flow will be immediately diminished, and other hemostatic remedies will have their power increased.

(c.) *Hematoma*.—Bloody tumours in the walls of the vagina are sometimes observed arising from the rupture of distended veins. This may occur in labour during the descent of the child's head, or from any cause which may suddenly increase the lateral pressure upon the walls of the veins. (See pelvic hematoma.)

(d.) *Hypersecretion*.—Vaginal leucorrhea is one of the most common and persistent of all the disorders to which women are liable. There must be some general infringement of nature's laws to account for this. It is originated, no doubt, in many different ways, but posture has certainly an important influence in its production. Some of the more active causes are, while they last, perhaps more potent than the passive; postural hyperemy, however, in its three forms is

so continuously acting and increasing in energy when improper positions are persistently maintained, that it is impossible not to assign to it a considerable share in the etiology of vaginal hypersecretion. Prolonged standing, sitting, and dorsal reclination are frequent causes of this disorder, and during pregnancy it often occurs in consequence of the pelvic venous stasis produced by uterine pressure.

Position has the same power of relieving vaginal leucorrhea as it has of initiating it. Moderate walking exercise should not be avoided ; but when at rest, the lateral or prone reclining, and recumbent postures will be found not only to diminish the discharge, but to relieve the accompanying symptoms.

(e.) *Hypertrophy*.—Besides the increased development of the mucous lining of the vagina, fibroid, cystic and polypus growths are sometimes met with, all of which may have their origin in postural hyperemy or in effusions arising from it.

(f.) *Hyperesthesia*.—When a mucous membrane becomes hyperemic its nervous susceptibility is also apt to become exalted. This may appear as vaginal pruritus and vaginitis, two very distressing disorders, notoriously difficult to cure, and not unfrequently owing to the resulting dyspareunia embittering and wrecking connubial happiness. In the treatment of these cases the medical man with other therapeutic or operative means will be wise to avail himself of any assistance he may possibly gain by prescribing such postures as have a tendency to reduce pelvic hyperemy.

## 6. *The Uterus.*

Of all the organs in the body the uterus is most liable to suffer from hyperemic disturbances. During the sexual life of a woman, it is never for one moment in a quiescent state. The changes it undergoes in fulfilling its various functions are peculiar to itself, and have *no* physiological analogues. It is profusely supplied with blood-vessels, which are ever receiving fresh affluxes of blood to supply the demands of its various functional actions. In rapid succession active hyperemies take possession of the uterus. If at the same time

its vessels should be habitually distended by impeded venous reflux the organ must necessarily increase in size, and present in due time all the usual hyperemic *sequelæ*.

(a.) *Varix*.—An unduly dilated condition of the uterine veins is in all probability not a very unusual affection. It is rarely observed in post-mortem examinations, because after death all vascular distension ceases and the abnormal appearance becomes so obliterated as to escape attention. Occasionally varices of the cervix are detected during life by means of the speculum and their existence is also sometimes made known during or after labour in consequence of the hemorrhage which their rupture causes—this condition, although perhaps more frequently found in the neck of the womb, is not confined to it. It doubtless often exists in its body, where, in fact, varicose aneurism has been noticed. It is difficult not to attribute to a permanently dilated state of the uterine veins some of those tiresome cases which baffle all treatment and continue for years to give symptomatic evidence of firmly established hyperemy.

Prolonged postural anti-hyperemic treatment will certainly reduce the varicose distension, and give the veins a chance of returning to their normal calibre.

(b.) *Hemorrhage*.—Although sometimes so disastrous in its results, it is perhaps on the whole a great advantage that an organ so profusely supplied with blood as the uterus, should be able to part with its superabundance readily. Metrorrhagia, or bleeding from the internal uterine surface, frequently takes place in consequence of simple passive hyperemy. If the superficial vessels, either from gravitation or obstruction become too full, like a safety valve, they open and an attempt is made to restore the equilibrium. When morbid conditions of hyperemic origin also exist, hemorrhage is still more liable to occur, for by the active hyperemy which their irritation causes, the veins become doubly distended and the hemorrhage proportionately increased.

The employment of posture in the treatment of metrorrhagia is most satisfactory. Its influence for good or evil is so vividly conspicuous, that it cannot possibly be disregarded. If a woman flooding be made to assume the upright posture

for a short time, the effect of gravitation upon the blood by accelerating its flow at once becomes evident. It is most important, therefore, that every woman suffering from uterine hemorrhage should be immediately placed in a recumbent position with the hips raised as far above the level of the shoulders as can be conveniently effected.

(c.) *Hematoma*.—Extravasations of blood, sometimes called uterine apoplexies, are generally caused by the rupture of varicose or weak-walled vessels. They are also probably determined by sudden hyperemic action, for it has been observed that the cases in which they have occurred have been accompanied by sanguineous effusion from the internal uterine surfaces.

In all cases of metrorrhagia, as has just now been advised, recumbency should be observed. This injunction must have its strength augmented, when it is remembered that blood may at the same time be escaping in the uterine tissue, and that the size of the resulting clot must be regulated by the intensity of the hyperemy, whether it be of an active or postural origin.

(d.) *Hypersecretion*.—Every gynecist is only too well acquainted with the glairy tenacious mucus which hangs about the os uteri, and can scarcely be wiped away. He also knows when he sees this secretion that he has before him a case which will probably prove very obstinate, and test his resources and patience to the uttermost. The cause of this discharge is a hyperemic condition of the cervix, and its source is the glandular structure of the cervical canal. Perhaps the cervix being the lower portion of the uterus is more frequently subject to hypostatic hyperemy than the body, but however this may be, any increase of blood supply acts at once upon the glands, and results in mucous hypersecretion. Immediately after denudation, and before the glandular structure of the body has become developed, of course all mucous flow from that portion of the uterus must cease. Any excessive escape of mucus from the os uteri must be looked upon as an unmistakable symptom of hyperemy. Whether this be of an active or passive character, the history of the case can alone decide. Whatever decision,



however, may be arrived at, the adoption of postures unfavourable to pelvic hemostasis will be found materially conducive to the success of other appropriate treatment. No remedy will permanently cure uterine leucorrhea if the patient from choice or necessity continues to spend the greater part of her life standing or sitting in the upright or semi-erect positions.

(e.) *Edema*.—A hyperemic condition of the uterus cannot long exist without being accompanied by watery effusion into its substance. In consequence of this transudation the organ becomes larger, softer, and more liable to displacement. Fibroid tumours of the uterus are also liable to become edematous, and they and it when affected in this way, are apt to vary rapidly in size according to the posture assumed. A night or a day or two spent in bed reduces their bulk very sensibly.

This fact sufficiently indicates the appropriate postural treatment.

(f.) *Hypertrophy*.—The increased afflux of blood which takes place during the processes of nidation and gestation results in the normal hypertrophy of various parts of the uterus. If a similar increased supply of nutritive material be produced mechanically, new formations also arise, and the mucous muscular and connective tissues of the uterus become overgrown. Uterine hypertrophy in its various forms is exceedingly common. It may be diffused, circumscribed, partial, &c., but whatever shape it may assume, it may originate entirely from hyperemy of a passive character. It is curious that these growths may themselves be affected by the same form of hyperemy which brought them into existence, the most dependent portions of them becoming distended by hypostatic hyperemy, followed by hemorrhage, hematoma, edema, ulceration, and disintegration. How far these new formations are capable of degenerating into those of a malignant type has not been positively decided. Proliferation of the connective tissue seems to a certain extent to have a curative influence by rendering the uterus so dense and hard as to resist further hyperemic distension. On the other hand, when it exists in undue proportion, it supplants

the other tissues, and seriously interferes with the functions of the organ.

It is not uncommon to be able to trace hypertrophic conditions of the uterus to passive hyperemy of postural origin. Once in existence they have a twofold power for evil. By their presence they determine active hyperemy in their own immediate region, and by their size and weight they displace and press upon the bloodvessels, and increase the hyperemy, which originally produced them. Postural treatment, therefore, must not be neglected in these cases. By its means the hypostatic form of hyperemy must be combated ; and by the maintenance of suitable positions, the mechanical hyperemy due to displacements ought to be relieved if not remedied.

(g.) *Ulceration*.—Simple erosion of the cervical, or most dependent portion of the uterus, is extremely common. It is usually due to a hyperemic or varicose condition of the mucous membrane, and according to its site and duration, assumes various forms and degrees of intensity. It may be caused from without by irritating discharges, or from within by serous or sanguineous effusion ; but these it will be observed are secondary hyperemic affections, still further tending to show how largely ulcerations of the uterus may originate in a hemorrhagic distension of its vessels.

In the treatment of these lesions, position has a marked influence. The recumbent posture always alleviates them, reduces the pain and discharge which accompanies them, and in some cases supplemented by simple hygienic rules, proves sufficient in recent cases to affect their cure.

(h.) *Hyperesthesia*.—A great deal has been written upon this subject under "irritable uterus," and various other names. Hyperemy of the uterus is seldom long disassociated with undue sensitiveness ; and that this hyperemy is intimately connected with posture is proved by the fact, that the uterine pain is brought on or increased by kneeling, sitting, or standing, and sometimes even by the slightest change of position.

Recumbency, as a rule, relieves this painful affection, and this is particularly necessary when any functional active

hyperemy also makes its appearance. Removing every tendency to pelvic blood stasis, will materially assist other appropriate treatment.

### 7. *The Fallopian Tubes.*

Although diseased states of the Fallopian tubes are often difficult to diagnose accurately during life, and all signs of vascular distension in them disappear, or are difficult to detect after death, there can be no doubt that they are frequently affected by hyperemy and varicosity, leading to all the consequent morbid conditions.

Obscure as these are when they occur in the tubes, the practitioner will occasionally be able to give relief by remembering the influence of posture. If the right be hyperemic, the left lateral recumbent position will alleviate, and *vice versâ*. If, on the other hand, the right be hypertrophied or distended, the painful dragging sensation will be removed by lying upon the side affected.

### 8. *The Ovaries.*

An organ which is constantly and directly subject to the active hyperemy accompanying ovulation, and indirectly to fluxions sympathetic with other generative functions, must be peculiarly apt to have these vascular repletions augmented and perpetuated by the passive or postural forms of hyperemy.

(a.) *Hemorrhage*.—Bleeding from hyperemic or varicose vessels rupturing upon the surface of the ovary, has been known to take place to such an extent as to cause death. It is, as we shall see further on, a not very rare cause of pelvic hematoma.

(b.) *Hematoma*.—Ovarian apoplexy, as it is sometimes called, is not uncommon. In post-mortem examinations effused clots are found, varying from the size of a pea to that of an orange. The escape of blood may take place either in the follicles or the stroma, and although the amount poured out may be influenced by position, the time of its occurrence must be so uncertain as to render postural treatment of little avail.

(c.) *Hypertrophy*.—Increased growth of the stroma, and more particularly of the tissues surrounding the follicles in the ovary, is a hyperemic consequence of the deepest interest; for this appears to be the commencement of the enormous cysts which prove fatal to so many women. “The starting point of this increase of liquid is almost always more or less prolonged hyperemy of the ovaries. This hyperemy, as may be easily understood, is communicated to the walls of the follicles, and is thus the cause of the hypersecretion which takes place upon their internal surface. But in order that the liquid thus secreted may remain in the Graafian follicles, it is necessary that the rupture of the walls of the latter be rendered impossible in consequence of an hypertrophy (Scanzoni).

(d.) *Hyperesthesia*.—Ovarian hyperemy is seldom unaccompanied by pain more or less acute. Sometimes it is excessive and surrounded by symptoms of a complicated and various character.

Opharalgia is generally relieved by recumbency. Exercise, which is so necessary for the health, may be taken with tolerable comfort, if the ovaries be supported by a suitable mechanical appliance.

### 9. *The Rectum.*

No one fails to observe the influence of posture in producing rectal disorders. Some of them are so common and are so generally attributed to passive hyperemy, that it would be a waste of time to bring evidence in support of the fact; at the same time it must be admitted, that the potent influence which rectal diseases has upon the other pelvic organs is not sufficiently appreciated, nor is their mode of detection and treatment carried out with the assiduity which their importance demands.

(a.) *Varix*.—Undue distension of the hemorrhoidal vessels is the cause of a large amount of discomfort and continuous acute pain among women. Sitting a great many hours during the day at an occupation which demands little or no movement of the body, and venous obstruction caused by the pregnant uterus, tumours or feces pressing upon the



trunks of the larger veins, are the more common conditions which produce varicosity.

(b.) *Hemorrhage*.—Distended hemorrhoidal veins bleed easily and freely, and sometimes to such an extent as to seriously affect the health of the patient and demand treatment. The hemorrhage can always be checked by the recumbent posture with the hips raised.

(c.) *Hematoma*.—Piles, which are produced by an escape of blood from the varicose veins into the tissue about the anus, are too well known to require more than mere mention. The influence of posture is also well understood, for the patient soon finds how much the pain is aggravated by standing, and relieved by lying down. The proper treatment is thus suggested in a forcible way, and if possible usually adopted at an early period.

(d.) *Ulceration*.—Other very painful results of rectal venous distension, are the ulcers and fissures which appear at the margin of the sphincter. These are not unfrequently found when the hyperemic or varicose condition of the region has become chronic.

Posture relieves or increases the pain, but it can only be in very slight cases that a cure can be expected to result from the most careful observance of the recumbent position. When the surgeon has done his work however, he knows well what advantage is to be gained by keeping his patient in bed during the healing process.

(e.) *Hypertrophy*.—Under the influence of increased blood supply, the rectal mucous membrane is liable to become softened and hypertrophied, and in this condition is not unfrequently subject to prolapsion. The enlarged and edematous mass appears outside the anus when the upright posture is long maintained, or during exertion.

In returning it, the knee-elbow position will be found most convenient, and it can be kept in its place by recumbency whilst other proper treatment is employed.

#### 10. *The Pelvis.*

General pelvic hyperemy due to posture must be a condition constantly recurring. By gravitation the blood

naturally falls into the bony basin at the lowest part of the erect body, and in this position the venous reflux is liable to be impeded by organs descending and compressing the larger blood-vessels. The hyperemy thus produced may be short lived and evanescent, but if the erect posture when at rest be adopted solely during the greater part of the day for months or years, permanent enlargement of the vessels of the whole pelvic viscera must take place, and the entire cavity must continually contain too large a quantity of blood. The symptoms which this condition produces are feelings of fatigue, weight, and aching ; or sometimes actual pain results, increased by walking or driving, and causing the patient to assume a bent position when standing or walking.

Pelvic posture has also a decided influence in producing passive hyperemy of this region. Debased pelvic inclination, which has already been alluded to (p. 31), by allowing the viscera to gravitate more directly into the cavity of the pelvis, favours displacement of the organs and vessels, and consequently the existence of mechanical hyperemy. An erect bearing is as necessary to prevent hyperemy as displacement.

What may be called exercise of the pelvic viscera seems to be necessary for their well-being. The pelvic veins appear to depend in a great measure upon the pressure from without which the constant movement of the parts surrounding them produces, for accelerating the onward flow of the blood which these contain. The unyielding walls of the pelvic cavity considerably reduce this expediting influence, and it is again further curtailed when sitting, by the floor of the pelvis also becoming an unyielding wall. That this exercise is necessary to insure free circulation, seems proved also by the fact that any morbid condition which has the effect of fixing the pelvic organs, invariably results in increasing the hyperemy of the parts affected.

(a.) *Varix*.—The venous trunks and plexuses which lie embedded in the pelvic tissues, are probably more frequently than is generally supposed liable to become varicose. Although this condition may produce no well-defined symptoms, and may after death be difficult to discover, nevertheless

it may lead, as will be presently shown, to very serious if not fatal consequences. When the parts accessible to the eyes, the vulva, vagina, uterus, and rectum, are found to be varicose, a similar condition of all the hidden intra-pelvic contents may be suspected, and more particularly of those large venous plexuses which convey the blood from the vagina, uterus, and ovaries.

Maintaining the body motionless in the erect posture is the most frequent cause of varix, with the exception of the pressure of the gravid uterus. A striking example of the influence of position in producing varicose veins is shown by the immunity which tailors enjoy from this affection in their legs. Always resting in a horizontal position, the veins of their limbs are never subjected to undue distension by blood gravitation. If women when at rest would lie down, they would also escape many hyperemic troubles of the pelvic organs.

(b.) *Hematoma*.—Pelvic hematoma has been divided into extra-and intra-peritoneal. Rupture of a distended pelvic vein is perhaps the most frequent cause of this dangerous escape of blood. When intra-pelvic varix exists, hematoma may be determined by long continuance of the upright position, by a fall, or by any sudden exertion, and either of these acts more potently at the periods when, owing to some functional action of the reproductive organs an active hyperemia of the pelvic contents exists. The veins in the broad ligament are those which give way most frequently, but the tubal, uterine, and vaginal may each be the seat of the lesion. The point of rupture determines whether the blood effused shall collect in the peritoneal cavity or in the extra-peritoneal connective tissue. When the hemorrhage is intra-peritoneal, posture has much to do with deciding at what point the resulting hematoma shall be. In every position except the lateral and prone recumbent, the blood naturally gravitates into the retro-uterine pouch, and here is consequently most commonly found the site of pelvic hematoma. The situation of the extra-peritoneal variety is not so much subject to gravitation, its position being determined by the anatomical boundaries of the point of rupture. There is

scarcely a region, however, in the neighbourhood of the pelvic organs in which blood tumours of varying sizes may not be found.

It is well-known that pus has a tendency to burrow by gravitation and abscesses to open at the most dependent parts; it may be worth considering whether posture might not be advantageously employed in endeavouring to direct the contents of suppurating hematomas to a safe site of exit. Position may certainly be used with good effect, if the patient be seen sufficiently early, in controlling the amount of blood effused by ordering her to lie down with the hips raised.

(c.) *Thrombosis*.—A serious result of postural hyperemy is the formation of clots in the stagnant blood of varicose veins. During pregnancy the womb may fall and so compress the iliac veins as to cause distension of the whole venous system of the pelvis and lower limbs. In the most largely dilated portions of these vessels the circulation may be so tardy as to be insufficient to preserve the blood in a fluid condition, and coagula may form. Should this take place, a period of danger arrives when parturition is over and the distended vessels begin to resume their normal calibre. The thrombi are then liable to disintegrate and become detached and displaced causing embolism, and sometimes death by entering the pulmonary artery.

Should this accident occur, absolute rest in the recumbent posture is necessary. Sudden death may result from the patient assuming the erect or sitting posture. In all cases of varicosity caused by pregnancy, exercise should be very carefully commenced after delivery.

(d.) *Edema*.—Serious effusions into the pelvic connective tissue, although they may be primarily determined by active hyperemy, are nevertheless capable of being much increased by posture. It is remarkable how rapidly the swelling in the roof of the vagina will grow larger or subside as the erect or recumbent positions are assumed.

It is always safer when these swellings are detected, to advise recumbency, for it is impossible to tell whether the tumefaction will turn out to be simply an evanescent exuda-



tion, or a plastic effusion ending in inflammation and supuration.

(c.) *Hyperesthesia*.—Pain or uneasiness is commonly caused by general pelvic hyperemy. An active hyperemic condition of the pelvic contents, which may be painless when in the recumbent position, will often prove intolerably painful when the passive hyperemy of the upright posture is super-added. Relief can only be obtained in one way, and pain, the great protector of the body in such cases, emphatically orders the sufferer to lie down and unload the pelvic vessels.

Another form of pelvic pain is caused by the pressure of the enlarged uterus upon the sacral nerves. In a hyperemic state it is peculiarly subject to displacements, and the consequent dragging upon the nervous filaments is a frequent source of uneasiness, the nervous disturbance produced by one or other of these causes, sometimes only amounts to a feeling of numbness, but occasionally it reaches a degree of pain almost unbearable. Both the numbness and pain may radiate to the trunk, limbs and head, and relief can only be obtained by the patient assuming such an attitude as will relieve by gravitation the nervous stretching or compression.

---

## A CASE OF RUPTURE OF THE UTERUS WITH PROTRUSION OF INTESTINE.—COMPLETE RECOVERY.

By O. LOWSLEY, M.R.C.S., L.S.A.

ON November 11th, 1873, at 9 o'clock, P.M., I attended Mrs. F., aged twenty-eight, in her fifth labour; her pains came on regularly every five or six minutes, and on examination, the os uteri was high up, but sufficiently dilated to admit two fingers. At 10 o'clock, on examination, the os uteri was well dilated and a foot and hand presenting. After slight manipulation, I succeeded in pushing the hand back, and pulling a foot down. I now left the labour to its natural course, and my patient was delivered of a male child at ten minutes to eleven. After waiting fifteen minutes, I attempted to remove the placenta, but finding my efforts

fail, slight traction was now made, but did not succeed; waited again a few minutes, introduced my hand to the insertion of the cord, and made gentle traction, but it gave way, being very gelatinous. Being obliged to introduce my hand into the uterus, there finding something like a portion of placenta, I brought it down, and to my great surprise discovered it was a loop of intestine, which I immediately returned. Seeing now the gravity of the case, I at once sent for Mr. Moxhay. In the meantime I gave my patient six ounces of brandy, as she appeared rapidly sinking into a collapsed condition, but she somewhat recovered by the time Mr. Moxhay arrived, about 12 o'clock. Having explained the case to him, he succeeded in removing the placenta, but on examining the uterus through the abdomen, it was found not contracted so firmly as usual, and sometimes it felt as if it dilated again. We now bound a book very firmly over the uterus, and gave her 30 drops of tincture of opium, and ordered ten drops to be taken every hour with beef tea, as her pulse was becoming very quick, and her hands and limbs cold. She rapidly sank into almost complete collapse. We now explained to her husband the danger of the case, in order that he might understand that neither of us expected his wife to live many hours. On visiting her again next morning at 8 o'clock, I found that she gradually recovered from her collapsed condition at 5 A.M., but complained of great thirst, and pain in the abdomen over the uterus. She had been sick several times after taking beef tea and medicine, pulse 148, and had not passed any urine. At 1 P.M. I drew off about a pint of urine, and at 5 A.M. next morning I was sent for, as it was supposed she was dying, she having vomited and fainted afterwards. I gave her more brandy and 25 drops of tincture of opium.

November 13th, at 3 A.M., pulse 160, great pain in abdomen, much increased in size, sick after everything she had taken. I drew off nearly a pint of urine, and ordered linseed poultices to abdomen and fomentations. At 2 P.M. vomiting and retching much increased, and the case was distressing to witness. I gave her again 25 drops of tincture of opium and 5 drops of tincture of belladonna every

hour. I thought she could not live more than a few hours.

November 14th, vomiting quite ceased after two doses of the medicine. She voluntarily passed urine; abdomen very tympanitic, pulse 125. Tongue coated at centre, but red at edges; slept a little in the night.

November 15th. Abdomen less swollen; was attacked with diarrhea in the night, bowels were open seven or eight times, the motions now ran away involuntarily. Tongue very much coated, pulse 130, gave a pill of acetate of lead, gr. 1, pulv. opii gr.  $\frac{1}{2}$ , every three hours, and a starch enema with tincture of opium 25 drops.

November 16th. The bowels have not acted since the enema; the abdomen much less tympanitic. Pulse 98, looks cheerful, and takes nourishment well.

November 17th. Bowels are not open, sleeps well at night, tongue much cleaner, breast flabby, complains of shooting pain in the uterine region. Still a discharge from uterus, and not much changed in colour.

November 18th. The milk only came into the breasts last night; they are full and rather uncomfortable. Bowels have acted naturally, pulse 80, tongue cleaner, and abdomen much flatter. Ordered cold water to breasts.

November 19th. Continues to improve.

November 30th. Is much stronger, but passes a good deal of discharge from uterus, and something like membrane is occasionally discharged.

On December 20th, she appeared to be quite well.

Rupture of the uterus is the most serious complication of labour. Smellie and Denman both thought that nothing could be done for the patient but leave it to nature.

Dr. Trask, who has collected 300 cases, states that only 128 occurred during parturition; of these, 13 occurred in the posterior part of the uterus, the spot at which, I believe, the rent took place in this case. There was not any disproportion of the pelvis in the case of Mrs. F., having had four living children before. Dr. Collins states that the cases which happen as a rule are not those in which there has been severe labour.

Dr. Murphy, who has written a paper on Ruptured Uterus, says in the majority of cases the rupture is caused by the uterus being atrophied, softened, and thinned. In my case there was not much hemorrhage externally, and this Dr. Churchill states in his work to be the usual condition.

Mrs. F. informed me that the last eighteen months she had suffered from a discharge from the uterus, and every time she went to stool there had been a bearing down. For six weeks before her labour there was a large discharge of water from the uterus every day, occasionally tinged with blood, and she very much dreaded her confinement.

In this case I can positively affirm that no violence was used. The cord broke from its extremely gelatinous nature. On introducing the hand to remove the placenta, it encountered the intestine. The whole of the case turns upon the fact that the intestine not only was in the uterus, but was drawn down sufficiently to be certain of the nature of the substance.

The case is all the more interesting from the fact of the recovery of the patient.

*Note.*—It could not possibly have been rupture of the vagina, as I traced the cord to its insertion before it gave way, and my hand was in the uterus for removing the placenta when I found the intestine. I was told by Mrs. F. that she suffered for several months previously with a fixed pain in the left side, and she quite recollected the feeling that something had torn and given way just before the child was born; after that she lost all consciousness, and for some hours after the accident did not recover her collapse.

Drs. Ramsbotom, Barnes, McKeever, and others, relate cases of rupture of the uterus with protrusion of intestine, and Dr. Ramsbotom warns you not to let the intestine get dragged in and nipped; but, on the other hand, Dr. Murphy objects to any attempt to replace intestine, and says there is very little risk of strangulation.



*Addendum, Feb. 17th, 1875.*

Fourteen months after the recovery of the patient, I received information from Mr. Smith, of Clifton, that he had attended Mrs. F. in that place, where she had died of phthisis. Mr. Smith was good enough to make a post-mortem examination. His account of the post-mortem is as follows:—

“There is a star-shaped cicatrix with central irregularly shaped white patch in the peritoneum covering the back of the posterior wall of the body of the uterus, the precise situation being as nearly as possible the middle of the posterior wall of the body. The left ovary was enlarged to the size of a hen’s egg, the right ditto shrivelled and wasted. The intestines were greatly thinned. The liver slightly enlarged, the remaining abdominal viscera healthy. Owing to the presence of relatives I could not remove the uterus and ovaries.”

I think this fully bears out my diagnosis of the case, both as to the character of the injury and the spot at which the rupture took place.

---

## RETROVERSION OF THE GRAVID UTERUS— RETENTION OF URINE.—RECOVERY.

By Dr. ARTHUR W. EDIS,

Assistant Obstetric Physician to the Middlesex Hospital; Physician-Accoucheur  
to the British Lying-in Hospital.

C. G., aged twenty-five; married five years; mother of two children, youngest two years. Last catamenia three months since. Presented herself in the out-patient department on Dec. 19th, complaining of severe pain in the lower abdomen, with swelling, and inability to hold or pass her water, there being also much difficulty in getting the bowels to act, with much bearing-down pain. The patient was unable to lie on either side, and had been unable to do any work for the last three weeks.

About the beginning of December, on attempting to pass water one evening, she found she was unable;

but during the night and the following day, "it came so quick she could not stop it." And since then she had been in a constant state of discomfort from the continuous flow of urine. On examination *per vaginam* the pelvis was found to be blocked up by an enlarged retroflexed gravid uterus, at about the third month of utero-gestation. The fundus was low down, pressing on the perineum, and causing a distinct rectocele. The os was hardly to be felt, high up beyond the pubis, above which, externally, a tense, firm, resisting swelling, with indistinct evidence of fluctuation, occupying the centre of the abdomen, and extending up beyond the umbilicus, was detected.

A No. 8 elastic catheter was passed with some difficulty in a vertical direction, and 94 ounces of turbid, ammoniacal urine were drawn off. The patient was then supported in the knee-shoulder position, two fingers of the left hand were passed per rectum, and the fundus guided round the right side of the promontory of the sacrum, the os descending to the normal position, lowest in the pelvis. A Hodge was inserted, and the patient instructed to relieve the bladder frequently. Ergot, strychnia, and bark were prescribed.

On the 21st she stated that the difficulty in holding her water had returned at 11 P.M. on the 19th, and the urine had run away from her ever since, the Hodge having been forced out the following day. On examination the condition of affairs was the same as when first seen, 86 ounces of turbid urine were drawn off, and the patient lying on the left side, the uterus was replaced as before, and a larger Hodge inserted.

On the 23rd she stated that she had had perfect control over the bladder since the last examination. The uterus was now found to be in its normal position, the os being lowest. 12 ounces of clear, healthy urine were drawn off to see if there were any further retention; the Hodge was *in situ*.

On the 30th she was seen, and was perfectly comfortable, there being no return of the difficulty in micturition. Hodge *in situ*. She has been seen twice since this and remains convalescent. Pregnancy advancing normally.

## Reports of Hospital Practice.

---

### GLASGOW WESTERN INFIRMARY.

#### HYSTERICAL (?) PARALYSIS AND COUGH IN A CHILD. RECOVERY.

*Under the care of DR. FINLAYSON, one of the Physicians  
to the Infirmary.*

MARGARET M., aged twelve, was admitted on February 26th, 1875, from an orphanage home in the city, where she had lived for three years. She was sent in with a hoarse, croupy cough, somewhat resembling an aneurismal cough, but remarkable in its being a single expulsive effort, and never assuming a paroxysmal or crowing character. The account obtained on her admission was very meagre, and some anxiety was felt as to whether serious laryngeal mischief might not supervene. A variety of remedies—steaming, sprays, poultices, and medicines internally—failed to produce any impression on the cough, which continued as before—neither better nor worse. The child seemed pale and delicate, but no evidence of disease in the lungs could be obtained. Some trace of dulness at the upper part of the sternum, and in the interscapular regions, coupled with a very slight elevation of the temperature, and the weakly aspect of the child, raised the suspicion of the peculiar cough being due to scrofulous glands in the mediastinum. She was put under treatment with a little wine and iron and chloric ether.

The day after admission it was stated by the resident physician, that the child was also paralysed in her legs, and could not walk. Little attention was directed at first to this, as it was supposed, from the absence of wasting, &c., to be some hysterical affection, and the laryngeal cough appeared the more urgent matter, and confinement to bed seemed desirable on that account. About a fortnight after admis-

sion the child was tried at walking : she limped considerably and complained of thus being hurt ; but it was pointed out to the students in attendance that everything seemed to hurt her, and she complained no matter where she was touched. On urging her to walk more quickly she cried much, and in her apparently weakly state, it was not deemed expedient to press the matter too much. As, however, she complained much on even being made to stand up in bed—her legs giving way below her—a careful examination was made (March 27th) to see if there might be any hip-joint or other mischief hitherto overlooked, but the result was completely negative, and it was now plain that the child should be compelled to walk, and she was accordingly clearly told that she must get a whipping if she refused ; the nurses made her walk a little that day with assistance, and in a day or two she walked without a trace of limping, and these efforts were duly encouraged.

The cough, however, remained as before ; the larynx had been examined with, practically, a negative result ; the cords were normal, although some congestion was supposed to exist below their level. A small blister was applied over the trachea, just above the sternum, on March 29th, and the effect was almost magical : *she was scarcely heard to give a single cough from that time onward.* Tonics and cod-liver oil were subsequently given, and the child was dismissed to the sea-side home at Dunoon on April 19th.

Some pains were taken to ascertain the previous history of this child's illness. The cough had dated from fifteen months before admission, and had come on (the matron of the orphanage stated) after getting a dose of cod-liver oil which had been ordered for her, as she was supposed to be threatened with consumption, and the remedy was not pushed. It had continued since then ; the matron thought the child coughed more when any visitors were present. Certainly in the Infirmary the occasional cough continued even when no one could be supposed to be paying any attention to her, as she lay in a side room at first, along with a companion from the same orphanage. The child had been withdrawn from school for the last twelve months



on account of her health, but she had received a fair education, and seemed a very intelligent girl.

The lameness had appeared six months before admission (August, 1874), when she complained of pain and weakness; but next day, when it was announced that the children of the orphanage were to go to the coast for a holiday, she got up, and at the sea-side played about like the rest. Since then she had had several recurrences of this lameness, lasting for a week or so at a time.

*Remarks.*—There seemed to be no doubt, on subsequently interrogating the patients and attendants, that this child practised intentional deception as to her lameness; for example, her companion actually carried her from the side room to the closets at the end of the ward, but afterwards when her bed was removed to the general ward, the child got out of bed, and after standing on her feet, got down on her knees and went on her knees to the closet and back, but had always to be lifted into bed. Owing, no doubt, to some derangement in the nursing, with a change of attendants, these points were not accurately reported till the child was better. In view of palpable deception, “hysterical” is, perhaps, a wrong word to apply to this case, but probably the deception was not always so complete as is here suggested.

The cough seems to have been of a nervous character, and however originating, perpetuated by a habit beyond the child's control. The circumstances which led the medical attendant at the orphanage to suspect phthisis, and which weighed so strongly in the same direction on her admission to the Infirmary, seem to deserve attention, as somewhat unusual in a child, and the effect of the small blister in breaking the habit of the cough was very remarkable.

---

## General Correspondence.

## GANDER MONTH.

*To the Editor of the Obstetrical Journal.*

SIR,—A few days ago I was looking over a novel of the last century ("The Triumvirate," Dublin, 1765), and I made a note as follows:—"It was now the season which midwives distinguish by the title of *gander month*." In another part of the book I met with the word "*gandering*." These terms are new to me, and I cannot find any explanation of them in the dictionaries at hand. Gander month seems to be the period during which a woman keeps (or used to keep) her room after her confinement, and that the husband was "*gandering*" about, and passing his time as agreeably as he could without the society of his wife. Can you give a more satisfactory explanation?

Believe me, yours, &c.,

AQUILLA SMITH.

121, Baggot Street, Dublin,  
May 7th, 1875.

[Dr. Aquilla Smith is quite right in his surmise as to the meaning of gander month. Wright, in his dictionary of obsolete and provincial English, says it is "the month in which a man's wife is confined. *Gandermooner*, one who acts the gallant at that season. *To go a gandering*, to gallant during the season."

Dr. Sermon, in his "English Midwife" (1671), speaking of women resuming their occupations directly after they are delivered, writes as follows apropos of the subject:—"The women in America are so kind to their husbands, that as soon as they are delivered (because they take some pains to beget them with child), presently rise up, and lay them in their own room, who are attended and looked after like to women in child-bed, and in the same manner visited by all friends. If English women would once become so loving to their husbands, it would certainly prevent them from kissing the handsome nurses, or visiting their neighbours' wives."—  
ED. O. J.]

---

# THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

JUNE, 1875.

---

## LOCHIAL DRAINAGE.

AT the discussions which have taken place lately at the Obstetrical Society of London, those who have hitherto spoken appear not to have sufficiently noticed what the writers of the last century called lochial fever—a form of puerperal fever caused by the absorption of putrid lochia. It was the only kind of child-bed fever with which our medical forefathers were acquainted, and without the microscope, or any of our refined diagnostic methods, they arrived at a rational and effective prophylactic and curative method of treating it. They looked upon the utero-vaginal canal after delivery as a sewer, upon the preservation of which in a wholesome condition the comfort and lives of their patients depended. So convinced were they of the necessity of keeping this human drainage-pipe in a sanitary condition that they insured its evacuation by manually removing from it all solid matters, by flushing it, and by causing the patient to assume such a posture as would produce a natural outfall or gravitatory exit of its fluid contents. Harvey, “who,” as Dr. Barnes rightly said, “if he had not been the greatest of physiologists, would have been perhaps recognised as the foremost obstetrician in the world,” was the first, at least in this country, to describe the causes and treatment of lochial fever. He pointed out that those women were most subject to it who lead a sedentary and lazy life, or were of sickly constitution. He describes the progressive changes in the lochia, and adds that sometimes “the after-purgings are depraved contrary to nature, or do not come away kindly.

For it often befalls women (especially the more tender sort) that the after-purgings being corrupted and grown noisome within, do call in fevers and other grievous symptoms. For the womb being excoriated by the separation of the after-burden (especially if the separation were violent) like a large inward ulcer, is cleansed and mundified by the liberal emanations of the after-purgings." Harvey then gives two cases which illustrate his general remarks, and incontestably prove him to have been possessed of extraordinary obstetrical knowledge and skill :—

## I.

"I have had experience in a woman who lying very sick of a malignant fever, and being very weak, did suffer an abortion, who, after the exclusion of the fetus, which was incorrupt and entire, yet lay exceeding weak with a disorderly pulse and in a cold sweat as if she were dying. I perceived the orifice of her womb was lax, soft, and very open, and her after-purgings were something noisome ; whereupon I suspected that something did lurke in her womb which did putrify, and, putting in my hand, I extracted a false conception as big as a goose-egg, which was made of most thick, nervous, and almost gristly substance, having some perforations in it whereout did issue a viscid putrified matter, and immediately upon this she was discharged of those grievous symptoms, and suddenly after did perfectly recover."

## II.

"A very honourable lady in child-bed falling into a fever, by reason no after-purgings came from her, had her privities swollen and scorching. The orifice of her womb being hard and shut up, I did open it a little way with an iron instrument that so I might immit an injection by a little syringe, whereupon black clotted and noisome blood did issue out even to some certain pounds weight, whereby she received present ease."

One more quotation from another seventeenth century English obstetrician, and our readers will see how early the necessity of efficient lochial drainage was appreciated. Dr. Sermon, in his chapter "What care ought to be had of Women in Child-bed," says :—

"Also let there be laid under her hams a little pillow doubled, that she may be somewhat kept up ; so that her thighs and legs lye not strait, let her neither lye along nor sit just upright, but between both, having her head and body rather raised than laid low, that her natural purgations may



with more ease pass from her." What advances have been made in the treatment of lochial fever since the time of Harvey and Sermon? We have confirmed the practice of the former, and know well that by removing portions of retained placenta, or membranous shreds and clots, or by syringing out the uterus, we can cut short one form of puerperal fever; but the practice of the latter is only partially adopted, notwithstanding its hearty recommendation by White, Kirkland, and others. Keeping the patient on her back constantly during the puerperal period is a most pernicious practice. In this position the utero-vaginal canal forms a curve, the most dependent part of which is at the junction of the vagina with the uterus. Here consequently the lochial fluid collects, decomposes, and infects, and here exist the wounds so frequently inflicted during parturition. We would not advise the plan adopted in Japan, where the women are kept in a squatting position for three weeks after their confinements, but, unless in exceptional cases, the plan of keeping a woman during the puerperal period in a strictly horizontal posture appears to us a hazardous mode of practice. The lochia when this plan is adopted become most offensive, so much so, indeed, that Dr. Tyler Smith advises putting bags of charcoal in the bed, whilst others find it necessary to syringe frequently with antiseptic fluids. At home a poor woman after labour has whilst in bed to perform several little acts connected with her domestic life. In a lying-in hospital she lies still. May not the efficient lochial drainage in the first case be one cause of women recovering so much better at their own homes? Far be it from us to decry or discourage microscopic or chemical investigations, but whilst they are progressing let us not lose sight of the grosser facts. As concerns the welfare of the patient, it matters not whether we call septic lochia "putrid after-purgings" or "pyrogenetic fluid containing bacteria." The fact remains the same. All agree that the poisonous liquid must not be allowed to remain in contact with living tissues, and none can doubt but that its existence may be in a great measure prevented by adopting a system of efficient lochial drainage.

## Notices and Reviews of Books.

---

*Cyclopedia of the Practice of Medicine.* Edited by Dr. H. VON ZIEMSEN, Professor of Clinical Medicine in Munich, Bavaria. ALBERT H. BUCK, M.D., New York, Editor of the English translation. London: Sampson Low, Marston, Low and Searle. 1875.

THIS magnificent work is to be published in fifteen volumes at regular intervals of three months, and will be completed in about three years at one guinea per volume. Of the German edition, seven volumes are already published, and the first two on "Acute Infectious Diseases" are already translated and issued. They form two large handsome books, well printed on excellent paper, and, we are happy to add, efficiently indexed. It is intended that this cyclopedia shall embrace the entire range of special pathology, and form a compendious practical handbook for physicians. It will doubtless, when complete, present to English readers an accurate epitome of German Medicine, and will be exceedingly useful to both students and practitioners as a work of reference and a guide in difficulties. We look forward with great pleasure to the appearance of the tenth volume upon the "Diseases of the Female Sexual Organs." The name of Prof. Schroeder, who has undertaken to write it, is a sufficient guarantee of its being competently performed.

Those who wish to subscribe, see the work, or learn more concerning it, may apply to Dr. J. W. Palmer, 23, Dorset-street, Portman-square.

---

## Abstracts of Societies' Proceedings.

---

### OBSTETRICAL SOCIETY OF LONDON.

*Meeting, May 5th, 1875.*

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., *President,*  
*in the Chair.*

*On the Relation of Puerperal Fever to the Infective Diseases  
and Pyemia.*

*(Continued from p. 123.)*

The PRESIDENT: It will be in the recollection of the Society that the discussion commenced by Mr. Spencer Wells was adjourned at the last meeting, on the motion of Dr. Barnes, seconded by Dr. Squire; and it will, therefore, be continued this evening. Before the discussion commences, I may state that Dr. Matthews Duncan has forwarded a letter of explanation in reference to a letter read at the last meeting. He understood that adverse comments had been made on his published letter, and he is very desirous of making some explanation regarding it, which I am sure will be interesting to the meeting. The letter was as follows:—

*To the Secretary of the Obstetrical Society of London.*

Edinburgh, May 3rd, 1875.

MY DEAR SIR,—As I think my former note to you was too short, I beg now to supplement it to a small extent.

One of the most important points for discussion by the Obstetrical Society at the present juncture is the conduct proper to midwifery practitioners, with a view to avoid disaster to patients from puerperal infection; and I expect much advantage from our great Society taking up this difficult question. At the same time, I feel called upon to deprecate premature decision; for the matter, in both its scientific and its practical aspects, is not ripe for more than intelligent, prudent, and fearless discussion. Observing recent trials of our humble and unlicensed female fellow practitioners, I think there have been precipitancy and rashness, believing, as I do, that Mrs. Dymond and Mrs. Marsden have been severely, and probably also unfairly, dealt with. I judge from the meagre reports regarding the cases of these midwives in the weekly medical journals.

The charge of homicide by infection is, I believe, a new one in the history of the law, and I am further of opinion that, in the present state of science and practice, such a charge cannot be substantiated. I know that Mrs. Marsden is now in prison enduring penalty for homicide by infection, and that this is an example of the charge being

sustained; but, while I grieve for this sufferer, I feel sure that the case would have broken down had it been tried in a proper manner.

The public, and to some extent, also, the professional mind, is not well informed on the subject of puerperal mortality and puerperal infection; and there is much excitement abroad regarding the prevention of disease generally. Under these circumstances, error is easily fallen into, even by authorities, when brought face to face with the terrible facts, probably for the first time.

I shall say nothing about the amount of puerperal mortality, presuming that many of the members of our Society have read some remarks on the subject in my last year's address at the Norwich meeting of the British Medical Association. But I may remind you that such high and justly respected authorities as Miss Nightingale and Dr. Wm. Farr have promulgated far too favourable views as to the smallness of this amount; and that exaggerated notions of the innocuousness of child-bearing are fostered by the careless talk of many experienced medical men, asserting, as they are often heard to do, that they never lose a case. If it be believed, as these would have it, that there are very few deaths, it is easy to understand the panic excited by the actual facts.

On another point there seems to be still greater misunderstanding on the part of the public and of the profession—namely, the precautions taken or considered necessary by the profession to avoid communicating disease to patients. For the prevalence of this error I can cite the evidence of the *Times* newspaper, whose utterly erroneous statement I now give as I find it in a leading article of that great organ of public opinion. "It is the invariable practice of medical men" (says the *Times*), "if they attend a case of this (puerperal) fever, to hand over the whole of their midwifery practice to other persons, for at least two or three months; and it has been shown, by ample experience, that this course is absolutely necessary to preserve the lives of their patients." Anything more inconsistent with truth it would be difficult to find. It is this error, thus widely promulgated, and no doubt entertained, that I wish to point out; but I may be allowed to add that, were this true, I would never have a chance of being in practice. I would be perennially on the shelf; and so would every one who had a large practice, especially if that practice involved hospital duty and private consultations.

The difficulties of determining the proper conduct of obstetricians are not to be solved by officious coroners foolishly sending threatening messages to practitioners, nor by judges giving decisions without due consideration; but by discussion in such a Society as ours, and by matured professional opinion. Were the conduct of the coroner in Mrs. Dymond's case, and of the judge in Mrs. Marsden's, to be held as good precedents, there is not one among us who would be secure for a day against the most terrible charges. No surgeon having a case of pyæmia or of erysipelas could go safely to any other



patient. No physician having a case of scarlatina could go elsewhere. The lying-in woman, no doubt, requires special care ; but such care does not differ in kind from the care of other patients. Homicide by infection may be as easily brought home to the surgeon or physician as to the midwife.

I regard the recent prosecutions of midwives as injudicious ; because I believe it would be difficult to find a physician or surgeon who might not on the same kind of grounds, be cast into prison. The crime—so-called—of Mrs. Marsden is to be found, almost every week lately, confessed by ingenuous practitioners writing in our journals. Moreover, I would have preferred that some *man* should have been first charged with this crime, and not a poor and comparatively defenceless woman ; and, as I have said, it will be easy for the authorities to find a criminal man. Where is the physician or surgeon who has not many many times exposed his patients to some risk of infection with dangerous disease ? Is a physician to be found guilty of homicide who exposes his patients to no greater risk than is that of those dearest to him at his home ?

The medical profession, with one voice, proclaims the necessity of extreme care and prudence on the part of medical men, and the utter abnegation of selfish views in their conduct of practice. But there is danger of the public, and of many professional men, being now led to seek for and expect a degree of safety which can in no ordinary circumstances be reached, and to entertain views as to the conduct of practice which are based on error, and led astray from the line of expediency and prudence.

Absolute safety can in no way be attained. As practice is at present conducted by careful practitioners, I believe it is nearly as safe as it can be. Among the necessary precautions in ordinary circumstances, I do not place giving up practice for a time ; and I may say, for myself, that in nearly thirty years of obstetric experience in private, in hospital, and in consultation, I have not, as a precaution, given up work for a single day. And I venture to say that it would not astonish me were it demonstrated that as much disease and death come from patients to practitioners and nurses, as go from practitioners and nurses, ordinarily and duly careful, to patients.

I must draw to a close without having said nearly all I wish to say, and merely name the following grand precautionary measures for obstetric practitioners :

1. Avoidance of the duties of nurses.
2. Avoidance of using the hands in post-mortem investigation.
3. Antiseptic cleanliness of the hands and of the dress.

One case of infection by a practitioner is as heinous as a series of cases. One homicide is as bad as four. The chapter of accidents may bring a series of cases to one practitioner, who is really careful and innocent : and the series may appear to be the result of infection. I have never had a series of cases, not even two near one another, in my private experience ; and I believe such an occurrence would drive

me away from my practice, whatever my theoretical views might be. Many practitioners have, in such circumstances, persisted in practice, and do so now, just as Mrs. Marsden did, in my opinion, foolishly, though not criminally, persist. If homicide by infection be a good charge of manslaughter, it is as good in a case of one infection as in a case of four, however appalling the latter may be. There are few practitioners in any department of medicine against whom such a charge might not be set up.

Allow me to express my regret at forced absence from your meeting, and believe me, dear sir, yours faithfully,

J. MATTHEWS DUNCAN.

Dr. BARNES.—I shall be quite willing to cede my right of priority to any visitor who may be willing to address us. We, who are in the habit of attending here, have spoken so often that I think we must be almost tired of hearing our own voices; and one great advantage of this particular discussion I take to be, that it brings into our Society the views entertained by persons who do not usually come here, especially of men practising in different departments of medicine. I have been in the habit of saying that in obstetric practice we may see at least the germs or the illustrations of almost every problem in medicine and surgery. We want, nevertheless, to have the advantage of the additional light thrown upon these problems from all sources. Of late years, owing to the enormous growth of the metropolis, and the enormous number of medical men practising in different departments, the tendency to cut up medicine into sections is attended with this disadvantage, that we cannot, except on rare occasions like this, get the benefit of the varied experience of men bearing upon one particular department. We should be glad to have the advantage of hearing those who practise surgery and medicine; at the same time, I think that the problems of pyemia and fever may receive some of the most valuable illustrations in the practice of obstetric medicine. I heard the letter of Dr. Duncan with considerable satisfaction. I think it places the grave and great difficulty which oppresses medical practice before us in a most striking manner. If prosecutions are to go on as they are now doing, I do not know how it will be safe for any one to practise at all. It is becoming a most serious matter. We are governed by the ignorance of the law and the ignorance of the judges. The public are taught improperly. Instruction upon this matter, as Dr. Duncan rightly suggests, ought to come from us; and that is one of the advantages, I hope, of a discussion of this kind. The lawyers and the public must receive, or ought to receive, medical opinions from the medical profession, from experts, those who are skilled and know what they are talking about. It will be dangerous to the public at large, unless this principle be carried out more fully than it is. With regard to the great question of puerperal fever, of course for many years as an old teacher I have had to consider this question; and I have been trying and struggling to

put my ideas into a definite form, capable of being understood and taken up by others. I have felt the difficulty expressed by Dr. Leishman at the last meeting ; still, after debating the subject in my own mind, I have come to something like an outline of definite ideas, which, of course, I am ready to change to-morrow, or even to-night from what I may hear, as I quite admit the propriety of holding the mind perfectly free and untrammelled, ready to receive any kind of opinion, from whatever source it may come, and especially to study facts and the bearing of those facts. The leading idea, which tends to simplify the investigation of the subject, is one which I took some pains to illustrate some years ago in a series of lectures published in the *Lancet*, but never finished ; that is, that we may at any rate divide the cases of puerperal fever into two great classes, and, having got as far as that, we can proceed a little further with the analysis, and perhaps pursue each series of cases ultimately to their true source. I think we may lay down this proposition, that we cannot discard the term puerperal fever. Notwithstanding the respect I have for my friend Mr. Hutchinson, whom I look upon as one of the most enlightened authorities of the day, I cannot see my way to discard the term. The meaning attached to it is simply this—fever in a lying-in woman, the general term expressing a number of perfectly different conditions. Having admitted that it simply means fever in lying-in women, we may proceed to analyse it, and to trace the different varieties. I think we may here divide it in this way. We must all of us see examples of it every day. There is the form of fever in the lying-woman which is the direct result of infection or contagion produced by some zymotic poison, as scarlet fever (perhaps the most common of all), or erysipelas, or measles, or typhoid. All those things we see and know, and we cannot for a moment dispute them. Well, then, we have a large class of cases, perhaps the most numerous, which we may call heterogenetic, the result of something which has arisen outside the patient's body, and been put into her. Then there is another class of cases, which may perhaps admit of more dispute—a class of cases which I should call autogenetic—in which all the conditions of the fever exist or arise in the patient's system, with which infection or contagion from without has nothing to do. That class of cases I am as convinced about as I am about the cases of scarlet fever ; they are manifest to every one. But the objection has been raised, and has occurred to most of us : How is it that lying-in women are especially prone to scarlet fever ? Like other persons, the great majority of them have had scarlet fever at some previous period of their lives, and may be supposed to be more or less protected, and how is it that their protection all of a sudden breaks down under the trial of childbirth ? It is said again, that scarlet fever can give nothing but scarlet fever, just as an acorn can give nothing but an oak. That may be true to a certain extent. I have seen cases in women traced to scarlatinal poison in which the



usual symptoms of scarlet fever were absent ; no particular sore throat, no swelling of the glands, no rash, and yet the cases have gone on to a fatal issue. Then, if we go a little further, we see what was alluded to by Dr. Richardson, who was, I think, the only speaker at the last meeting who hit upon the right key. If we look at what a lying-in woman is, we there see a peculiar constitution, ready to receive poisons, and ready for those poisons to ferment and go on to a disastrous issue, while in another case the poison has no such effect. Then there is a peculiar condition following labour, where the system has been loaded with matter. There are the involution of the uterus, the discharge of superfluous blood, the milk process coming on—a state which is just treading on the verge of fever ; at any moment the slightest excitement, or the slightest noxious matter carried into the blood, is ready to ferment and set up a fever. It does not matter what the poison is. I think that one observer in his investigation showed that even a case of cancer in a ward was the starting point of a series of puerperal cases. It may be said that a cancer-germ will produce nothing but cancer. It will produce fever in a lying-in woman ; so will scarlet fever always manifest itself in the usual way which we see in non-pregnant and puerperal women. This peculiar constitution is one which we may recognise in the range of surgery and medicine as well as in obstetrics, although in obstetrics we get the most striking illustrations of this, as in many other pathological forms. In surgical practice, for example, we may see sometimes that there are persons, adults especially, who cannot be vaccinated with impunity ; we all know cases of that kind ; the blood is in a ferment at once. This simple, mild, laudable poison, which we may call vaccine lymph, will set up a ferment, and the patient may die. Sir Benjamin Brodie used to tell us of a man who was killed by the sting of a bee, so great was the irritation set up by the poison ; but that argued a peculiar state of the system. We know there are persons who cannot scratch themselves without a fester ; all going wrong in a moment. Well, all that condition exists in an exaggerated degree in a lying-in woman, no matter what the poison be, whether you call it scarlet fever, or measles, or anything else. This is more especially observed in cases where the symptoms are manifested earlier than they are in the other class of cases where the poison arises in the patient's own system. The fever breaks out in twenty-four hours, or in two or three days, whereas in the other cases it comes on later. With regard to scarlet fever, it is enough to set up any mischief in a lying-in woman, and produces all the mischiefs of any other form of poison. We all know that scarlet fever poison, whatever pathological change it produces in the woman herself, can propagate scarlet fever. Then I would pass to the autogenetic cases. These cases are as distinct in their origin as many cases of infection. For example, you see a woman in the country away from all sources of infection : a little bit of the placenta is retained, a clot of blood is there, or some change



takes place in the uterus, and there is an element of infection ; it runs along the veins or the lymphatics, is absorbed in the mucous membrane ; then you get the blood tainted, and the slightest matter will set it going ; the whole system is in ferment, just as it was from the poison of scarlet fever, or anything else ; you may call it pyemia or septicemia, the result is about the same. These cases come on a little later than those which have a zymotic origin, and they can often be arrested by washing out the uterus and bringing away any superfluous matter there. Many women will succumb at once, or rapidly, to a single dose, no matter how small it may be ; but others can resist to a certain extent ; their excretory organs may be in good working order, and they may throw off a moderate dose or two moderate doses ; but they cannot go on surviving repeated doses. If you can wash out the uterus, you may prevent the process from going on, and stop the disease. That principle has recently come into vogue again. It was practised and taught with success by Harvey, who, if he had not been the greatest physiologist living, would have been perhaps the foremost obstetrician in the world. This mode of infection is one of very great importance for us to consider. There was a case referred to by Dr. Wilson, a gentleman in the country, who believed that infection was due to the skin. I believe it may be propagated by the breath of a medical attendant or a nurse ; we must all be conscious sometimes of taking in poisons by coming into contact with poisonous patients. I have gone away from a craniotomy case with a foul brain, stinking of it ; my breath smelling for a day or two. So with other diseases. In a case of dysentery I have had shivering, diarrhea, and foul breath from the odour of dysentery stools two or three days after being in contact with a patient of that kind. So with other diseases. A man may walk about charged with infectious disease, and those who are susceptible, with whom he comes into contact, may catch it ; those who are not, may perhaps have a little dose, which they can throw off, the system being in good working order, and there is an end of it ; but if a patient be in a lying-in state, with the blood ready to ferment, such a person would be ready to be attacked. There is the secret of the difference. A medical man, who has seen a case of scarlet fever, comes into contact in the course of the day with twenty or thirty patients, and it is perhaps only the lying-in woman who takes the disease ; not that he is necessarily longer in contact with her, but there is a greater liability on her part. I had a letter this morning from the son of an old member of the Society, a former vice-president, who calls my attention to a work by his late father, Dr. U. West. He gives one example, which is instructive, and I thought you would like to hear it. He had an outbreak of puerperal fever in his practice. He had to deliver a woman, to whom he went straight from a case of erysipelas, and this woman had erysipelas the next day. He went afterwards to another patient, whom he did not examine, and that woman escaped. The next day

he went to another case, and the woman died. That is the history of a great many of the epidemics in the German lying-in hospitals, where they have the faculty, as it seems to me, of manufacturing puerperal fever on a large scale. Those women who are lucky enough (one would consider the most unfortunate) to be confined in the streets on their way to the hospital, escape puerperal fever. It is only those who are examined repeatedly that catch it. That has been observed over and over again. If I might be permitted a moment or two longer, I could give you the history of a series of cases occurring in the practice of one midwife in a short time, while all the surrounding neighbourhood was at the time pretty free. I will now only sum up my conclusions in reference to the questions submitted by Mr. Spencer Wells. "Did you ever see a case of puerperal fever which is not really a case of scarlatina, or rubeola, or erysipelas, or traumatic fever caused by the bruising or tearing of parts?" I do not think there is any fever caused by bruising the parts. If there be a little scratch, no matter how small, and the poison be conveyed in that way, that is a different case. That is the way in which wounds act so badly, no doubt. We are not to suppose that there is a puerperal fever which runs a definite course like scarlet fever. We get a fever, which is fatal, and that is serious enough to be considered as a case of puerperal fever. As to the question, whether there is a real form of puerperal fever which is not scarlatina or any of those conditions specified, I will say that there is, and I have called it excretory puerperal fever. We have albuminuria at the end of pregnancy, and those cases are extremely apt to go into puerperal fever: the blood is peculiarly overcharged, the liver and the kidneys cannot act, excretion is at an end, and then the poison runs in without any further injection of poisonous matter from without. That is pure puerperal fever which the patient herself can generate. Then, how can the spread of the disease be prevented? I can only say, by careful isolation of the patient. The conditions of practice are sometimes incompatible with that. There was a case that I met with Dr. West; he was on the point of going away, and there was no one to take his place. The patient sent for him, and would have him, and she fell a victim. That happens to every one in the country, he cannot get away when he likes. With regard to the question of bacteria, as I know nothing about it, I had better say nothing; but I may reserve my doubt with Dr. Richardson, and wait till the bacteria doctrine is proved by those who understand it. With regard to the value of antiseptics, in order to keep hospitals free, I think that to keep hospitals free from puerperal fever is an extremely difficult matter—more difficult than it is in a surgical hospital to keep it free from pyemia. You cannot keep a series of patients in a hospital isolated, in the proper sense of the word. You have the same nurses attending them. You have a variety of poisons acting upon one or two patients, and the consequences may be radiated to others. There is only one secret for safety, and that is to have

the woman confined at her own home, where she can have her own nurse, who has not been anywhere in the way of infection, and her own medical man, who is free from infection. Then, the chances are, that she will go on favourably and happily. Without that, there is no security. A lying-in hospital is not now, by any means, so serious a matter as it used to be ; still, it is always like sitting on a volcano, which will explode at any moment.

Dr. SQUIRE.—I am sorry that the limitation of time has made the valuable observations of Dr. Barnes a little hurried. I must say that, agreeing with him much, I was a little surprised to hear him support a statement of Dr. Richardson's as to the very febrile condition of lying-in women, an idea which, I thought, was derived from the depths of his inner conscience, and not as the result of bedside experience. In the course of a very large inquiry, I have failed to find that febrile disturbance in the majority of women in well-to-do circumstances. This important subject was ably brought before us for discussion under two divisions. It is to the queries under the first of these divisions that I wish to reply ; and it is to the second question that I intend to confine myself more particularly. I presume that there *is* a fever after childbirth caused by a morbid poison communicable by infection ; and, while admitting a large class of infection associated with puerperal fever, I hope to show that the whole class of acute specific diseases may be set aside. It may be one extreme of error to deny the existence of puerperal fever ; it is certainly the other extreme to call every post-partum illness by that name. Almost any febrile attack in the puerperal state will cause suppression of the lochia, arrest of the lacteal secretion, and other common symptoms ; so that we have to separate many febrile ailments, as well as what may happen from retained placenta or other causes of metritis and phlebitis, from the disease under consideration. Our first duty in meeting with such cases is not to call them by the name of puerperal fever, but to diagnose their nature. Many of them we can soon relieve, and in all be sure of carrying no infection to others ; while no case of puerperal fever is without this risk. Question 2 deals with the relations of puerperal fever : first, to the specific infective diseases ; and second, to the traumatic fevers and septicemia. Clear as was the introductory statement, I noticed that, in repeating the question, Mr. Wells transposed erysipelas from the second category to the first. I do not mean to deny it a place there ; but, for the present purpose, I prefer to keep to the terms of the original proposition. I am then able to answer the second part in the affirmative ; and to give to the first part a denial as precise and distinct as possible. I go beyond this, and say that no form of puerperal fever is to be referred to attacks of the specific infective fevers. The *Transactions* of our Society, abounding in excellent materials for the subject in debate, furnish more commentaries against the view I take than in its support. In the very carefully prepared paper in vol. iii., I find it stated from the chair that "any of



the agents which produce zymotic maladies might cause puerperal fever;" and, again, in vol. x. the President, among remarks of high value, says "it can be caused by the contagion of typhus, measles, and scarlet fever." At the last meeting this opinion seemed general. This view has led some to look upon this disease as merely the puerperal form of scarlet fever, or to consider it typhus. This view I controvert, and assert that not only is puerperal fever not typhus, typhoid, small-pox, measles, diphtheria, nor even scarlatina, but that these diseases are little modified by the puerperal state and retain their distinctive characters, so as to be recognisable; they ought to be diagnosed under this, as under other conditions, and called by their own names, and not by that of puerperal fever. Moreover, though pregnancy and puerperal fever may prejudice the prognosis in such complications, and abortion happen in some of them, yet the puerperal accidents are not always so grave as is supposed. I will first take typhus. Of 1000 typhus patients admitted to the London Fever Hospital, 16 were far advanced in pregnancy; they miscarried, and the majority made good recoveries. This is very different from the results of abortion in typhoid, where, however, the fatal result is owing to the serious importance of this accident to the fever, and not to the development of puerperal mischief. Though it is quite possible for puerperal fever or purulent infection to occur after abortion, even in the earlier months, so we see nothing in typhus to cause puerperal fever. Yet I have a good authority against me. Sir Henry Marsh, in the fourth volume of the *Dublin Hospital Reports*, says: "The true character of epidemic puerperal fever seems to be typhus." This is founded on the experience of Dr. Johnson, Professor of Midwifery to the College of Surgeons of Ireland, who states that the wardmaids of the Dublin Lying-in Hospital caught typhus from patients received there. Typhus was prevalent at Dublin, and this shows how typhus may find its way into a lying-in hospital. Twenty years ago there was typhus in Leeds. A friend of mine attending to midwifery in the Leeds School of Medicine met with a case of typhus at childbirth; the fever was well marked, it commenced before delivery, and the characteristic mulberry rash occurred. It was not communicated to any other of the numerous patients consigned to his care, though some cases of puerperal fever were met with in the practice of other students. It is not contended that small-pox is modified by the puerperal state. Vaccination saves us from meeting with it in the severe form; but I have one instance where it was easily recognised, and did not produce puerperal fever. When I saw the patient she was in severe pain; she had gone her full time; she looked ill, and the temperature was 104 deg.; that being utterly different from anything I had found in an ordinary case of labour, and being what occurs in no other ailment besides, I was led to question what had happened. I found that a young man, sickening with small-pox, had been removed from a house in which she was. The next day, labour



pains came on, and in the night she was delivered. The spots of small-pox came out on the following day. The child was removed, and vaccinated at the same time. The woman made a good recovery without any puerperal symptoms whatever. Now if the poison of small-pox can produce puerperal fever, that was a case in which it ought to have occurred. In measles it is rare to find parturient women unprotected by a previous attack : but when so protected, I have seen a mother nurse her child in measles without the least injury up to the time of delivery. Lately, I saw such an instance where the poison must have been concentrated in the room, for the baby had a rash a fortnight after birth, the mother being quite well. Therefore the statement that measles will convey contagion to parturient women seems doubtful. I will quote one instance from the Sidney Papers, of measles at parturition in a woman. Lady Sidney was sickening for measles when, on the third day, with severe cough and full rash, "she is brought to bed of a goodly fat son;" the child was also full of the measles, mostly in the face, yet it sucked the nurse as well as any child could. Both did well. With respect to scarlet fever, no one has a more wholesome dread of it than I. Every means of isolation should be used to keep it out of families. Once it happened to me to see in the family of a physician who devotes his time to the study of how to prevent disease, scarlet fever in a little girl, whose mother had just been delivered. Not only were there no ill symptoms, but not a trace of puerperal disease ; but the progress was favourable. The mother had never had scarlet fever before, yet it is quite certain the infant took the disease and did well. Dr. Braxton Hicks, in his admirable paper, in vol. xii. of our *Transactions*, gives the most valuable support by his cases, though we differ in the conclusions to be drawn from them. Of his 89 cases, but one is entered as truly puerperal fever ; and of the 39 cases of scarlet fever, there was no difficulty in the diagnosis, 20 of them having well marked rash. The great point demonstrated by these cases is the special liability of puerperal women to scarlet fever ; infection is resisted up to the time of delivery ; then they succumb to three or four doses. Diphtheria has given me much anxiety ; and, to my surprise, soon after delivery, the exudation, instead of extending, had cleared off, and an obstinate ulceration was healed. On turning to our *Transactions* for a case of post-partum diphtheria, I find it headed "Mild Puerperal Fever." When we come to erysipelas, the case is very different. Dr. Rigby noticed that the children born during an epidemic of puerperal fever had erysipelas. I do not mean to say that puerperal fever is erysipelas, but this shows its connexion with that and hospitalism and purulent infection, and probably cancer. I believe we can connect this closely with that class of infectious diseases which the investigations of Billroth and Lister have not only enabled us to understand, but to control. I believe there is danger in those dissecting attending midwifery cases. I have seen a dissector suffer from peritonitis after operating on a bad

part ; and I believe he might have been the means of conveying infection to those whom he attended.

Dr. BRAXTON HICKS.—A remark fell from Dr. Squire with respect to the rash accompanying puerperal cases of scarlatina. I think he will find no large number of such cases. Certainly my observations have gone to show that there is no rash, and that there are none of the prominent symptoms of scarlatina where scarlatina has been unquestionably mixed up with the case.

Dr. J. BRUNTON.—As a general practitioner, I think that this subject of the power of carrying infection from one patient to another is of vast importance to all who are in the habit of attending all sorts of infectious diseases, and at the same time attending midwifery cases. If the poison of contagious diseases be so productive of puerperal fever as it has been pronounced to be by many of the speakers in this assembly, I cannot help saying that, in my own practice, I have certainly failed to find it such. The reason for it is this. I come forward as a practical man. Theory is all very well ; but, when we come into general practice, one must look at what is seen in every day's work. The first midwifery case I ever attended was in the small-pox ward of the Royal Infirmary, Glasgow. The patient was in the full bloom of small-pox, and aborted about the seventh month. She recovered without any puerperal symptom of any kind. Then, as Dr. Leishman has said, the students at the University of Glasgow, during the time they are dissecting, are in the habit of attending midwifery cases. I did the same, and I know some gentlemen in the room who were fellow students of mine, who also did the same, and, during the whole time of our practice, we had no puerperal fever at all. Then I came to practise in London, and I went to see a lying-in woman who had two children lying ill with the scarlet fever ; she made a good recovery, with not the slightest feverish symptom whatever. Then, again, general practitioners are in the habit of making post-mortem examinations. They meet with cases of sudden death, and the coroner calls upon them to perform post-mortem examinations. I have done it over and over again. I have gone on with my obstetrical practice all the time, and I have not had, in the whole course of my practice, one single case of puerperal fever. I saw one case of scarlet fever many years ago, where the patient went through the whole course of the disease, with very putrid discharges, as offensive as could well be, and yet she recovered. The skin peeled off ; she had the usual affection of the kidneys, and so on, and yet did well. My evidence in this discussion is decidedly negative. It appears to me that, if scarlet fever, typhus, measles, and small-pox are to be reckoned such very strong producers of puerperal fever, I ought to have had a great deal of it in my practice ; but that has not been the case. I will read a note that I received the other day from Dr. Caskie. "A few months ago, I was sent for to see Mr. P. I found him covered with the scarlet fever rash, and by his side his wife in bed, three days pre-

viously confined. Her labour had been natural. The husband was taken away, and she made a good recovery. Her own children escaped, but the children of the landlady in the house took the fever, and two of them died." Now, if scarlet fever in that case were so deadly, Mrs. P. ought to have taken it and passed through the usual course, or died. I cannot help referring to the remark made by Dr. Richardson and Dr. Barnes about the peculiar physiological condition of lying-in women. It is said that at that time their blood is in a peculiar condition. Certainly that is the case; but I look upon it from another point of view. While there is an excess of fibrin and a diminution of blood-corpuscles, a diminution of solids, and an excess of albumen, I look upon this as Nature's preparation for the casting-off of the ovum. I look upon the excess of fibrin as Nature's safeguard against hemorrhage. I also look upon the excess of fibrin as one of the means for the easy production of the lacteal secretion to follow. Mention was also made of the feverish condition that is said to follow the immediate birth of the child. Since the last meeting, I have had the opportunity of speaking to Dr. Richardson, and I have made observations on this point, and in only one case have I been able to find any increase of temperature worth speaking of, except in one case which was not attended by myself, and in which the lady desired not to suckle her child. There, while the pulse remained at 72, the temperature was as high as  $104^{\circ}$ ; the breasts were very hard, showing some sort of rapid tissue-change at all events. What appears to me to be the nature of puerperal fever, as far as I can theoretically make out, is this: that it is simply autogenetic; that it begins in the uterus of the female who has very lately had hemorrhage, or is weakly, and there is an imperfect contraction of the uterus; a clot remains, that clot decomposes, and then you have pyemic poison; then I do not deny that the poison coming off the excreta from the woman in this pyemic condition may be infectious to others. I regard such a woman as being in a similar condition to one who has had a thigh amputated. We know that, if pyemia occur after such an operation, the dressers are not allowed to dress the wounds of any other patients. No doubt, as Dr. Barnes has said, during this process of pyemia, the poison may be absorbed and then excreted by the lungs of the individual who is in attendance, and be carried to another patient, and there set up a similar form of disease. While Dr. Barnes was speaking, I came to the conclusion, from what he said, that, if the contagious diseases be so productive of puerperal fever, and if the medical man be a focus of infection by exhaling the poison, the judges at the present day should look upon a man as a criminal if he attend obstetric cases while he is attending a fever case. I must say that my experience is exactly the opposite of that.

Dr. HUNTLEY (Jarrow).—As my experience has been somewhat peculiar, of a nature that does not occur to many medical men (perhaps not a single member here present has had a similar expe-



rience), I may be allowed to give rather freer expression to my opinions than otherwise I might do. I will briefly describe the experience to which I refer. About ten years ago, in December, 1864, I had an outbreak of puerperal fever in my practice. I was then a very young man, and the anguish of mind which it caused me was something which I shall never forget. In the course of two months I attended fourteen cases of fever, five of which died. All the cases that I attended were not affected by the disease: at intervals two or three escaped; however, at the end of two months, I think, two patients died. Then I was obliged to cease practice, having come to the conclusion that the disease was in some measure connected with myself; for, although there were four other practitioners in our district, not one had a case of the kind. I was forced to the conclusion that coincidence could not account for such a continued sequence of cases, and that I was the cause of propagating the disease. But how could I propagate it? That was a matter that exercised my mind for many a day. We had small-pox very prevalent at the time in our midst, but no other infectious disease. I was then attending a case of severe burns with great suppuration, but that I only dressed for a few days, and I think it is very unlikely that the infection could be carried from such a source for two months continuously. If it were so, I think such instances would be very frequent, and I could not but consider it absurd in the extreme to think that for weeks, considering all the precautions which I took, frequent ablution, and change of clothes, I still kept the disease. It seemed to me then, and I have no reason to change my opinion now, that the disease was somehow or other reproduced; and the only source from which I thought it could be reproduced was from some poison generated in mal-assimilation, or in some defect in the secretory or excretory system. Arguing from analogy, I see no reason to doubt that many infectious diseases are propagated in this way, and not from clothing. Suppose a nurse is attending a case of scarlet fever for weeks, and does not have the germs of scarlatinal poison entering her system and being exhaled from it, we cannot suppose that the disease will be communicated. There is no proof at all that the clothes communicate the disease always. Perhaps there are instances in which they do. In the case of washerwomen it has been communicated in that way; but I think it is just as reasonable to suppose that vitalized material will be allied with the poison as unvitalized. But it was not chiefly to relate this experience that I came here to-night. I had an impression, from reading the report of the last meeting, that opinions were expressed which were very far from correct. The opinions to which I refer are those chiefly discussed by the two previous speakers. I think it was Dr. Braxton Hicks who asserted that scarlatina was a very prolific source of puerperal fever. But in tracing infection in such a way, you are very likely to drift into error; for, if you approach the subject with any fixed opinion of your own, the wish is apt to become father



to the thought, and you are brought to a conclusion not in accordance with facts. I have never in my own practice been able to associate scarlatinal poison distinctly with puerperal fever. On the 12th November last, I was attending two severe cases of scarlet fever occurring in children. The mother of these children took scarlet fever, and the symptoms in her case differed not in the least iota from those of the children. I can call one or two other instances to mind where such has been the case. So with small-pox. I have seen two or three cases of small-pox in parturient women, but they have always manifested the characteristic eruption, and there could be no doubt about stamping them as small-pox. In the case of scarlet fever to which I have referred, I may say that the woman had no symptoms of inflammation of the uterus, or anything at all to distinguish it. I think what we should really do is not to argue from a particular case to a general conclusion, but to take general facts and deduce particulars from them. I regret to say that the returns of the Registrar-General are not useful in this respect in assisting us to arrive at a satisfactory basis. The returns of puerperal fever, I think, are annual, and extend to counties. If these returns were given quarterly in particular districts, we should then be able to see whether scarlet fever occurred synchronously with puerperal fever, and then we should have a safe guide.

Dr. BROWN.—About six weeks ago I was engaged in attending a patient in her confinement. At that time a child in the house (her own house), was taken ill with a rather severe attack of scarlet fever. She was under great apprehension at the time of her confinement, expecting daily that she would suffer from scarlet fever. About a fortnight after the rash came out on the child she was confined, and did perfectly well, without a bad symptom. The infant had no sign of scarlet fever. About ten days after that another child in the house had scarlet fever badly. I attended another similar case about two months ago. The patient had never had scarlet fever herself—she was quite positive of it. In another case I attended a woman while her own child was ill with the scarlet fever in the house. In some days she was laid up for her confinement, and she got up without a bad symptom. She had never had scarlet fever. I may mention that I attend on an average three or four midwifery cases a week, and I have never had a case of scarlet fever in which I could say that I had conveyed the disease to the patient.

Dr. SWAYNE (Clifton).—I think the letter read at the beginning of this meeting is calculated to do great good, especially as regards the unfounded statement that appeared in the *Times* that medical men could retire from practice for two or three months. This statement has in some places created a panic that is becoming quite a nuisance. I will give an instance that occurred to myself. About the 16th of March last, I consented reluctantly to see a bad case of puerperal fever. I listened to the solicitations of the husband, but I only saw the patient once. A gentleman whose wife I had engaged to

attend at the beginning of this month happened to hear of it, and wrote to me requesting me to release him from the engagement, which, of course, I did. She is not yet confined. As to the mode in which the infection of puerperal fever is conveyed, I think it is generally conveyed by the person of the accoucheur, more than by the clothes or any other way. Some men are peculiarly unfortunate in this respect. It is often observed that all the puerperal cases in a district are limited to a few practitioners. It is not only that they have the run of them at a particular time, but even after a long interval, it is the same men who get them. Some men, I believe, have the power of absorbing and exhaling these poisons to much greater extent than others; and from remarks I have made in my own experience, I am inclined to think that the poison is much more likely to be given off by the skin than by the breath of the practitioner. I have come to the conclusion that men who have moist perspirable skins, especially moist hands, are much more likely to exhale it than those whose hands are generally dry and cool, especially if they have to make frequent examinations during labour. With regard to the precautions to be taken to prevent the spread of puerperal fever, I cannot think that it is necessary for a medical man to seclude himself for more than a week at the outside from midwifery practice. If he be unfortunate enough to have a bad case, probably by that time the poison will have passed out of the system. He should also take the precaution of not wearing the same clothes. I am not aware of having conveyed anything of a puerperal kind to a patient except one; that was in a case of scarlet fever. As a general rule, I now refuse to go to a case of scarlet fever, unless, of course, it should occur to a lying-in woman. I refuse to see children with scarlet fever, on account of the danger of conveying it. Some years ago I did not do so. I was attending a child with scarlet fever, and I attended a lady at the same time. About a week afterwards the rash came out, with sore throat; she did perfectly well, and did not show any symptoms of puerperal fever. But the worst case of scarlet fever I ever saw was in a lady, who, about a week before her confinement, called at a house where the fever was; the children were ill, and she was afraid to go in, but most imprudently the mother came out directly from the sick room to report how they were going on, and she put her head into the carriage to talk to this lady. In that case the scarlet fever came on *pari passu* with the labour. I attended her about ten o'clock at night. I observed that the face became red, and at the latter part of the labour the speech was rather muffled. The redness of the face did not excite particular attention, as it generally comes on at the second stage of labour. On the next day I found she had been delirious; she was covered with a thick rash, had a sore throat, and died in two days. Immediately after seeing a case of puerperal fever I go home, and before going to bed, take a warm bath, and wash myself with carbolic soap; and, on the next day, I take a Turkish bath, which, I think, is

an excellent way of eliminating the poison and cleansing the skin. I need hardly say that, with every accoucheur, personal cleanliness is of the greatest possible importance. We know that cleanliness is next to godliness; we cannot be too particular about it, especially after seeing cases of this kind; and we should be very careful not to wear the same clothes. With these precautions, in a few days, there will be little or no danger of conveying the poison from one patient to another.

Dr. GRAILY HEWITT.—The subject which is now before the Society is one which has on many occasions interested me very deeply. I have seen a great deal of this disease, in public and in private, and it has been my duty not only to learn what the disease is, but to endeavour to combat it, and to teach others in the best manner I could how to deal with it. I think this discussion is extremely opportune, and that it is likely to lead to very good results. Our thanks are due to the President and to Mr. Spencer Wells, who have endeavoured to bring the matter before this great Society. Now, I will not dwell further on preliminaries, but go at once *in medias res*. The opinion which I have formed with respect to puerperal fever is, that it is essentially a form of blood-poison. Seven or eight years ago I read a paper before this Society, in which I detailed the experience which had occurred to me in the British Lying-in Hospital, with which I had been connected for some time. I brought forward a considerable number of cases, and in giving my conclusion, in reference to the nature of the malady, I expressed myself in these words:—"It is impossible to escape the conclusion that puerperal fever consists in nothing more nor less than an injection into the general circulating fluid of a poisonous material of animal origin—that it is a form of pyemia for the production of which the minutest portion of the morbid agent may prove sufficient." I was very early impressed with a lecture, published by the late lamented Sir James Simpson, of Edinburgh, I think, about 1859, in which he strongly advocated the doctrine that puerperal fever is a form of pyemia; and what I had seen at that time, and what I have seen since, has certainly led me to endorse that view of the matter in the strongest manner possible. I may say that I entirely disbelieve in the existence of a form of fever which is sufficiently definite and precise to receive a distinctive name, in the same sense that we speak of typhus fever, or typhoid fever, or measles, or scarlet fever, or small-pox, each of which has a definite and well-determined course. I can see nothing in puerperal fever which at all resembles this. I think all the clinical evidence producible on the subject is entirely opposed to that view, and therefore I have no hesitation in answering that part of the question put by Mr. Spencer Wells in the way I have done. I think it would be profitable to divide the cases of puerperal fever, or puerperal pyemia, into two classes. There are the cases in which there is very distinct evidence of the introduction into the system from without of a morbid animal poison; there are,



in the second place, cases which do not resemble these, in which the evidence is wanting of the introduction from without of such a morbid poison; and I will endeavour to make my remarks under those two heads. In the first place, in reference to the cases that occur from the introduction of poison from without, after what has been said on this occasion, and on former occasions, there can be no question that it has over and over again happened that the disease called puerperal fever has been produced by inoculation (the word seems to me to be suitable), in those unfortunate cases where the medical man himself conveys the disease, and in those cases where the midwives are equally efficacious in communicating it. It is to that class of cases that I would first call attention. It is my impression from all that I have seen that the manner in which this communication takes place, in a large proportion of cases, is by means of the hand; and I believe that the spaces beneath the nails and under the skin which covers the nails, are exceedingly liable to harbour these animal destructive products. I do not say that they are limited to those parts; the skin of the fingers may also be their habitat. We know what sometimes takes place after a post-mortem examination, when it is exceedingly difficult to get rid of the smell of the corpse from the hands, cleanse them in whatever way you will. I am speaking now more particularly of what happened some years ago, before carbolic acid and other disinfectants were much known or used; it is not so difficult now as it was; at all events, I believe, that the nails and the neighbourhood of the nails are the parts more particularly liable to harbour these infecting materials. It seems to me rather unlikely that the clothes carry infection so readily. Of course, in cases where a person allows the cuffs of his coat to be dipped in putrescent material, and then makes an examination of a patient in childbirth, something may happen after that. On the whole, I think the hand is particularly efficacious in this matter. I share the opinion that some previous speakers have expressed, that very great attention should be bestowed on cleansing the hands, in all cases where they are to be used in midwifery practice. A good deal has been said of late years in reference to the *armamentaria* that medical men should be provided with, and I think an useful addition would be a pot of carbolic ointment, and a nail-brush. In the next place, I would remark that it has seemed to me that any animal poison introduced from without may produce what we term puerperal pyemia, the same as may be produced by a student who is handling surgical wounds in the hospital, and attending midwifery cases. I have seen instances of this in the case of a medical practitioner going from one puerperal case to another. I see no difference in the attack produced in these two different ways; there is nothing whatever different in regard to the clinical features of them. Another mode in which the poison is introduced from without is due to the carelessness in washing the patient, or an improper method of washing. I think water never ought to be used for cleansing the perineum in the



case of a recently delivered woman. I think it is far preferable to use a dry clean rag, or dry cotton-wool. This plan secures greater immunity from the passage of *débris* from without. Then, there is another method by which poison may be introduced from without, that is, in case of laceration of the perineum. I think this mode of introduction is not very uncommon. I recollect being called into consultation to see a lady who was suffering from puerperal pyemia. Very great care had been taken with her to keep her exceedingly quiet, and she had not been allowed to be moved from her position (lying on the back) for some days after labour. The perineum had been slightly torn. Now, in this case, the discharges were putrescent. It seems hardly possible to escape the conclusion that in such cases as this there is a pool of putrescent material in the vagina constantly in contact with the abraded perineal surfaces; is it to be wondered at if some of this is absorbed? I think it is bad practice in all cases to maintain the patient too persistently in this position on the back. I think there is an advantage in turning her from side to side occasionally, in order to allow the escape of putrescent *débris* from the vagina, not that it will produce pyemia in all cases, but if there be a laceration of the perineum it is more likely to do so. So far for the cases which come under the first category. I will now proceed to discuss those which are more difficult to define and particularize—those in which the affection does not apparently depend upon anything introduced from without. This class corresponds to the cases denominated by Dr. Barnes autogenetic. In the first place, I would remark that it is exceedingly easy to produce pyemia in the non-puerperal woman by handling the uterus in a certain way. I have seen acute pyemia produced by the action of a sponge-tent at the os uteri, left in too long, and producing violent inflammatory symptoms, and death in a few days. It is well known that this event may happen. The uterus is so constructed that it is exceedingly easy for pyemia to occur in it, and in its tissues; the walls are very thick, the vessels are very large, and they communicate very freely (I am now speaking of the uterus in the non-puerperal state), and a slight wound may at any moment set up pyemia. I have always been unable to see any precise difference between an attack of pyemia in a puerperal state, and in a non-puerperal state; the symptoms are precisely identical, they only differ in degree. I do not wish to give my remarks the semblance of a lecture, but I thought it might be interesting to bring a specimen representing the healthy uterus immediately after delivery, in order to show the condition of the organ at that moment. I have also here a plate exhibiting the condition of the uterus after delivery. Doubtless, most teachers of midwifery are familiar with it. It will serve as a peg on which to hang the remarks to which I shall call your attention in the next place. In the uterus, after delivery, we have an organ precisely analogous to a sponge. It has large interstices, large spaces freely communicating, and moreover, these spaces open into the

uterus through the vessels which have communicated with the placenta, and these openings are filled up by certain clots. The natural event after labour, of course, is that the uterus becomes contracted. A progressive diminution occurs in its size. This is the safeguard, and this is the important vital phenomenon which belongs to the after period of childbirth. Now it is my belief and conviction, from all I have seen, that if we wish to obtain a clue to the manner in which puerperal pyemia is produced, we must regard this process very attentively, and notice particularly the deviations from the normal physiological process which are liable to occur. It is some years now since my attention was first directed to the fact, but it is a fact which I have been able to confirm by repeated observation since, and which I deem to be of the greatest importance in reference to this matter, that concurrently with the commencement of the attack of puerperal pyemia, the uterus is found to be enlarged, in other words, in a state in which its involution is absolutely retarded. I have never seen a case of puerperal pyemia in which this condition of the uterus was absent, and absent at the very commencement of the malady. It seems to me, if this be a fact, that it is an exceedingly important element in the explanation of cases of puerperal pyemia, of whatever kind they may be. It has seemed to me, in endeavouring to carry out explanations of these cases, that the thing which fails is the contraction of the uterus. That is where the breakdown originally occurs. The woman has a large bleeding, for instance, at the time of childbirth. The want of contraction in the uterus acts in precisely the same way as if all the locks of a house were taken off and ingress allowed to any burglar who wished to enter the premises. This is precisely what happens, in fact, after a labour. The contractile power of the uterus fails to a certain extent. The expulsion of the *débris* ceases, and there occurs a suction-action in the uterus, by which the *débris* is taken up into the circulation. I explain in this way those cases in which these scarlet fever-poisons and other fever-poisons apparently produced disease. They destroy the vitality of the patient to a certain extent; they take away the safeguard; they abolish the contraction of the uterus, and they produce a paralysis of the organ for the time being; and when they have once produced this paralysis, the rest of the explanation is sufficiently simple. The pyemic process immediately passes into the uterine sinuses, and it is a question whether the patient will survive, or whether the injuries in this manner will end in speedy death.

Mr. CALLENDER.—I came here to listen and to learn, but, as you have called upon me, I feel bound to offer some remarks on the subject under consideration; and I do so the more willingly, because I quite agree with what fell from Dr. Barnes with reference to the fact that, although we may be all engaged at various points of what I may call the professional circle, yet we are all attracted to one common centre, and at that centre I read the words, “prevention and cure of disease.” I must be allowed to draw upon my experience

as a surgeon in referring to this matter, and I feel that you will be the more indulgent to me when I recall the fact already referred to by several speakers, that there seemed to be many points of resemblance between affections such as erysipelas and septicemia, which are constantly coming under our notice, and the disease of puerperal fever, which is at present under discussion, and which has been held by many speakers to be, in many instances, due in its origin to septic causes. I think that, amongst others, one speaker said that at least three-fourths of the causes were traceable to such an origin. With regard to one's experience of septicemia, I prefer to limit myself to this term; for if I venture to speak of other affections of which I have heard spoken, and which are closely allied to this disease, such as those in which the veins are implicated, I fear, after the remarks that have been made by Mr. Jonathan Hutchinson, I should be regarded as almost a heretic if I ventured to propagate the views I hold as to the part that the veins have in these affections. I confine myself to septicemic affections; and I say that, so far as I can judge from practical experience, we must consider them with reference to two distinctive sets of signs, those which may be spoken of as speculative, and those which may be regarded as facts. With regard to these speculative matters, I think it is purely a matter of speculation (although there seems to be a common consensus that such is the case) that these affections are due to the influence of some septic matter or poison. Then beyond this, it seems to me quite a matter of speculation as to whence that poison is supposed to come. There are some who hold the idea that the poison is to be found in germs which come from without the patient; there are others who seem to hold the view that the septic poison is greatest in the woman herself, as the result of the production of decomposition; and there are yet others who say that the septic material is due directly to the products of inflammation. All these, I hold, are mere matters of speculation; and what I should like to put before the Society is the question, how far we have a series of facts to bring to bear upon these various speculations which have been offered to us. Speaking entirely from a practical point of view (people must be allowed to speak from their own experience), there are certain points which have forced themselves on my attention—certain negative facts, such as these. I hold that, in the treatment of patients in a hospital ward, the first point we have to attend to is, that there must be no foulness about the wounds; the wounds must be absolutely clean; directly we see anything unpleasant about them, we must take care to remove it. The point is that no foulness must be capable of being recognised; the wounds should be most carefully kept from any contamination from other wounds, both by the nurses and dressers; and in regard to sponges and such like materials, all possibility of contagion from this source should be avoided. This leads me to remark that, in the treatment of wounds, a patient would be tolerant enough of decomposition which may be



set up in a wound on his own body, but will be intolerant of poison conveyed to him or her from any other wound. That is another point of which we must be extremely careful. It is a fact that is constantly forced upon one's notice, the possibility of a wound being contaminated from another wound. Then the patients should as far as possible be isolated. It is easy enough to do this in a surgical ward, where a case in which there is an open wound may be surrounded by cases not suffering from wounds, so that they can be, for all practical purposes, isolated. Then no discharge should be allowed to accumulate in a wound, not merely the discharge resulting as a consequence of the ordinary inflammatory process, but the discharge thrown out of the wound during the first few hours after it has been inflicted. I know nothing more irritating or acrid, than the fluid in a wound immediately after an operation, if it be allowed to accumulate. Then, again (and I think this is another point of great negative moment), there should be no movement of the part; the wound should be kept absolutely at rest, allowed to heal from the beginning, and not permitted, by jars and jolts to reopen, so as to create new wounds, through the cracks and fissures of which putrid or other foreign matters can enter. It is by such means as these that I have learned how to banish, for all practical purposes, such affections as those commonly called pyemic (though I prefer the term septicemic), from surgical wards, except now and then when we make errors, and trouble arises in consequence. No such thing as pyemia exists in the hospital wards under my immediate care. By the experience we get from these facts, we have some light thrown upon the worst of the suspicions that I spoke of—namely, the suspicion that these cases of septicemia are in some way connected with the presence of a poison. I think, also, we have some light thrown upon what is patent to us all—that which has been ably spoken of this evening by Dr. Graily Hewitt, the sort of common sense rule that should guide us with regard to the treatment of women in a puerperal state, such as the maintenance of absolute cleanliness and absolute quietness. In addition to this, we do not, from these facts, get much light thrown upon what I further spoke of as a matter of suspicion regarding the cause of the disease. I do not at all see my way clear as to the exact source of the poison; it may be in the air; it may be brought to the patient through her attendant; it may possibly be in the festering of a wound, or be present in the direct products of inflammation, but I do not see my way to the absolute cause. All one can see from the collection of facts surrounding one's cases, extending over a considerable time, is that septicemia is a disease which, in surgical practice, may be absolutely prevented. Here I should like to answer one of Mr. Spencer Wells's questions, whether the disease can possibly be produced. I entertain myself no doubt that pyemia and septicemia can be produced. If, after I had performed an operation upon a patient in otherwise good health, I left that patient unprotected by all the



many means that one is bound to adopt to insure, as a matter of certainty, that a patient shall be conducted towards a successful termination, it would be a mere matter of chance whether that patient did or did not suffer from pyemia. I say a matter of chance, because people differ very much among themselves : a patient may be strong and robust, and resist any poison brought into contact with the system, or the patient may be feeble, and readily succumb to the slightest dose of poison. With reference to another question, I would say, before I sit down, that I hold that antiseptics are of very great use to surgeons. I think they are of the very greatest possible use in surgical practice ; and I take this opportunity, as it is a public one, of saying that, although I may have seemed to differ very much from Mr. Lister with regard to carrying out his principles of antiseptic treatment, I am entirely at one with him as to that matter, and I fully acknowledge the great benefit he has conferred upon us by his advocacy of the antiseptic treatment. All I wish to show is that the treatment should be carried out on a more simple plan without that elaborate attention to detail which Mr. Lister has thought necessary.

The discussion was again adjourned.

---

## OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, March 10th, 1875.*

DR. MATTHEWS DUNCAN, *President, in the Chair.*

Professor SIMPSON exhibited a fibrous polypus, which he had removed from the cavity of the uterus by torsion. It had been expelled into the cavity of the cervix uteri, but not entirely, and a constriction indicated where the internal os had grasped it after the expulsion into the cervical cavity. As the os externum was very narrow, he (Dr. Simpson) had first divided the cervix at both sides with a pair of strong scissors, and then seized the exposed polypus with a pair of abortion forceps. The narrow pedicle which grew from the back wall of the uterus was easily twisted through, and the patient made a good recovery.

Dr. JAMES YOUNG showed the placenta of a dead-born fetus. He explained that it exhibited appearances of thrombosis in various cotyledons.

Dr. KEILLER showed a cast of a fetus, with an interesting and rare malformation of one of the inferior extremities. The right leg, a short way above the ankle-joint, was the seat of a supposed intra-uterine compound fracture, which had healed and united, with some antero-posterior displacement in utero. There was talipes valgus of the left foot. The fetus was otherwise healthy.

Dr. MACDONALD had seen a case some years ago where both arms and the left leg were amputated spontaneously in utero.

Dr. CUTHBERT had seen a fetus at about the third month, where the process of amputation was going on, the cord being twisted round the limb.

Dr. YOUNG had had a case of intra-uterine fracture of the femur, in which, under appropriate treatment, the fracture united perfectly.

---

### *Enlargement of the Thyroid Body in Pregnancy.*

By LAWSON TAIT, F.R.C.S. Ed. and Eng.

I need not here discuss the many and very various interpretations of the physiological import of the thyroid body, for I have met with none which seems to me to be more than a guess. All that we know of certainty is that it is a structure of much greater importance in the economy of the female than in that of the male, being in this respect somewhat analogous to the mammary gland; for it is always larger, and its diseases are much more frequent, in women than in men. Thus the remarkable vascular hypertrophy which constitutes one of the phenomena of the exophthalmic goitre of Begbie, Graves, Basedow, and Trousseau, is far more common among women than among men; so also are the cystic and fibrous forms of hypertrophy; whilst the simple adenoid increase in size is almost unknown among males. In women there seems to be a recurrent variety of this enlargement which is directly dependent on, or at least coincident with, certain epochs of their genetic functions, especially during pregnancy.

This puerperal enlargement of the thyroid body would further seem to me to be dependent upon some endemic cause; for on no other ground can I explain the various estimates given by authors as to its frequency, and the singular fact that, while resident in Wakefield, I saw numerous cases of it, a selection of which will be afterwards detailed, whilst in Birmingham I have seen only one case out of a very large *clientèle* of hospital patients, and in that case the patient had not resided long in this town.

Natalis Guillot is, as far as I know, the first author who has drawn attention to a specific enlargement of the thyroid during pregnancy. His paper is in the *Archives de Médecine*, tom. xvi., and he describes the post-mortem of two fatal cases in which the goitre was found to be constituted by a hypertrophy of the fibrous and glandular elements. Dr. Tanner narrates a fatal case in his work on "Diseases of Pregnancy," but that seems to have been of the vascular variety, associated with exophthalmos, and therefore not such as I have to allude to. Simpson notes enlargement of the thyroid in pregnancy as an "occasional and rare" occurrence; whilst Holmes Coote, in his paper on "Diseases of the Thyroid,"

in Holmes's "System of Surgery," says that it has been not uncommonly noticed.

In a recent paper entitled "*Etudes sur les Maladies Chroniques d'Origine Puerperale*," by Dr. Auguste Ollivier, in the *Arch. Gen. de Médecine*, January, 1873, this peculiar condition is very carefully described; and as none of that author's conclusions tally very closely with my own, I may here briefly summarize them. He finds that puerperal goitre may assume various distinct forms, of which the first is the *goitre subaigu et passager*. This is developed slowly, and only reaches any very considerable size in exceptional instances. It causes no pain, and generally the patient goes to her full time without experiencing any disturbance of respiration. There is no such pulsation as marks the vascular goitre, or that which accompanies exophthalmos. After delivery, it diminishes gradually in volume, and after a few weeks or months, no trace of it is to be found. It usually appears about the third or fourth month of pregnancy.

In Dr. Ollivier's second variety, the goitre is developed very rapidly, and gives rise to accidents by suffocation. I have not seen a case of this kind associated with pregnancy, and I suspect that in it the enlargement belongs to the vascular elements of the gland; for vascular bronchocele of rapid or even sudden growth, and with or without exophthalmos, is not at all rare, independent of pregnancy. We can readily understand how such an occurrence would be much more serious in a pregnant woman.

In the third variety of puerperal goitre described by Dr. Ollivier, the enlargement proceeds slowly during the course of pregnancy, remains stationary after delivery, or may undergo slight increase in size during each succeeding gestation. Sometimes it succeeds a goitre of rapid growth which has been arrested in its development. In some cases the goitre does not appear, or is only recognised some time after a pregnancy. It progresses slowly and insidiously, and without any appreciable cause. This form is far from rare, and its real origin may not be recognised *faute de prévenir*. Instead, however, of remaining stationary, the chronic puerperal goitre is sometimes seen, at a period more or less remote from the pregnancy during which it appeared, to increase rapidly in size so as even to threaten suffocation. After a time it may also be subject to inflammation and suppuration.

For my own part, I cannot see sufficient distinction between Dr. Ollivier's first and third varieties to enable me to consider them as other than different phases of the same condition.

My first experience of puerperal goitre was in the year 1863, when I was acting as assistant to my much esteemed friend Dr. David M'Farlane of Drymen. On 11th December I was called to see Mrs. M., aged thirty-two, who was in labour of her fifth child. I noticed that she had a large goitre, and, on asking its history, she told me that it appeared in her first pregnancy about the fifth month,

gradually increased in size, and completely disappeared soon after labour. It reappeared during her second pregnancy, and similarly grew and disappeared. So in her third, but it did not disappear entirely after her labour. It grew rapidly soon after she became pregnant for the fourth time, became larger than ever, and diminished only slightly after her labour. She first suspected her fifth pregnancy from the increase of the bronchocele. She had had pretty smart flooding after her fourth labour, and I anticipated that the same complication would occur in the one for which I was attending her, and I was not wrong. Labour was natural and easy, but the uterus was very flaccid, and I had much trouble in keeping it contracted. As it was, she lost an unusual though not an excessive amount of blood. I saw her occasionally for two or three months after her labour, but the goitre did not perceptibly diminish, and she remained somewhat anemic.

This woman was a native of Carluke, and had been married there. She told me that such enlargements were not uncommon in the necks of the women in that neighbourhood, but that she did not remember having seen one in the neck of a man.

All the other cases which I have seen, practically presenting the same features as the above, number about twenty, and all, with one other exception, were natives of and residents in the vale of Calder, of which the centre is Wakefield. They came under my care there as hospital patients, and the majority of them applied for ailments which had either no relation to the goitre, or only an indirect one. Out of some thousands of patients I never saw a man with a goitrous enlargement in that district, nor one in a woman who had not borne children. The town and district are physically well situated, the ground being gently undulating, with a deep subsoil of loose drift lying on very porous sandstone, and the watershed is almost exclusively millstone grit. The drinking water is mostly spring or surface well-water, except in the town, where the supply is taken from the river, filtered, and it is marked by a rather abundant quantity of lime, so that stone is rather a prevalent disease.

The trade is chiefly wool-spinning, and the people are of a healthy Saxon type. I cannot suggest any feature which would explain the endemic occurrence of this peculiar disease. In the majority of the cases there was a marked tendency to uterine hemorrhage, which, when the story of the case could be intelligibly given, was generally found to be of subsequent date to the appearance of the goitre. Those women in whom the goitre was of old standing were always anemic, and an almost uniform large and hard condition of the uterus was found, the lips being thick and the cervix open, and the menstrual periods were too frequent and very profuse. The salts of potash, especially the bromide, had a uniformly good result in arresting the excessive flow, whilst iron always increased, it without benefiting the patients in any way; but no kind of treatment was found of any use in diminishing the size of the goitre.



It would be tedious to narrate the therapeutics to which each case was subjected, and I therefore give the above summary of the results.

I have preserved notes of the following cases :—

CASE I.—S. R., aged forty-three, has had eleven children. When pregnant for the tenth time she noticed a goitre, which appeared during the later months of pregnancy and disappeared rapidly after delivery. It reappeared during her eleventh pregnancy, and diminished very much in size after delivery, but did not altogether disappear. In 1867 she had a miscarriage at the fourth month, by which time the goitre had much increased from the size at which it had rested in the interval between her eleventh and twelfth pregnancies. Nine months after, she again miscarried at the third month, by which time the bronchocele had again increased, and since then it has not (till March, 1868) diminished perceptibly in size. Since the last miscarriage she has had two severe attacks of flooding at the monthly periods. The goitre is of large size and three-lobed, showing distinctly the exaggerated outlines of the anatomical divisions of the gland. The floodings were completely arrested by a liberal administration of bromide of potash.

CASE II.—The woman in the case detailed above brought me her sister, because she also had a goitre. She was thirty-six years old and had had two children. In her the goitre made its appearance for the first time during the later months of her second pregnancy, and was only beginning to disappear, some months after her confinement, when I saw her, in August, 1868. It was stated by both sisters that in this case the goitre had been at least three times larger during the pregnancy than it was when I saw it. She menstruated too often and very profusely.

CASE III.—Mrs. W., aged thirty-six, presented herself at the hospital on account of symptoms of well-marked spanemia, associated with profuse menorrhagia. She had a large three-lobed goitre, of which she gave the history that it appeared first as a small lump when she was about three months gone in her first pregnancy, being then twenty-three years of age. It disappeared after delivery, but reappeared with increasing size in each succeeding pregnancy. It never wholly disappeared after the first confinement, but after every other it diminished considerably in size. When I first saw her the neck measured eighteen inches and a half in greatest circumference, and there were occasional attacks of dyspnea, though this latter condition may have arisen from the spanemia. She had seven living children, with one miscarriage between the sixth and seventh, and three miscarriages since the seventh child. She stated that at each of the four miscarriages, and at the sixth and seventh confinements, she had lost a great quantity of blood, and that this had not been the

case in the confinements previous to the sixth. I attended her for the last miscarriage, and the hemorrhage then was quite alarming. She had been made aware of the last three conceptions before the completion of the month by the rapid increase of the goitre, and this matter of detail she gave me without any leading question on my part, and as a point on which she was positive. The increase on each occasion was so great as to make an uncomfortable difference to her breathing. The menorrhagia was completely subdued by full doses (thirty grains) of the bromide of potash, though no difference was noticeable on the tumour. There was no appearance or history of exophthalmos or vascular disturbance.

In August, 1868, I made an experimental trial of the subcutaneous injection over the goitre of ergotin in doses of one-third of a grain, but the constitutional disturbance, indicated chiefly by fainting and sickness, was so great after each injection that it was only repeated thrice. It made no difference to the goitre.

CASE IV.—H. M., aged thirty-seven, had had three children, and in each pregnancy a goitre had appeared at about the fourth month, and in each successive pregnancy it was larger than it had been during the preceding one. It always had rapidly disappeared after confinement. During the interval between the second and third pregnancies, and ever since the last, she has had profuse menstruation, till her condition at the time of my first seeing her was one of serious spanemia. Her last pregnancy had occurred three years previously, and the goitre which remained from it was well marked. She was ordered twenty-grain doses of the chlorate of potash, to be taken with every meal, and this had the effect in two months of completely subduing the excessive menstruation, and in her opinion, though not in my own, of reducing the size of the goitre.

CASE V.—Mrs. G., aged thirty-eight, seen first on 5th March, 1869, with a large solid goitre. She had noticed it first when about five months pregnant of her first child, she being then twenty years of age. It increased in size until the child was born, and then rapidly disappeared. It reappeared during the second pregnancy at about the same period as in the first, but it grew to a much larger size. It diminished very much after the second confinement, but did not disappear entirely. She has since had eleven children, and the commencement of increase in size of the goitre has always occurred at about the same time in each pregnancy; and at every increase it has become larger than it was on the occasion previous. She gives no history of post-partum hemorrhage or of menorrhagia. She had no very marked anemia, though she looked worn down and prematurely old, and she applied for treatment of an ailment which had no relation to the goitre.

CASE VI.—S. J., aged sixty, had her first baby at twenty-two, and

she remembers noticing the growth of a lump in her neck during the last two or three months of her pregnancy. It disappeared soon after her confinement. At twenty-five she had her second child, and the lump reappeared about the same time, or perhaps rather earlier in the pregnancy, but it did not entirely disappear after her confinement. She has had thirteen subsequent pregnancies, and in every one the goitre began to increase in size about the fifth month, and diminished somewhat after delivery. She suffered for many years from profuse menstruation, dating, she thinks, from the birth of her seventh child, and she had the change of life at fifty-two. She was extremely anemic, and was only a short time under notice.

CASE VII.—Betsy L., aged thirty-three, was married at sixteen, and had her first baby when she was nineteen. She distinctly remembers that during the sixth month of that pregnancy a lump began to grow on the front of her neck, and that it went on increasing in size until her confinement, after which it disappeared entirely in a few days. It reappeared at the same time in her second pregnancy, and in four succeeding pregnancies. It was bigger each time, but always completely disappeared soon after confinement. On her seventh pregnancy it grew to a very much larger size than on any previous occasion. This pregnancy proved to be twins, confinement having taken place eight months previous to my having first seen her. The goitre had not wholly disappeared, but was large enough to attract my attention and lead me to inquire into its history. At all her confinements, she had had serious hemorrhage; and ever since her first confinement she had menstruated every three weeks, save during gestation and lactation, the period always lasting eight or nine days, and the loss being very profuse. She was a stout handsome brunette; and though there was no reason to doubt her story, her appearance certainly did not support it, for she was not in the least spanemic-looking.

CASE VIII.—Emily S., aged thirty-three, has been married fourteen years, and has had six pregnancies. In the course of every one, the thyroid has enlarged during the last three or four months, and has always nearly disappeared after confinement. When I first saw her, 25th March, 1870, she had a well-marked goitre, but she stated that it was only about half the size it had been during the ultimate month of her last pregnancy, December, 1869. She was very anemic, and suffered very much from dyspeptic symptoms.

CASE IX.—Emma D., aged thirty-eight, has been married twenty years; has been confined eight times at the full period, and has had eight miscarriages. When I saw her in October, 1869, she had a well-marked goitre, which she stated had been in existence since puberty, though it had been of small size previous to her first pregnancy, during which it increased so rapidly in size that it interfered

seriously with her breathing. After her confinement it rapidly resumed its original size. Both before and after marriage her menstrual periods were always profuse and prolonged. She also always lost an excessive amount of blood at her confinements. She stated that she thought the goitre had increased in size with each pregnancy, but on this point she was not very clear. She had no vascular or nervous symptoms.

CASE X.—Eliza S., aged forty-three, was married when twenty, and had her first child within the year. A goitrous enlargement appeared during her fifth pregnancy, between the fifth and sixth months. She had six subsequent pregnancies; and though the goitre entirely disappeared after the fifth and sixth confinements, it has increased in size with all those subsequent, and in January, 1870, it was very large and hard, but gave her very little inconvenience. Since the existence of the goitre she has had serious floodings at her labours, and her menstruation has been very profuse.

CASE XI.—Harriet H., aged forty-four, was married at twenty-four, having had a child two years previous. Her third pregnancy occurred when she was twenty-eight, and an enlargement in the neck was noticed about the fifth month. It disappeared after her confinement, but grew again in her fourth pregnancy, but did not again disappear. It increased in size with every succeeding pregnancy, and she was under the impression that its growth was specially influenced by a copious bleeding, to which she was subjected with a view of diminishing it. She always has had flooding at her confinements, and has always menstruated profusely. I only saw this patient once, but her goitre was the largest I have ever seen, the greatest measurement round the neck being twenty-four inches.

CASE XII.—Mary Ann H., aged thirty-eight, an out-patient at the Birmingham Hospital for Women, had no indications of goitrous enlargement before marriage. It appeared during the first pregnancy, and did not altogether disappear after the confinement. It has gradually increased in size with each succeeding pregnancy, diminishing somewhat after each delivery. She is very anemic, and has suffered for many years from profuse menstruation, but does not know whether this was or was not antecedent to the appearance of the goitre. She was born in Worcester, and has lived in Hereford, Gloucester, Ludlow, and lately in Birmingham.

In many features these cases are perfectly uniform, whilst in others they present some striking peculiarities, of which a little may be said. Thus in every case but one (IX.) the goitre appeared first during a pregnancy, and even in the exception it is probable that the uniformity is not broken, for in that case only was the first growth of



the goitre very acute. Probably the tendency to this peculiar condition existed before it was so excited by pregnancy. In only one case (V.) the tendency to hemorrhage was not present; and of those in which the history was given decidedly, the majority state that the appearance of the goitre preceded the metrorrhagia. There seems also a marked tendency to miscarriage.

I have not been fortunate enough to have a post-mortem examination of the thyroid of a case of which I have known the history; but in 1869 I made a medico-legal examination in Wakefield of the body of a woman who was six months pregnant, who had had several children, and who had a well marked goitre. I could, however, obtain no history of the goitre, but I have little doubt it belongs to the variety met with in the cases I have detailed; and in the absence of more precise evidence of the structure of these goitres, I may describe this one.

On making the usual incision and reflecting the integuments off the gland, all its parts were seen to be uniformly enlarged—that is, both lobes and the isthmus—and the lobes wrapped round the posterior borders of the trachea. It was increased, roughly, to five times its normal size, and the right lobe was the larger of the two. On section, the structure was everywhere normal, no cysts, sinuses, or dilated vessels being seen anywhere. Several small pieces were removed for microscopic examination. They were placed in a bottle containing equal parts of a two per cent. solution of chromic acid and methylated spirit. The bottle was labelled, but unfortunately misplaced, so that it was not till November, 1874, that an examination of the fragments was made. By that time they were very hard, and cut easily, but did not seem in the least brittle or altered in structure.

A section stained with carmine, and examined by a low power, showed that the fibrous elements generally were exaggerated, and at one spot, in the neighbourhood of a vessel, this was so marked as to be visible to the naked eye. As this vessel was the only one in the section, it was impossible to determine whether it was an artery or a vein, but its appearance was more that of the latter. The fibrous bands running between the alveoli seemed to enter into the coat of the vessel so intimately, that I was at first led to suspect that I had to deal with bundles of unstripped muscular fibre, and the regularly fasciculated appearance presented here and there by the bands favoured this suspicion. No examination by reagents, however, displayed the rod-shaped nuclei which characterize unstripped muscular fibre.\*

In this same section were seen a number of alveoli, of irregular outline and distribution, which were filled with fat and epithelial débris. These were evidently old follicles, the contents of which had

---

\* Dr. Matthews Duncan kindly examined a section, and substantiated this observation.

undergone fatty degeneration, and were being removed ; and I think that the fibrous hypertrophy in the neighbourhood of the vessel must be due only to the coalescence of the fibrous bands consequent on the removal of the follicles.

In another section, stained with hematoxylin, and examined with a higher power, a different and indeed quite opposite state of matters was displayed. Three follicles in the field are shown as having attained what is probably their mature size, and in one there were three brightly refracting points of a yellowish colour, which showed that the process of fatty decay had begun, but still the epithelial elements were perfect. The majority of the follicles were, however, of small size, and the largeness of the cells with which they were stuffed showed that they were just starting into life. A few follicles were occupied by light-brown colloid matter, which, from the coincident absence of the epithelial lining, seemed to indicate an arrest of development of the follicle.

This case seems, therefore, at least to illustrate the follicular hypertrophy of the thyroid, described by Rokitansky, an increase of size due merely to a multiplication of its normal elements—a true hypertrophy, due probably to an increase of the amount of work the gland has to do. To my view, it also explains the process of the periodic enlargement of the thyroid, which I have described in the cases detailed above.

Let us suppose that, during an early pregnancy, the thyroid increases in size by a rapid and considerable increase in number of its follicles, due to some physiological necessity, the nature of which we do not know, but which is in existence only during pregnancy. With the growth of the follicles the inter-alveolar fibrous tissue must also increase ; but when the follicles die and are absorbed, that fibrous increase remains entirely or in great part, its removal being no such easy matter as is the removal of epithelial cells and their products. During the second pregnancy in which the goitre appears, the new follicular growth would seem to have a fresh site, for apparently it is not reproduced in the midst of the fibrous and fatty remains of the old follicles ; and here again it would leave behind it another cirrhotic addition to the permanent bulk of the gland, after the end of the pregnancy and the removal of the temporary increase of the follicles, which I have supposed to exist. This repeated time after time would explain the facts common to all the cases I have met with, that after the goitre has appeared it becomes larger during each successive pregnancy ; and that on each successive diminution after delivery, it is found to have received an accession to its permanent bulk.

As this disease seems to have been, by circumstances, removed from my field of observation, I have failed to bring to bear on it more extensive and painstaking investigation ; and, perhaps, the conclusions I have offered may seem to be based on insufficient experience. But I submit them mostly with hope of drawing the attention of

others more favourably situated with regard to this curious condition, a careful examination of which may yet afford a clue to the physiological significance of the thyroid gland.

Dr. COGHILL said he had a child who was born with a goitre, which he attributed to the circumstance that his wife, during pregnancy, was under malarious influences. It disappeared shortly after birth. This fact, he thought, would support Mr. Tait's views.

Dr. C. BELL was struck by the fact, that in general goitre benefited by the use of iron, but in Mr. Tait's case it was hurtful. It would be interesting to know, if in Derbyshire, where goitre is common, it is most so in pregnant women. He had seen a goitre in a pregnant woman which was quite unaffected by that condition. She was a strong robust-looking woman, accustomed to out-door work. Goitre was occasionally met with in the upper ward of Lanarkshire, but although he had spent much time there, only three other cases had come under his notice. They occurred in the same family, which occupied a low-lying cottage, a good deal shaded by trees. The springs were particularly pure, many of them flowing over limestone.

Dr. MACDONALD had seen cases of goitre in pregnant women, but was not aware of their alleged special increase in size during pregnancy, and had not paid any attention to the point. He would hardly say that the goitre was caused by the pregnancy, but rather that it was the effect of endemic influences, and was exaggerated by the condition of pregnancy. He was astonished at the alarming effects of the one-third grain injection of ergotin mentioned in the paper, as he was in the habit of using ergotin frequently, and had just recently injected first two grains, and again on the following day three grains, in a case of menorrhagia, with nothing but good resulting.

Dr. SIMPSON thought the paper a very valuable one. He had observed minor degrees of the conditions described in pregnancy, but had not met with any case of permanent increase in size resulting from the pregnancy. He thought there was a rare field for studying the question in Derbyshire and Cumberland. He had had a case where the whole mechanism of the head, in its passage through the pelvis, was deranged by the presence of a goitre in the fetus. It had disappeared soon after birth. He knew that some authors had noticed small doses of ergot followed by alarming symptoms.

Dr. FEHLING had had a case under his care of congenital goitre, which did not diminish after birth.

Dr. KEILLER had seen a case in which congenital goitre had interfered with the birth of the child. The mother in this case came from Cumberland, but had no goitre; the goitre in the child had afterwards almost entirely disappeared. The paper of Mr. Tait was unquestionably an interesting one, and showed that its author had looked with a single eye at the character and history of the cases on which he founded his observations. The connexion of the



bronchocele with the condition of pregnancy was doubtless very obvious, as stated by Mr. Tait, but he (Dr. Keiller) could not help thinking, while listening to the illustrations given in the paper, that the goitres and the pregnant condition had been too intimately associated—as if, indeed, the beginning and end of both were interdependent. Dr. Keiller had not enjoyed opportunities of observing the thyroid gland under the stimulation of pregnancy in districts in which goitre was common; but from his (Dr. Keiller's) observation of cases of goitre occurring in pregnancy and otherwise, he could not agree, that by pregnancy alone the thyroid could be so invariably influenced as described by Mr. Tait. In other physiological as well as diseased conditions in the female, apart from pregnancy, and especially in ovarian, uterine, and other cases, Dr. Keiller had observed the sympathetic relations between the thyroid and other glandular structures of the uterus. These relations indeed were known and admitted long ago.

Dr. M. DUNCAN knows goitre to be not rare in young women: he sees them sometimes of considerable size, and frequently curable. In connexion with pregnancy, he has had his attention called by women to the enlargement of the gland, and also in menstruation; and he thought the subject ought to be studied. He had attempted to make out the enlargement of other glands, such as the spleen and liver, during pregnancy, in the living subject, but could not convince himself in this way that any such enlargement existed. He was sure it was the opinion of the Society that they were very much indebted to Mr. Tait for his able paper, and suggested that the Secretary should inform him of this circumstance.

---

*Exhibition of the Work on Inversio Uteri of Crosse by the President.*

Dr. DUNCAN remarked that when recently studying *inversio uteri*, he had been amazed that there was not a copy of Crosse's great work on the subject in Edinburgh. He had written to Crosse's son, the present Dr. Crosse of Norwich, who had sent him his own copy of his father's work, and also five additional volumes, which formed a basis for the continuation of the book. It is a very remarkable work, full of learning and ingenuity, and containing many valuable observations, recorded with distinctness and accuracy. It was greatly to be regretted that Dr. Crosse did not live to complete it.

---

## Obstetric Summary.

*On the Non-Existence of an Essential Puerperal Fever.*

Dr. Siréday (*Ann. de Gyn.* April, 1875) enters a protest against preserving the name of puerperal fever, which serves only to per-



petuate an error, and keep up the confusion and obscurity upon a question that it is of the highest importance to elucidate.

In scarlet fever, typhoid, measles, &c. we have characteristic lesions serving to indicate the class of fever; but not so in puerperal fever, where the lesions discovered after death are numerous, variable, and inconstant, the peritonitis generally met with being almost always complicated by inflammation of the uterus and its appendages. There are two well marked varieties of primary inflammation—phlebitis and angeioleucitis, and it is important to separate these two, for puerperal fever is most frequently nothing but a lymphangitis.

*Rigor* ushers in both affections, but it is slight, and often escapes attention in angeioleucitis; whereas in phlebitis it is never wanting and is extremely violent, often being repeated several times.

In lymphangitis the rigor occurs shortly after labour, whereas in phlebitis it is seldom before the sixteenth day. The *temperature* also presents characteristic differences. In lymphangitis it rises to 104° or 106° F., and remains nearly at the same; whereas in phlebitis there is a progressive augmentation of the temperature until the first rigor appears, when it fluctuates.

*Pain*.—In lymphangitis it is fixed, constant, permanent, and extremely acute. In phlebitis the pain is far from being so constant or manifest, and peritonitis is a less frequent accompaniment. Purulent infection is an habitual consequence of puerperal phlebitis, but is very exceptional in lymphangitis.

*The expression of the physiognomy* varies. In lymphangitis at first the face is red and animated; as the peritonitis becomes confirmed the countenance becomes pale, the eyes become hollow, the nose pinched, but the countenance never presents the earthy aspect or yellowish hue met with in phlebitis.

*The progress of the malady* varies. In angeioleucitis it is very rapid, death occurs eight or ten days after delivery, and in severe cases even in two or three days. Phlebitis runs a longer course, which varies from two to many weeks. The malady commences later, develops slowly, with alternate aggravations and remissions, and when it terminates fatally we observe symptoms of purulent infection.

## Gynecic Summary.

### *Serous Ovarian Cysts.*

M. Panas, in a memoir on this subject, comes to the following conclusions:—

1. Among the reputed ovarian cysts there exists a class of unilocular cysts, containing a special fluid, of which the treatment is as simple as certain in its results.
2. The characters of the cystic fluid are the complete absence of

viscosity, its perfect transparency, its small amount of proteic matter (modified albumen), and its relative richness in alkaline salts (principally chloride of sodium). Slightly or not at all precipitable by heat and nitric acid, the liquid in question is precipitable by alcohol.

3. At present we leave the question of whether the origin of these cysts is really the ovary or the parovarium (corps de Rosenmüller).

4. The treatment of these cysts is even more simple than that of Boinet, who recommended puncture followed by the injection of iodine. Simple puncture by the trocar suffices in every case to obtain by the complete or partial evacuation of the fluid a definite cure.

5. In proceeding thus—not only is there nothing to fear, but we avoid subjecting the patient except to the smallest amount of pain. In a word the treatment of these cysts is as easy as that of simple or spermatic hydrocele in man, which requires almost invariably the employment of caustic or strongly irritant injections.

---

#### *Myo-fibromata of the Uterus.*

Dr. Hildebrandt, in *Gazette Méd. de Paris*, in recording his experience of the subcutaneous injection of ergotine in these cases, states that the conditions most favourable for success are met with in those cases where the tumour is rich in muscular elements, or where its seat is submucous, where the contractility of the uterus is perfectly intact. He adds—It is well to add a few drops of glycerine to the solution of ergotine, to prevent the development of inferior organisms.

---

#### *On the Employment of the Actual Cautery in Uterine Disorders.*

A series of articles on this subject have just been brought to an end in the *Echo de la Vienne Médical*, April, 1875, the author thus summing up his conclusions:—

1st. The malady for the treatment of which the actual cautery is best indicated is chronic metritis, in both stages, whether accompanied or not by ulceration.

2nd. In chronic metritis cauterization ought to be deep in the first period, or period of congestion; superficial in the second, or period of anemia.

3rd. In cancer the actual cautery is rarely useful, and, on the contrary, often very dangerous.

4th. Acute inflammation of the uterine parenchyma, and especially peri-uterine inflammation, distinctly contra-indicate the use of the cautery. Pregnancy should make us very cautious as to the employment of the cautery.

5th. The employment of the cautery is almost absolutely harmless, provided only that the requisite precautions are taken, such as have been previously given.

## Pædiatric Summary.

### *Acute Pemphigus of the Newly-born and Early Infancy.*

Dr. Ladislas Faloy (*Thèse de Paris*, 1875) having witnessed an epidemic of infantile pemphigus, concludes that—

1st. Acute pemphigus—neonatorum—and that of early infancy, have the same nosological characters as acute pemphigus of the adult.

2nd. Benign pemphigus in infants is generally apyretic, epidemic, and contagious to the mother: it is not inoculable.

3rd. Acute infantile malignant pemphigus—generalized—is fatal.

4th. Acute pemphigus of infants is not always, as has been affirmed, syphilitic.

5th. The principal cause of pemphigus is excessive heat.

### *Chloroform in the Surgery of Infants.*

Dr. Bergeron, of Paris, in publishing his experience upon this subject, has come to the following principal conclusions:—

1st. Chloroform in the infant is endowed with an almost absolute harmlessness.

2nd. The child not having come to the age of reason, nor feeling any moral emotion, suffers from no apprehension of the dangers to which it may be exposed, nor experiences the apnea which produces so much terror, and is, the author imagines, a most important cause of death supervening suddenly during the administration of chloroform.

3rd. Chloroform may be administered to the infant from the first day of its birth.

## BOOKS, PAMPHLETS, AND PAPERS RECEIVED.

“Cyclopædia of the Practice of Medicine.” By H. von Ziemssen. Vols. I. and II.—On Acute Infectious Diseases. London: Sampson Low and Co.

“Ueber die Fissur des Blasenhalases.” Von Prof. Spiegelberg. Breslau.

“Till Retroflexionernas ætiologi och therapi,” af Dr. F. Eklund, Stockholm.

Communications have been received from Dr. Aquilla Smith, Dr. Grosholz, Dr. Edis, Dr. Carter, Dr. Finlayson, Mr. Lowsley, Dr. Jno. Brunton, &c.

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.







THE  
OBSTETRICAL JOURNAL

OF  
GREAT BRITAIN AND IRELAND.

~~~~~  
No. XXVIII.—JULY, 1875.  
~~~~~

Original Communications.

---

A LETTER IN FACSIMILE,  
AND OTHER HITHERTO UNPUBLISHED  
MEMORIALS OF HARVEY.

Collected by J. H. AVELING, M.D.

Physician to the Chelsea Hospital for Women, Honorary Librarian of the  
Obstetrical Society of London, &c.

IT is with the greatest pleasure I now have the opportunity of giving in facsimile a letter by Harvey. Specimens of his writing are very rare, and if the memorandum at the back of Dr. Ward's letter, which has no signature, be excepted, it is the only letter in Harvey's own handwriting of which we have any knowledge. For those who are not accustomed to reading seventeenth century writing, a copy in modern characters is given :—

Right Hon<sup>ble</sup>

it is more then time then to give yo<sup>r</sup> hon<sup>r</sup> thanks for  
you<sup>r</sup> favour & benefites to me, reseyved, when I am to com  
w<sup>th</sup> a new sute. butt it (being, butt the consumatinge &  
pfecting what by you<sup>r</sup> favour I have reseyved already) ;  
giveth me the confidence to write att this time, intreating  
yo<sup>r</sup> Hon<sup>r</sup> to be my mediator to his maiesty that, my patent  
being stopped att my Lo. Tresurers (a bysines I am enformed

No. XXVIII.—VOL. III.

R



yo<sup>r</sup> Hon<sup>r</sup> is already acquainted w<sup>th</sup>) y<sup>t</sup> he would be pleased in his gracious favour vnto me, to lett it be, as already it is for my life, according as the rest of my fellows have, & as by you<sup>r</sup> Hon<sup>rs</sup> extraordinary favours to me you have made me yo<sup>r</sup> obliged servaunte for ye would be pleased to protect me. it is written to me besides that one D<sup>r</sup> Mezler hath gotten to be appoynted to wayte in my place for the houshold (before I went I intreated & appoynted D<sup>r</sup> Chambers & Doc<sup>r</sup> Bethune,\* & one Doct<sup>r</sup> Smith of London, one of them att all occasions to pforme that duty for me; & I acquaynted the officers of the howsehold therw<sup>th</sup>, it is not vsual: fo<sup>r</sup> serieant primrose was away above a yeare (& he is surgeon of the howshowld) & yett none putt in his place to wayte whilst he was in Germany w<sup>th</sup> my lo. Marquess. S<sup>r</sup> Theod. Maiern, in Switzerland in K. James his time, was away very long & none putt in his place. I beseech your honnor y<sup>t</sup> noe preiudise arise thereof to me, according (as I humbly thanke yo<sup>r</sup> Hon.) the K. Maiesty made to me his declaration, noe such thinge should be done, by yo<sup>r</sup> Hon<sup>rs</sup> mediation. the miseryes of the Cuntries we have passed & the hopes of our good success & all such news yo<sup>r</sup> Hon: hath from better hands, I can only complayne, that by the waye we could scarce see a dogg crow kite Raven or any bird, or any thinge to anatomise only sum few miserable poeple the reliques of the war & the plague where famine had made anatomies before I came. it is scarce credible in soe ritch, populous & plentifull cuntries as these weare that soe much misery desolation & poverty & famine should in soe short a time be, as we have seen. I interpret it well, y<sup>t</sup> wilbe a greate motive for all heare to have & pcure an assurance of a setled peace. it is time to leave fighting when, ther is nothing to eate nothing to be kept & gotten & the same partyes robb on the other if they gett but once out of sight

Y<sup>r</sup> Hon :

Humble servant

WILL. HARVEY.

---

\* Drs. James Chambers and David Bethune were physicians in ordinary to the King.

This interesting letter is now preserved at Oxford in the Bodleian Library amongst the Clarendon Papers, No. 2076. It is without date, or name of place at which it was written. The person who calendared the Clarendon Papers has dated it 1645, but this is evidently an error, for Dr. Bethune, when the letter was penned, was certainly alive, whereas in 1645 he had been dead six years.\* From the evidence contained in the letter, and from contemporary history, I feel no hesitation in dating the letter some time during the year 1631. In September, 1629, licence was procured by Secretary Dorchester for James Stuart, Duke of Lenox, to travel for three years, and to take with him Dr. Topham, Dean of Lincoln, John St. Alman, and eight other servants. We also know, from the "Records of Harvey, in Extracts from the Journals of the Royal Hospital of St. Bartholomew," by Sir James Paget, Bart., that on the 21st of January, 1630, "Doctor Harvey phisicion to this hospital declared to this court that he is commanded by the Kings most excellent ma<sup>ty</sup> to attend the illustrious Prince the nowe Duke of Lenox in his travells beyond the seas and therefore desireth that this court would allowe of — Smith† Doctor in Phisick for his deputy." In a letter from Sir Henry Mervyn to Nicholas (Clerk of the Council), the former writes (1630, July 28th) of "having put over my Lord Duke (Lenox) for the coast of France." We therefore gather from this that the party did not start upon their travels until July, 1630. They seem to have remained at towns upon the coast, for on August 2nd Sir Henry Mervyn writes that he is going to attend the Duke of Lenox, and purposes to be in the Downs, &c.; and again, on August 10th, he says he has landed the Duke of Lenox at Dieppe. September 23rd of the same year, Edward Dacres writes to Secretary Dorchester

---

\* Dr. Bethune was one of the Royal physicians, and in attendance upon the Court at Berwick when he died.

"1639, July 10th. Dr. Beten (Bethune) is burried this day, and Dr. Harvey is in his place, and Dr. Johnson has Dr. Harvey's place."

"1639, July 12th. Dr. Bethune died after a week's sickness, and was burried in the church of Berwick on Wednesday last."—*State Papers, Domestic.*

† This is doubtless the same Dr. Smith mentioned in Harvey's letter.



that the Duke of Lenox is now settled in Paris for the winter; and again, on November 22nd, saying that the Duke is willing to stay in Paris, and that "In the spring hee intends the tour de France and in the end of the summer to goe into Italy unlesse the continuance of the warrs or the plague hinder him." Dacres writes again, on April 5th, 1631, that the Duke is still in Paris, but thinks of going out of town for a few days. Nothing is known of their movements after this until August, 1631, when Dacres again writes to Dorchester: "Bloys prov'd a place not longee to bee indur'd by my Lo. because of the plague which grew hot there, as Tours likewise where wee made very little stay, soe that wee came downe to Saumurs there to passe the dog dayes, from whence wee are now parting they being at an end. My Lord hath continually bin in good health and intends now to follow your Lo<sup>ps</sup> directions this winter for Spaine whither we are now bending our course (via Bordeaux) where we shall bee before the later end of September." In February, 1632, Sir Thomas Edmonde, writing to Sir Harry Vane, says the Duke of Lenox has been made a Grand in Spain.\* It was about this date the party probably returned from their travels.

It is impossible to read this sketch of the progress of the Duke of Lenox without seeing how nearly the condition of the countries through which they passed is described by Dacres and Harvey, and difficult not to come to the conclusion that they were fellow-travellers.

The Dr. Mezler mentioned in the letter was probably intended for Dr. Adam Moesler, who became an extra licentiate of the College of Physicians of London in 1627.† It is satisfactory to be able to state that the news of his appointment was false, and that Harvey's fears were unfounded, for among the State Papers of 1633 may be found the petition of "Dr. Harvey, one of His Majesty's physicians in ordinary, and for the King's Household."

---

\* These facts are obtained from the State Papers—Domestic Series.

† Dr. Munk's "Roll of the College of Physicians."

The name of the person to whom Harvey wrote his letter is uncertain. We may, however, with probable certainty, surmise the recipient of it to have been the Viscount Dorchester, at that time Principal Secretary to Charles I.

(*To be continued.*)

---

## SOME CASES OF PUERPERAL ILLNESS IN PRIVATE PRACTICE.

By CHARLES J. CULLINGWORTH,  
Surgeon to St. Mary's Hospital, Manchester.

"It is much to be regretted that observers do not record more facts, and fewer opinions."—*Read, on Placenta Prævia*, 1861.

CASES such as I am about to relate frequently occasion much anxiety and solicitude to the medical attendant. I need not stay to enter into any enumeration of the reasons why this is so; they are well known to all. Any surgeon who has been brought face to face with serious puerperal illness in his own practice, and more particularly who has had to deal with a case that, from some special circumstance (such, for instance, as personal relationship), has been of altogether exceptional interest to him, will probably have ransacked the pages of medical literature for such help and information as fully detailed reports of similar cases alone can give, and he will probably have returned from the search disappointed at finding the great majority reported far too briefly to be of much practical use to him.

In the following notes I have been at the greatest pains to record faithfully the points which seemed most urgent, or which most impressed me, and although several of the cases extended over a period of many weeks, every observation, even of pulse and temperature, was, with the single exception referred to in a footnote, made by myself personally, and noted down in writing at the bedside. It is only in private practice that such observations are possible; puerperal illness, as seen in hospital practice, either takes the form of a malignant epidemic or of chronic disease following an acute attack. In the former case, the type of disease

is very different from that which is here described, and in the latter case the history of the earlier stages of the illness is necessarily imperfect. I think that the great scarcity in English medical literature of similarly minute records may be accounted for partly by these considerations, and partly by the fact that those private practitioners who are most likely to meet with these cases, are generally much too busily occupied to enter upon such detailed observations.

The first case appeared in outline in the twelfth volume of the "Transactions of the Obstetrical Society of London," along with a chart showing the range of pulse and temperature during the first three weeks; with this single exception, the cases are narrated here for the first time.

*CASE I.—Natural Labour.—Rigors on Evening of Second Day, &c.—Rise of Temperature.—Pelvic Inflammation; Peritonitis, Cellulitis ending in Suppuration.—Nine Weeks' Illness.—Bursting of Abscess into the Bowel.—Recovery.*

Mrs. C., aged twenty-four, the wife of a journeyman printer, was confined of her first baby at 5.20 A.M. on the 4th January, 1870. The labour was easy and natural, and all went on well until the evening of the 5th, when the patient had a rigor. This was succeeded by others during the night and following day.

*January 7th.*—Headache; hot skin; pulse 100. Complaints of feeling "deadly faint," and of pain across the bottom of her back and down the thighs. No cough or rapidity of breathing. No swelling, pain, or tenderness in any part of the abdomen. Milk secreted normally, and lochia natural.

*8th.*—10.0 A.M. Temp.  $101^{\circ}2$ ; pulse, 109; some fulness and tenderness in left iliac region; still complaining of pain in back and thighs. Ordered  $\frac{1}{2}$  ounce of brandy in milk every three hours, and a mixture containing morphia. 6.0 P.M. Countenance pale; temp.  $104^{\circ}7$ ; pulse 128; increased fulness and tenderness in left iliac region.

*9th.*—9.0 A.M. Temp.  $103^{\circ}2$ ; pulse 118, small and weak. Has had severe pain in the abdomen during the

night, lasting for two hours. Free from pain at present. 12.30 P.M. Temp.  $102^{\circ}8$ ; inclined to sleep; milk scanty. 6.0 P.M. Temp.  $104^{\circ}2$ ; pulse 116; no pain. Ordered vaginal washings with tepid water containing Condyl's fluid, and a turpentine mixture (15 drops every 4 hours). 9.30 P.M. Temp.  $103^{\circ}8$ ; subcutaneous injection of morphia (gr.  $\frac{1}{4}$ ), to be repeated night and morning.

10th.—9.30 A.M. Temp.  $101^{\circ}8$ ; pulse 102. Great distress from blistered surface of abdomen, to which the nurse has applied, at the patient's request, a succession of turpentine stupes. 1.0 P.M. Temp.  $102^{\circ}4$ . 6.0 P.M. Temp.  $104^{\circ}$ ; flushed and perspiring profusely. 9.30 P.M. Bowels acting. Temperature not taken.

11th.—10.0 A.M. Looks better and more cheerful. Temp.  $101^{\circ}2$ ; pulse 100; no pain. 6.0 P.M. Blistered surface, suppurating and surrounded by inflamed skin. Temp.  $102^{\circ}0$ . 9.30 P.M. Temp.  $102^{\circ}2$ .

12th.—10.0 A.M. Temp.  $102^{\circ}6$ ; pulse 100. The distress owing to the condition of the skin continues. Complains of pain in the left iliac region and in the vagina, and constant tenesmus. Tongue covered with yellowish fur. Ordered a poultice to the blistered surface, and a morphia suppository (gr.  $\frac{1}{2}$ ). 6.0 P.M. Temp.  $105^{\circ}0$ ; pulse 116. Drowsy, sinks into the bed, tenesmus relieved, great thirst, complains of severe and continuous pain in left iliac region above and to left of the spine of the pubes, where hardness and tenderness are distinctly marked. 9.30 P.M. Temp.  $104^{\circ}3$ . Zinc ointment to the blistered surface, to be again covered with poultices. Subcutaneous injection of  $\frac{1}{8}$  gr. of morphia, to be repeated every night.

13th.—10.0 A.M. Temp.  $99^{\circ}8$ ; pulse 88. Had a good night; washed her own hands and face this morning for the first time. No pain. 6.30 P.M. Temp.  $101^{\circ}2$ ; pulse 94. Slept most of the day; tongue cleaning. Ordered a simple enema. 9.30 P.M. Temp.  $101^{\circ}6$ .

14th.—10.0 A.M. Temp.  $100^{\circ}2$ ; pulse 90. Disturbed night through the baby's restlessness; feels weak. Ordered a fluid diet without stimulants. 6.30 P.M. Temp.  $101^{\circ}7$ ; pulse 95. Profuse perspiration; tongue cleaning; has a full



supply of milk. Urine sp. gr. 1020, very slightly albuminous. 9.30 P.M. Temp. 102°6. There has been slight uterine hemorrhage. Urine slightly albuminous; sp. gr. 1020.

15th.—10.0. A.M. Temp. 100°9; pulse 112. A little pain in abdomen occasionally; marked tenderness over uterus and in both iliac regions; blistered surfaces healing; tongue dry and rough. 6.15 P.M. Temp. 101°2; pulse 90. Very prostrate; lochia healthy. 9.30 P.M. Temp. 102°5; pulse 100.

16th.—10.0 A.M. Temp. 99°9; pulse 94. Tongue less dry; profuse perspiration from time to time. Is able to assist herself better than hitherto. 9.30 P.M. Temp. 101°4; pulse 95. Had a quiet day. Catching pain in left iliac region, lower bowel, and vagina, felt on breathing deeply, yawning, or moving.

17th.—10.0 A.M. Awake for some hours, in spite of last night's subcutaneous injection, and perspired most profusely. Temp. 101°6; pulse 104. Feels very prostrate. Tongue clean and moist. Ordered three ounces of sherry daily. 9.30 P.M. Temp. 102°3; pulse 104. Free from pain; bowels relieved by enema. Ordered to discontinue the turpentine mixture and subcutaneous injections, and to take a morphia draught if required.

18th.—10.0 A.M. Could not sleep; took the draught ( $\frac{1}{4}$  gr. morphia) at 2.0 A.M.; then slept by snatches, perspiring most copiously. Temp. 99°9; pulse 98. Looks more cheerful. Ordered a dilute sulphuric acid mixture. 9.30 P.M. Constant tenesmus, without action of the bowels. Tongue clean; appetite good. Temp. 100°6; pulse 83.

19th.—10.0 A.M. Restless during early part of night; so took draught containing succus conii, ʒj, and slept in about half an hour afterwards. Temp. (taken in both *axillæ*) 98°6; pulse 92. Tenesmus continues; perspires less abundantly. 9.30 P.M. Bowels relieved by enema. Complaints of great pain above left groin, striking directly backwards. Temp. 101°; pulse 86.

20th.—10.0 A.M. Restless early in the night, then slept well without draught. Temp. 98°9; pulse 82. No per-

spiration. 9.30 P.M. Bowels have acted naturally. There is some abdominal distension. Temp.  $101^{\circ}5$ ; pulse 104.

22nd.—9.30 P.M. Temp.  $102^{\circ}6$ ; pulse 106. Very pale, but in good spirits; abdomen distended; desires to be allowed meat.

23rd.—9.0 A.M. Slept well. Temp.  $99^{\circ}7$ ; pulse 90. Chop dinner.

24th.—10.0 A.M. Temp.  $99^{\circ}2$ ; pulse 95. No discomfort after the meat dinner.

26th.—Tenderness and resistance in left iliac region lessened. No dulness. To sit up for half an hour.

27th.—A little pain in left iliac region after sitting up. No increase of fulness or tenderness. Rather more vaginal discharge of blood. Pulse 94.

30th.—Severely purged, with paroxysmal pain.

31st.—10.30 A.M. Temp.  $100^{\circ}$ ; pulse 100. Pain continues, having kept her awake all night. Abdomen universally tender on pressure; breath offensive; tongue covered with brownish-white fur. Ordered to return to milk diet, and to remain in bed. 7.0 P.M. Gripping pain almost ceaseless; very little diarrhea; no tenesmus. Administered  $\frac{2}{3}$  gr. morphia subcutaneously, and ordered spongio-piline to abdomen.

February 1st.—No pain or purging; vomiting last night and again to-day; abdomen tender on pressure. Temp. (at 10.30 A.M.)  $100^{\circ}2$ ; pulse 98.

2nd.—Noon. Tongue dry, and covered with yellowish-white fur; thirsty; no appetite; urine turbid and scanty; bowels unopened. Temp.  $100^{\circ}2$ ; pulse 86.

3rd.—Tongue the same; breath offensive; bowels not relieved since evening of 31st ult. Temp.  $99^{\circ}7$ ; pulse 86.

5th.—Looks better; bowels moved naturally; much pain in left iliac region, with thickening and tenderness. Temp.  $100^{\circ}3$ ; pulse 88.

6th.—1.0 P.M. Condition same. Temp.  $99^{\circ}6$ .

7th.—Complains of unpleasant odour in nostrils, and unpleasant taste in mouth. Temp.  $100^{\circ}4$ .

8th.—Ordered six leeches to left iliac region.

9th.—10.0 A.M. Temp.  $100^{\circ}4$ ; pulse 96. Pain relieved; fulness and tenderness as before.

10th.—Marked tenderness over whole surface of abdomen. Pain of a sharp stabbing character on slightest movement, and on deep inspiration, over abdomen and in the right side of chest. Nothing abnormal on auscultation. Ordered a grain of opium every four hours.

12th.—10.0 A.M. Temp.  $103^{\circ}2$ ; pulse 130. Abdomen more distended, tympanitic, tender on pressure, painful on movement; micturition difficult. Ordered four ounces of brandy daily; turpentine stupe, to be followed by spongipiline poultices.

13th.—10.0 A.M. Temp.  $100^{\circ}$ ; pulse 108. Looks better; abdomen still tender; less pain.

15th.—10.0 A.M. Temp.  $99^{\circ}8$ ; pulse 88.

16th.—Bowels relieved by enema; much pain in rectum and vagina; a little distension, and slight general tenderness.

27th.—10.0 A.M. Temp.  $102^{\circ}4$ ; pulse 120, small and wiry. Tenderness diminished at upper part, still marked below level of umbilicus; countenance very pinched and pale; speaks in a whisper; constant nausea; bowels unopened since 22nd, and then but very slightly after an enema. An ounce of brandy every three hours. 3.0 P.M. Temp.  $102^{\circ}4$ ; pulse 120. Countenance less pinched; after a series of enemata, some small nodules of fecal matter passed. 9.0 P.M. Pain "like knives" in lower part of belly and in rectum. Ordered suppository of morphia (gr.  $\frac{1}{2}$ ).

28th.—10.0 A.M. Good deal of stabbing pain; difficulty in micturition increases, so that she has now to wait a quarter of an hour or more before she is able to pass urine. Temp.  $100^{\circ}8$ ; pulse 110. 8.0 P.M. Temp.  $102^{\circ}4$ .

*March 1st.*—10.0 A.M. Temp.  $100^{\circ}7$ .

2nd.—10.0 A.M. Abdomen very slightly tender, except in the left iliac region, where the tenderness is excessive; there too the distended bowels are more prominent, giving the abdomen an uneven appearance. The intestines can be seen in strong vermicular movement through the closely adapted parietes. Pain inconsiderable. No action of bowels.

3rd.—Last evening suffered intensely from paroxysms of pain in the bowels and constant desire to micturate.

4th.—After emptying the rectum of some scybala of the size of hazel nuts, its cavity was explored. A firm, non-fluctuating mass was felt in front, pressing backwards so as to diminish the calibre of the bowel.

5th & 6th.—No action of bowels ; much suffering from piles, and a sense of pressure upon the rectum and vagina. Omit the opium pills.

7th.—At 2.0 A.M. passed a lump or two of feces, together with a small quantity of thick, ropy pus, without ill odour. A little pus drained from the bowel until, at six o'clock, a few similar masses passed with more pus. On each occasion the bearing-down sensation was agonizing. 10.0 A.M. Tenderness and sense of resistance in left iliac region reduced. Less pain at the anus. 7.0 P.M. Three or four ounces of thick pus have passed during the day ; all sensation of bearing-down has gone. No dulness, tenderness, or feeling of resistance in left iliac region. The appearance of the abdomen is normal ; there is no abdominal pain or dysuria. Temp. 98°6.

8th.—Passed most copious evacuations with more pus. Tongue moist and cleaning.

9th.—Tenesmus ; the alvine evacuations consist of pus, membranous shreds, bits of feces, and streaks of bright blood. 10.30 A.M. Ordered starch and opium enema. 7.0 P.M. No further straining. Temp. 98°5. Ravenously hungry ; has been asking for dry bread. Fluid diet to be continued, with the brandy ( $\frac{1}{2}$  oz. every four hours).

10th.—No action of bowels ; passed a small quantity of pus.

11th.—Ditto.

13th.—No pus has been seen since the 11th ; during the night the bowels acted, for the first time since the 9th, with no trace of matter. Ordered three ounces of wine daily instead of the brandy, and a light meat dinner.

19th.—Temp. 98°8 ; pulse 80. To sit up for an hour.

24th.—Improving daily ; sits up three or four hours to-



gether without inconvenience. All the excretory functions are performed naturally.

The patient got perfectly well, and has since been confined of a second child, without any untoward occurrence.

*Remarks.*—I am not able to throw any light on the origin of this attack. The patient's friends attributed it to her having been left unattended during the night following her confinement, and having repeatedly sat up in bed to rock her child to sleep in her arms.

I may just note in passing the frequency with which puerperal pelvic cellulitis, ending in abscess, occurs in *primiparæ*. Out of 133 cases that I have collected from various sources, 62, or nearly one-half, are reported to have taken place after the first labour.

The situation of the pelvic mischief became apparent on the third day after the first rigor, when an ill-defined fulness and some tenderness were discovered in the left iliac region. As early as the eighth day symptoms of pressure upon the pelvic viscera pointed to the spread of the inflammation to the tissues in front of the rectum. Micturition was not, however, interfered with until considerably later. There was no rigor to indicate the occurrence of suppuration, and I was never able to satisfy myself of the presence of fluctuation.

The speedy relief to all the symptoms when the abscess burst, leads one to suspect that the opening was a larger and more practicable one than is said generally to obtain;\* and this observation is confirmed by the fact that it only continued to discharge itself for the short period of five days.

The treatment consisted in the enjoining of rest, in methodical feeding, in the local application of moist warmth, in the free administration of opiates, and in the occasional resort to small quantities of wine or brandy.

---

\* McClintock, A. H., "Clinical Memoirs on Diseases of Women." Dublin, 1863, p. 15. "I doubt not that in most of the cases where these pelvic abscesses communicate with the intestinal canal, it is by a small [capillary] orifice of this kind. In this way can be explained the tardy recovery supervening upon such a mode of evacuation of the abscess."

CASE II.—*Rupture of Perineum.—Immediate Introduction of Sutures.—Severe Sciatica on Third Day.—Rise of Temperature.—Albuminuria.—Deep Slough over Right Trochanter.—Pelvic Inflammation; Metritis, Peritonitis, Cellulitis.—Four Months' Illness.—Recovery.*

Mrs. D., aged twenty-seven years, of light complexion and cheerful disposition, the wife of a gentleman in a Manchester warehouse, was confined of her first baby on the 26th June, 1873. Rupture of the perineum occurred up to, but not through, the anal sphincter; five interrupted silver-wire sutures were inserted at once, the urine drawn off every eight hours, and a grain of opium administered every six hours.

On the 27th and 28th all went on well. On the 29th I learnt that the patient had been kept awake all night from pain along the course of the right sciatic nerve. I administered a subcutaneous injection of  $\frac{1}{4}$  grain of morphia, which quickly relieved the pain, and an order was given to wash out the vagina with tepid water. On visiting her in the evening, I found her free from pain and very comfortable. Later on in the same evening I was hastily summoned on account of a sudden attack of intense pain along the course of the *left* sciatic nerve. There had been no rigor. The temperature was  $105^{\circ}0$ ; the pulse 120; the tongue was thickly furred, presenting a deep red appearance along the edges and tip, and prominent *papillæ*. Another morphia injection was given.

June 30th.—8.0 A.M. No pain; wound looking irritable, and inclined to slough here and there; patient very tremulous, and betraying an assumed appearance of cheerfulness, humming little tunes, and speaking in an unnaturally jerky and jaunty manner. Temp.  $105^{\circ}1$ . 10.30 A.M. Pain has returned over the *right* sciatic nerve. Administered a subcutaneous injection of morphia and ordered two grains of quinine every four hours. 1.30 P.M. Perspiring profusely; looking calmer and less restless; urine sp. gr. 1025, acid, loaded with lithates, and containing a slight amount of

albumen. Temp.  $102^{\circ}$ ; pulse 104. 5.0 P.M. Temp.  $102^{\circ}4$ ; pulse 105. 7.30 P.M. Skin hot and dry; return of pain behind *right* hip. Temp.  $103^{\circ}5$ ; pulse 115. Subcutaneous injection of morphia (gr.  $\frac{1}{4}$ ). 11.0 P.M. Sleepy; free from pain. Temp.  $102^{\circ}9$ ; pulse 106.

*July 1st.*—8.0 A.M. Temp.  $103^{\circ}6$ ; pulse 116. A little pain felt just below umbilicus on taking a deep inspiration. Ordered five grains of quinine and one grain of opium every four hours. 11.0 A.M. Temp.  $103^{\circ}4$ ; pulse 120. 3.0 P.M. Temp.  $105^{\circ}1$ ; pulse 132; nervous excitement, but freedom from pain. 5.30 P.M. Temp.  $103^{\circ}8$ ; pulse 120. 8.0 P.M. Asleep; perspiring profusely. 11.0 P.M. Temp.  $101^{\circ}6$ ; pulse 107; quite calm; a little hemorrhage from the vagina.

*2nd.*—8.0 A.M. Temp.  $102^{\circ}8$ ; pulse 120; no pain; restless night from the baby having been troublesome. Ordered  $2\frac{1}{2}$  grains of quinine, and one grain of opium every four hours. 5.0 P.M. Temp.  $103^{\circ}8$ ; pulse 124; sleeps a good deal; starts during sleep; no delirium; no edema. Urine sp. gr. 1024, containing a considerable amount of albumen. 11.0 P.M. Temp.  $103^{\circ}4$ ; pulse 120; has now been lying for forty-eight hours on her right side; says she feels no discomfort.

*3rd.*—8.0 A.M. Temp.  $102^{\circ}3$ ; pulse 120; slept well; for the first time passed urine involuntarily during sleep; is perspiring profusely; tongue still deeply furred. Catheterism as usual. 11.0 A.M. Temp.  $102^{\circ}3$ ; pulse 120; all swelling and redness disappeared from perineum; urgently desiring to pass water; is to be allowed to pass it voluntarily. 3.0 P.M. Temp.  $102^{\circ}9$ ; pulse 122. 10.0 P.M. Temp.  $102^{\circ}8$ ; pulse 120.

*4th.*—8.0 A.M. Temp.  $101^{\circ}3$ ; pulse 120; threatened slough over *right* hip; lies quite a dead weight; great tendency to faintness; says she feels more like herself; complexion has assumed a yellowish, livid, mottled aspect; prostration extreme. To discontinue quinine, and administer  $\frac{1}{2}$  ounce of brandy every four hours in water in divided doses. 1.0 P.M. Temp.  $102^{\circ}8$ . 5.0 P.M. Temp.  $103^{\circ}5$ ; pulse 120; complains of commencing soreness over the *left* hip. Ragged

ulcer on each side of tongue. 10.0 P.M. Temp.  $102^{\circ}3$ ; pulse 120; moved on to a water-bed.

5th.—7.30 A.M. Temp.  $102^{\circ}6$ ; pulse 120; stitches removed; wound completely united. To discontinue opium. 1.15 P.M. Temp.  $101^{\circ}3$ ; pulse 120; a little discharge from the bowel of yellowish irritating liquid; no solid feces. 5.0 P.M. Temp.  $101^{\circ}3$ ; moved to the other side of the bed, while a little more water is poured into the bed. Happening to be but lightly covered, she suddenly became cold, livid, and pinched-looking; reaction, however, soon set in. Ordered an ounce of champagne every hour. 10.30 P.M. Temp.  $101^{\circ}5$ ; pulse 116; feels overpowered with heat; pulse thready. Ordered a drachm of potassium chlorate to be taken daily in barley-water.

6th.—9.0 A.M. Temp.  $99^{\circ}3$ ; pulse 116; slept little until four o'clock, when a large solid motion was passed. 1.0 P.M. Temp.  $99^{\circ}5$ ; pulse 110. 5.0 P.M. Temp.  $100^{\circ}7$ . 10.0 P.M. Temp.  $102^{\circ}0$ ; passed more hard, colourless lumps of feces.

7th.—8.0 A.M. Temp.  $99^{\circ}9$ ; pulse 122; passed more scybala. 1.0 P.M. Temp.  $100^{\circ}6$ ; pulse 120. 5.0 P.M. Temp.  $101^{\circ}3$ ; pulse 120. 10.0 P.M. Temp.  $103^{\circ}2$ ; pulse 120; headache; pain in right hip; resp. 36, no cough, no pain in chest; nothing abnormal on percussion and auscultation in front.

8th.—8.0 A.M. Temp.  $99^{\circ}6$ ; pulse 110. A very copious action of bowels after an enema. 1.0 P.M. Temp.  $98^{\circ}9$ . 5.0 P.M. Temp.  $101^{\circ}8$ ; pulse 112; tongue covered with brownish-white fur; mouth a little dry; distinct fulness and tenderness to right of the enlarged uterus. 10.0 P.M. Temp.  $103^{\circ}0$ ; pulse 132.

9th.—8.0 A.M. Temp.  $104^{\circ}0$ ; sleepless night, owing to colicky pains; great tenderness across lower part of abdomen. Ordered a linseed-meal poultice. 1.0 P.M. Temp.  $104^{\circ}4$ ; pulse 140. 5.30 P.M. Temp.  $103^{\circ}7$ ; pulse 130; pain easier, mouth less dry, general aspect more satisfactory. Ordered a mixture containing sulphate of iron. 10.0 P.M. Temp.  $103^{\circ}7$ ; pulse 128.

10th.—8.0 A.M. Temp.  $102^{\circ}6$ ; pulse 128; better night,



less pain, good action of bowels. 5.0 P.M. Temp.  $102^{\circ}7$ ; much less pain. 10.0 P.M. Temp.  $102^{\circ}3$ ; pulse 130.

11th.—8.0 A.M. Temp.  $100^{\circ}3$ ; pulse 120; no abdominal pain except when the bladder is full. 5.0 P.M. Temp.  $101^{\circ}6$ ; pulse 116; a good deal of colicky pain, and the swelling and tenderness are increasing towards the left side. 8.30 P.M. Temp.  $103^{\circ}0$ ; pulse 126; large and abundant sudamina over both back and front of trunk.

12th.—8.0 A.M. Temp.  $100^{\circ}8$ ; pulse 116; bowels relaxed. 3.0 P.M. Temp.  $102^{\circ}7$ . Ordered six leeches over lower abdomen. 9.30 P.M. Temp.  $102^{\circ}5$ ; pulse 120.

13th.—8.30 A.M. Temp.  $101^{\circ}0$ ; pulse 110; tenderness and fulness diminished on the left side; no pain now when bladder is distended. 5.0 P.M. Temp.  $102^{\circ}5$ ; pulse 130; liquid feces frequently pass away from the bowel involuntarily in small quantities. To discontinue champagne, beef-tea, and medicine, and to take brandy and one dose of opium. 10.0 P.M. Temp.  $101^{\circ}9$ ; pulse 118; urine pale, turbid, slightly alkaline, sp. gr. 1012, containing small amount of albumen.

14th.—8.0 A.M. Temp.  $101^{\circ}6$ ; pulse 120. Good night; no involuntary action of bowels since 1.0 P.M. on the 13th. Ordered six leeches to hypogastrium. 5.0 P.M. Temp.  $102^{\circ}8$ ; pulse 124. A little membrane and a colourless, transparent, jelly-like material evacuated from the bowels; considerable amount of griping; tenderness over swelling on right of the uterus diminished. Ordered a dose of opium. 10.0 P.M. Temp.  $102^{\circ}5$ ; pulse 128. Respirations 28; a very scanty solid motion, with a trace of blood, and a little membrane; no more griping.

15th.—8.0 A.M. Temp.  $99^{\circ}4$ ; pulse 110; resp. 22. Looks better; tenderness and swelling limited to the middle of the hypogastric region, from the pubes to within three inches of umbilicus, and measuring three inches in breadth (probably the uterus itself). 5.0 P.M. Temp.  $101^{\circ}$ ; pulse 120. 8.0 P.M. Temp.  $101^{\circ}2$ ; pulse 112.

16th.—8.0 A.M. Temp.  $99^{\circ}9$ ; pulse 112. 5.0 P.M. Temp.  $101^{\circ}3$ ; pulse 120. Propped up by means of a

bed-rest ; passed a solid motion. 10.0 P.M. Temp.  $101^{\circ}7$  ; pulse 112.

17th.—8.0 A.M. Temp.  $100^{\circ}7$  ; pulse 112. 5.0 P.M. Temp.  $101^{\circ}7$  ; pulse 120. 10.0 P.M. Temp.  $101^{\circ}5$  ; pulse 116.

18th.—8.0 A.M. Temp.  $100^{\circ}9$  ; pulse 108. 5.0 P.M. Temp.  $101^{\circ}7$  ; pulse 120. 10.0 P.M. Temp.  $101^{\circ}2$  ; pulse 120. Hip very stiff ; slough over trochanter not yet separated ; general condition worse, looks sunk and pinched.

19th.—8.0 A.M. Temp.  $100^{\circ}3$  ; pulse 108. 5.0 P.M. Temp.  $101^{\circ}8$  ; pulse 116.

20th.—8.0 A.M. Temp.  $100^{\circ}8$  ; pulse 116. 5.0 P.M. Temp.  $102^{\circ}7$  ; pulse 124.

21st.—8.0 A.M. Temp.  $100^{\circ}5$  ; pulse 112. Slough deepening ; ordered a lotion of carbolic acid, and the surrounding skin to be painted with tinct. benzoin. 5.0 P.M. Temp.  $102^{\circ}1$  ; pulse 120.

22nd.—8.0 A.M. Temp.  $100^{\circ}7$  ; pulse 116. 5.0 P.M. Temp.  $102^{\circ}2$  ; pulse 120.

23rd.—8.0 A.M. Temp.  $100^{\circ}5$  ; pulse 120. Prostrate ; complaining of aching pain over left tibia, where there are redness, tenderness, and swelling. 5.0 P.M. Temp.  $101^{\circ}6$  ; pulse 120.

24th.—8.0 A.M. Temp.  $100^{\circ}2$  ; pulse 120. 5.0 P.M. Temp.  $100^{\circ}8$  ; pulse 108.

25th.—8.0 A.M. Temp.  $100^{\circ}$  ; pulse 108. For the first time since the application of leeches, complains of pelvic pain when the bladder is distended ; slough separating. 8.0 P.M. Temp.  $102^{\circ}$ .

26th.—8.0 A.M. Temp.  $100^{\circ}5$  ; pulse 116. 8.0 P.M. Temp.  $101^{\circ}9$  ; pulse 120.

27th.—8.0 A.M. Temp.  $100^{\circ}3$  ; pulse 104. 10.0 P.M. Temp.  $100^{\circ}8$  ; pulse 112.

28th.—8.0 A.M. Temp.  $100^{\circ}$  ; pulse 108. 10.0 P.M. Temp.  $101^{\circ}1$  ; pulse 116.

29th.—8.0 A.M. Temp.  $100^{\circ}4$  ; pulse 112. Ordered six leeches to hypogastric region.

30th.—8.0 A.M. Temp.  $100^{\circ}2$  ; pulse 112. 9.0 P.M. Temp.  $101^{\circ}8$  ; pulse 112.

31st.—8.0 A.M. Temp.  $100^{\circ}3$ ; pulse 120. 9.0 P.M. Temp.  $101^{\circ}8$ ; pulse 128. Abdominal swelling not lessened; considerable pain and tenderness in the uterine region, extending laterally. Per vaginam, fulness and tenderness of all tissues surrounding the uterus; scarcely any cervix to be made out.

*August 1st.*—8.0 A.M. Temp.  $100^{\circ}8$ ; pulse 108. 10.30 P.M. Temp.  $101^{\circ}3$ ; pulse 116.

2nd.—8.0 A.M. Temp.  $100^{\circ}$ ; pulse 116. On the left side, the tenderness now reaches to the level of the umbilicus. 9.0 P.M. Temp.  $101^{\circ}9$ ; pulse 116.

3rd.—8.0 A.M. Temp.  $99^{\circ}9$ ; pulse 114. 9.0 P.M. Temp.  $101^{\circ}9$ ; pulse 112.

4th.—8.0 A.M. Temp.  $99^{\circ}8$ ; pulse 100. 9.0 P.M. Temp.  $101^{\circ}5$ ; pulse 116.

5th.—8.0 A.M. Temp.  $100^{\circ}1$ ; pulse 116. 9.0 P.M. Temp.  $102^{\circ}1$ ; pulse 120.

6th.—8.0 A.M. Temp.  $100^{\circ}1$ ; pulse 108. 5.30 P.M. Temp.  $103^{\circ}$ ; pulse 124.

7th.—8.0 A.M. Temp.  $100^{\circ}7$ ; pulse very small and variable, 112 to 124. 9.0 P.M. Temp.  $102^{\circ}6$ ; pulse 124.

8th.—8.0 A.M. Temp.  $101^{\circ}1$ ; pulse 120. 5.0 P.M. Temp.  $102^{\circ}4$ ; pulse 120.

9th.—8.0 A.M. Temp.  $100^{\circ}8$ ; pulse 112. 9.30 P.M. Temp.  $102^{\circ}4$ ; pulse 120.

10th.—8.0 A.M. Temp.  $99^{\circ}8$ ; pulse 112. 8.30 P.M. Temp.  $101^{\circ}4$ ; pulse 120.

11th.—8.0 A.M. Temp.  $101^{\circ}3$ ; pulse 112. Further increase of tenderness. 8.30 P.M. Temp.  $102^{\circ}2$ ; pulse 124.

12th.—8.0 A.M. Temp.  $99^{\circ}8$ ; pulse 112. 5.0 P.M. Temp.  $102^{\circ}3$ ; pulse 124.

13th.—8.0 A.M. Temp.  $100^{\circ}7$ ; pulse 116.

14th.—8.0 A.M. Temp.  $100^{\circ}7$ ; pulse 116. 9.0 P.M. Temp.  $102^{\circ}2$ ; pulse 112.

15th.—8.0 A.M. Temp.  $99^{\circ}2$ ; pulse 108. 10.0 P.M. Temp.  $102^{\circ}$ ; pulse 120.

16th.—8.0 A.M. Temp.  $100^{\circ}1$ ; pulse 108.

18th.—8.0 A.M. Temp.  $100^{\circ}6$ ; pulse 120. Great tenderness over hypogastrium to within an inch of the umbilicus,

extending more towards the left side. 8.0 P.M. Temp.  $103^{\circ}2$ ; pulse 124.

19th.—8.0 A.M. Temp.  $100^{\circ}2$ ; pulse 112. 8.0 P.M. Temp.  $102^{\circ}3$ ; pulse 120.

20th.—8.0 A.M. Temp.  $98^{\circ}9$ ; pulse 108. 8.0 P.M. Temp.  $102^{\circ}6$ ; pulse 128.

21st.—8.0 A.M. Temp.  $100^{\circ}0$ ; pulse 116. The deep hollow over right trochanter left by the separation of the slough, is nearly half filled, but, at the extreme upper corner, bone is exposed. 8.0 P.M. Temp.  $102^{\circ}1$ ; pulse 120.

22nd.—8.0 A.M. Temp.  $99^{\circ}5$ ; pulse 120. 8.0 P.M. Temp.  $102^{\circ}4$ ; pulse 124.

23rd.—8.0 A.M. Temp.  $98^{\circ}8$ ; pulse 108.

24th.—12.30 P.M. Temp.  $101^{\circ}4$ ; pulse 120.

26th.—8.0 A.M. Temp.  $99^{\circ}2$ ; pulse 116. 8.0 P.M. Temp.  $101^{\circ}5$ ; pulse 116.

27th.—8.0 A.M. Temp.  $99^{\circ}2$ ; pulse 116.

28th.—8.0 A.M. Temp.  $99^{\circ}4$ ; pulse 112. The surface of exposed bone is now hidden by granulations, and the immense gap is filling up quickly. No trouble now either in micturition or defecation.

31st.—Loathes food more than ever; constant nausea and acidity. Thrice the urine has escaped involuntarily during efforts at vomiting. Ordered milk and soda-water only.

September 1st.—8.0 A.M. Temp.  $101^{\circ}$ ; pulse 132. No more vomiting; cheeks unnaturally flushed. Milk diet. 6.0 P.M. Temp.  $102^{\circ}6$ .

2nd.—8.0 A.M. Temp.  $98^{\circ}8$ ; pulse 116. Perspired profusely in the night; looks better; abdominal tenderness remains the same.

8th.—8.0 A.M. Temp.  $98^{\circ}8$ ; pulse 116. Hypogastric tenderness greatly diminished; the enlarged uterus can be moved a little from side to side; it measures  $4\frac{1}{2}$  inches at its widest part, and reaches to within  $\frac{3}{4}$  inch of the umbilicus. The swelling extends rather more towards the left than the right. Tongue flabby, and presenting numerous prominent *papillæ*; no appetite. Gap over trochanter filling up; granulations swollen, pale, and indolent; long sinus



undermining upper margin. Bowels constipated. Ordered cinchona wine with aloes.

9th.—While moving position during the night, a sudden and severe pain came on in the left groin, which continuing, seemed to render the left lower limb powerless. At 6.0 A.M. the calf of the leg was attacked with pain, which became worse and worse; meantime the pain above was only felt on moving. 11.0 A.M. Pain in calf so severe as to cause the patient to scream; at the seat of pain, swelling, hardness, and exquisite tenderness. Subcutaneous injection of  $\frac{1}{4}$  gr. morphia relieved the pain instantly. Temp.  $99^{\circ}2$  6.0 P.M. The whole left leg painful on movement. Temp.  $100^{\circ}6$ .

10th.—8.0 A.M. Temp.  $100^{\circ}6$ . No return of the pain. 8.30 P.M. Temp.  $101^{\circ}$ ; pulse 128. Great pain in the muscles on bending the knee and hip.

11th.—8.0 A.M. Temp.  $98^{\circ}8$ ; pulse 116. Left leg much better; sore on right side healthier.

12th.—8.0 A.M. A little remaining soreness and stiffness of muscles in front of left thigh. Asks for a little meat.

13th.—Much better. Hypogastric swelling measures  $2\frac{1}{2}$  inches at its widest part, and reaches to within  $\frac{3}{4}$  inch of the umbilicus.

15th.—Continues better. A little general swelling of the left lower extremity. Temp.  $97^{\circ}8$ ; pulse 108.

18th.—Good appetite; a little edema of left lower limb still remaining.

22nd.—Abdominal tumour diminishing; gap over right trochanter lessened to size of a sixpence.

26th.—Examination per vaginam reveals a spacious vaginal canal, os uteri high up and central, no cervix to be felt. Body of uterus, bulging the abdominal parietes forwards, reaches to  $\frac{3}{4}$  inch below umbilicus. Can raise herself into sitting posture; ordered to have an abdominal belt. Temp.  $98^{\circ}4$ ; pulse 96.

October 3rd.—Sat up for an hour.

5th.—Uterus rather smaller; sore over trochanter healed.

9th.—No desire for food, otherwise very well. Temp. (10.0 A.M.)  $98^{\circ}2$ ; pulse 70.

27<sup>th</sup>.—Has been out of doors a little ; limps slightly on right hip. Cervix uteri can now be felt indurated and tender, protruding about  $\frac{1}{2}$  inch into the vagina. Uterus can still be felt through abdominal parietes, and remains tender, but it can no longer be mapped out as a distinct hard body.

30<sup>th</sup>.—Preparing for the sea-side. For some months the appetite remained capricious, and the limping continued. At the time of writing (May, 1875) the patient walks without limping, and is in the sixth month of her second pregnancy.

*Remarks.*—This case seems to be distinctly septicemic, the morbid material being in all probability absorbed through the lacerated surfaces of the perineum.

The absence of rigor is noteworthy ; the nervous impression which ordinarily takes that method of declaring itself, in the adult at least, appears to have manifested itself on this occasion in the form of intense nerve pain.

*(To be continued.)*

---

## NOTES ON PUERPERAL FEVER.

By F. H. V. GROSHOLZ, L.K.Q.C.P.I., L.M., &c.

As the subject of Puerperal Fever is now being so prominently brought before the medical profession, and as the interest manifested in the subject by Obstetricians appears so great, it seems becoming in all who have had any cases of puerperal fever under their care to record those cases, thus adding their share towards the solution of the difficulties with which the consideration of this terrible disease seems surrounded.

The following, I think, represent pretty fairly the class of cases that are usually met with in private practice :—

CASE I.—On the morning of September 8<sup>th</sup> I was requested to see Mrs. U., a woman aged thirty-seven years, and the mother of six children. I found her highly delirious,

and with a copious red eruption over the whole of the body ; the abdomen was very much distended and tympanitic, and pressure over the uterus caused great pain. Temperature  $104^{\circ}2$ . Pulse 120.

On inquiring into the history of the case I elicited the following facts. In the evening of September 5th, strong labour pains suddenly commenced, a midwife was sent for, and the labour speedily terminated. At that time a severe epidemic of scarlet fever was raging in the district. A child which had suffered from it in a most malignant form died, and was washed and laid out by the very woman who, the same evening, only an hour later, acted as midwife to Mrs. U.

On September 9th I met Mr. R. J. Martin, of Atherton, in consultation at the case. The patient was still delirious, but occasionally had short lucid intervals. The abdomen was still more distended and extremely tender to the touch. She was now very prostrate. Temperature  $106^{\circ}$ . Pulse 140.

I saw her again in the afternoon, and on arriving in the evening at 9 P.M., found her perfectly unconscious and sinking rapidly.

She died at 10.30 P.M., just four days after delivery.

CASE II.—Mrs. L., aged thirty-four, engaged me to attend her in her fifth confinement. I was sent for at 1 P.M. on November 22nd. As soon as I arrived I noticed that Mrs. L. was very flushed and restless. Her pulse was 120, her tongue furred and dry, and the skin preternaturally hot. At one side of her face there was a red-coloured, hard, and somewhat raised patch.

The labour terminated at 3 P.M. She was put to bed and a saline mixture given her.

On visiting her next day, well-marked symptoms of erysipelas were present ; the temperature had increased and the pulse was 125.

Some pain was felt in the abdomen, which was slightly tympanitic. Tincture of perchloride of iron was administered regularly in large doses, and subsequently combined with

sulphate of magnesia and a little morphia. Milk, beef-tea, and stimulants were also used.

On November 30th she seemed much better, but on December 1st the uterus became exquisitely painful, and the abdomen very much distended. Vomiting and diarrhea set in, the patient became delirious, and on December 2nd died.

CASE III.—Mrs. E., aged twenty-seven, a primipara, commenced to feel labour pains on May 21st at 8 P.M.

I saw her next morning and again in the afternoon. The os uteri was then moderately dilated, but the pelvis I found was small.

In spite of very good pains there was scarcely any advance, and, as symptoms of prostration began to show themselves, I sent for Dr. W. J. Martin of Walkden, who applied the long forceps.

Extraction was most difficult and tedious ; and it was not till 3 A.M. on May 23rd that she was delivered. There was a slight laceration of the perineum, and on the following day Mrs. E. complained of being very sore. The uterus became tender and painful, the abdomen distended, and the discharges excessively offensive ; the temperature and pulse both rose rapidly, and the patient became very feeble and slightly delirious.

Disinfectant injections were used, and quinine, opium, and stimulants administered internally.

For many days her life trembled in the balance, but eventually defervescence commenced, the pain subsided, her system rallied, and in five weeks she was up and in moderate health.

CASE IV.—Early in the afternoon of January 31st I was sent for to attend Miss P., a primipara, aged twenty-one. Labour had commenced on the morning of January 29th, but her mother, who was a midwife, thought all was going on favourably, and that she would be "able to manage herself." Just before my arrival Miss P. had a severe convulsion. On examination I found the head very firmly impacted in a small pelvis. I applied the long forceps and administered



ergot, but the difficulty of extraction was so great that it was more than an hour before delivery was accomplished. In about sixteen minutes after the birth of the child the placenta came away, and all seemed going on well.

In half an hour, however, the most terrible hemorrhage commenced, and very soon Miss P. was in a state of syncope. As long as I grasped the uterus firmly in my hands I could restrain the hemorrhage, but the moment I diminished the pressure the blood gushed forth again. Every means were tried, and eventually, after employing pressure, ergot, and cold, and plugging the vagina, the hemorrhage was stopped, but the patient was left in such a fearfully low state, that for long I despaired of her recovery ; however, in a few hours she rallied, and I left her comparatively comfortable.

The next day she was feverish ; the uterus was excessively tender, and there was hardly any discharge. These symptoms continued, and the patient daily grew worse, becoming highly delirious, and the face assuming a pinched and anxious look.

Disinfectant injections were used as there was now a most horribly offensive discharge ; stimulants, quinine and opium were administered, and turpentine stupes applied to the abdomen, which was very painful and distended.

A gradual amelioration of the symptoms commenced, and in three weeks the patient was convalescent.

CASE V.—Mrs. W., a very small and delicate woman, aged twenty-one, was delivered of her first child on June 18th. The labour was very long and tedious, and the perineum was lacerated. All went on well for two days, but when I then went to see her I was quite overpowered with a most terrible odour, which I found proceeded from several napkins which the patient was using, and which were saturated with a most highly offensive discharge.

She seemed very unwell, and I ordered a purgative to be given her. However, she rapidly went worse ; tenderness over the uterus, pain and distension of the abdomen quickly making their appearance.

The same mode of treatment was employed as in the

previous case, and eventually, at the end of a month, the patient was out of danger.

*Remarks.*—On attempting a classification of these cases in accordance with the *origin* of the *poison* producing the puerperal fever, we are naturally led to place them in two divisions ; the first comprising those cases in which the infecting matter is derived from without ; and the second those in which it has its origin in the infected organism itself.

For the reception and development in the system of the septic poison it is of course necessary that there should be some recent wounds, and such are always present even after a natural labour ; as not only does the separation of the placenta leave the mouths of the uterine vessels open, but there is also in most cases some laceration of the cervix and vulva.

In Case I. there can be no doubt that the midwife was the means of introducing the scarlatina poison into the system of the parturient woman.

She had not, I found, even changed the clothes she wore when laying out the child, nor employed any kind of disinfectant ; but had gone just as she was, laden with the deadly poison, direct from the chamber of death to the bedside of the lying-in woman.

Such gross carelessness is highly culpable ; but ignorance was her plea, and how can we wonder at such occurrences when the law permits any ignorant woman to practise as a midwife, without insisting upon either previous training, or any test of her knowledge and fitness for the responsible duties she undertakes ?

Case II. seems to present features which might place it in either of the classes we have mentioned, for at first sight it seems to be a case of auto-infection ; but on closer inspection we come to the more probable conclusion that the poison was introduced from without, that is, septic germs from the localized inflammation that was going on in the skin were absorbed by the abraded uterine surface, and these developing produced uterine, and subsequently peritoneal inflammation.

The remaining three are all cases of auto-infection ; in each there was decomposition of matter retained in the utero-vaginal canal, and as there were solutions of continuity in the mucous membrane lining that passage, the septic matter was quickly absorbed, and local inflammation ending in severe constitutional disturbance was produced.

As to the Treatment pursued, it may briefly be described as stimulant and eliminative. Strong beef-tea, milk, and wine, in small quantities, were given repeatedly.

Purgatives were administered till the bowels were freely opened ; turpentine stupes were applied to the abdomen, and disinfectant injections freely used to the vagina. Sedatives to allay irritation and procure sleep, and oxalate of cerium to restrain excessive vomiting, were also employed.

And here, in passing, I would just mention the exceedingly satisfactory evidence I have had of the beneficial effects of oxalate of cerium in many other kinds of vomiting besides that dependent on pregnancy, to the treatment of which I believe its use has been hitherto almost entirely restricted.

The prognosis of puerperal fever, judging from the above cases, would seem to be more favourable in cases of auto-infection than in those in which the poison is introduced from without, or, in other words, where the constitutional disorder is dependent on the uterine inflammation there seems better hope of recovery than where the uterine and abdominal symptoms are secondary to the general affection. Though I was actively engaged in midwifery practice, attending from twelve to twenty confinements a month, in no instance did I convey the puerperal fever from one patient to another.

All the cases of puerperal fever were strictly sporadic, and the disease showed no tendency to become epidemic.

---

## Reports of Hospital Practice.

---

### SAMARITAN HOSPITAL.

#### ON THE PERFORMANCE OF OVARIOTOMY TWICE ON THE SAME PATIENT.

By T. SPENCER WELLS, F.R.C.S.

Surgeon to the Queen's Household and to the Samaritan Hospital for Women.

THE following remarks were made at the bedside in the Samaritan Hospital on the 16th of June, 1875, not as a formal lecture, but in conversation with several German, Italian, and American surgeons who were present, and in reply to their questions:—

“This young woman is a dressmaker. She is thirty-two years old. Some of you saw me remove a multilocular cyst of the right ovary from her abdomen just a fortnight ago, and apply a clamp upon a broad short pedicle. She has recovered without one unfavourable symptom. The bowels acted on the eleventh day, and the clamp came off on the twelfth day. It had been very tight, the integuments being drawn back almost to the sacrum, but she had very little pain. The wound is rather slow in healing, I suppose from the neighbourhood of the cicatrix, left after the removal of the other ovary five years ago, but she is on the sofa on the fifteenth day after operation, and thinks us too strict in not allowing her to get up. She has suffered much less after the second operation than she did after the first, when the pedicle was tied and the ligature returned.

“The first operation was performed in this hospital on the 18th of May, 1870. An ovarian cyst of very rapid growth, and extensively adherent to the abdominal wall and to the omentum, was removed. A very short pedicle on the left side was tied in two portions with whipcord. The ends of the cord were cut off close to the knots, and returned with the pedicle. Several silk ligatures were tied to bleeding shreds of omentum and returned. Projecting from the right ovary was a cyst as large as an orange. This I laid open by an incision, emptied, and returned with the rest of the ovary, rather than remove it, as it appeared to be healthy. The



solid part of the tumour removed weighed 3 lbs., and the fluid contents measured 8 pints. She suffered a good deal after the operation from pain and vomiting till the fifth day, the temperature rising to  $101^{\circ}4$ , and pulse to 124; but she made a good recovery, and went to Ramsgate twenty-four days after the operation. She remained in good health for four years, earning her living as a dressmaker, and menstruating regularly until a year ago. Then the abdomen began to enlarge and menstruation became irregular, recurring at intervals of two or three weeks, and with much pain on the *left* side of the abdomen. Increase was not nearly so rapid as with the other cyst, but the abdomen was so large in May, that I removed the tumour on the 2nd of June, as you saw. The incision was carried about half an inch to the right of the former cicatrix. On dividing the peritoneum, some omentum (which was adhering in some places to the cicatrix) protruded. On pushing this aside a free cyst was tapped, emptied, and drawn out. A short pedicle on the right side was secured in a middle-sized clamp, and kept out with some traction. Two vessels in the separated omentum were tied. At the right of the left ovary there was a small, hard substance, doubtless the whipcord which secured the pedicle at the first operation. I saw nothing of any of the silk ligatures which had been applied to the omentum. The cyst was nearly single, only weighing 9 ounces. It contained 8 pints of fluid. She recovered without any sickness, with much less pain than after the first operation; the temperature not rising over  $100^{\circ}8$ , and the pulse being generally about 88, the highest 108.

“At the first operation I was doubtful if I should remove both ovaries, as the second contained a cyst. I preferred only to lay open the cyst for several reasons. She was then only twenty-seven years old, and might marry. The rest of the ovary was healthy. I had seen other cases where patients had married and borne children, although I had punctured cysts in the remaining ovary, and where there had been no return of disease, and I did not wish to add to the risk of the single ovariectomy by removing the second ovary. If she recovered the ovary might remain healthy. If it be-

came diseased it might be removed. Actually it did remain for four years without any sign of disease, and when it did enlarge it was removed. This is, I think, the safest course to pursue in similar cases. In my book on diseases of the ovaries, any of you who are interested in this subject may find a chapter on the removal of both ovaries at one operation; and another chapter on ovariectomy performed twice on the same patient. Of four cases in which I had performed the second operation at the time of publication in 1872, two recovered and two died. In one case the first operation had been performed by Mr. Baker Brown. So that of 500 cases of ovariectomy in my own practice, in only three had a second operation been called for. Since June, 1872, I have done more than 200 more first operations, bringing up the number to 710, and I have only had one other case besides this one of second ovariectomy. That was a young lady, a patient of Mr. Bishop of Tunbridge, who also recovered quite as well as after the first operation. Thus the total number of cases of ovariectomy performed by me for the second time on the same patient amounts to six, with a result of four recoveries and two deaths."

---

## THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

JULY, 1875.

---

### DENIDATION.

PERHAPS the most satisfactory and important work connected with our branch of medicine which has been done within the last few years, is that which has reference to the anatomy and physiology of the uterine mucous membrane. But notwithstanding all this excellent labour, the periodic changes which take place in the lining of the uterus during its unimpregnated life still remain undecided. Dr. Tyler Smith, who was one of the earliest to write upon this sub-

ject, arrived at very definite conclusions. He had examined the uteri of several women who had died during the catamenial flow, and in each found the mucous membrane of the body of the uterus either in a state of dissolution or entirely wanting. In one case, in which he was assisted by Dr. Handfield Jones, in examining the uterus with a microscope, no traces of the epithelium or of the utricular glands could be found. This evidence has been corroborated and supplemented very recently by the elaborate researches of Dr. John Williams in a paper which appeared in this Journal; and further evidence confirming his views has come to us from Dr. Barnsfather, of Cincinnati. He says that for a number of years he has been examining microscopically from month to month the menstrual discharges of women. From his investigations he finds in all of them exfoliation of the mucous membrane, even from females perfectly healthy. In those suffering from dysmenorrhea the membrane was hypertrophied, and came away in larger pieces. Dr. Aveling also agrees with these authorities, and has proposed to call this removal of the developed mucous membrane "denidation." Unfortunately, in spite of all this concurrent evidence, complete unanimity does not yet exist, for within the last few months Dr. Engelmann has stated, as the result of his observations, that in not one of the many uteri he has examined after the cessation of the catamenia has the mucous membrane, or even its superficial layer, been wanting. He believes the tumefied mucous membrane to be removed by a gradual disintegration of its elements and rapid absorption, but admits that in some cases the entire upper stratum may be detached and expelled *in toto* as decidua menstrualis. He does not, however, explain why, when the normal physiological process is increased to morbid intensity, the disintegrating and absorbing processes should fail in removing the developed membrane in what he believes to be the normal manner. There are at present, therefore, two contending theories—the desquamative and the involutive. None deny the existence of periodic changes in the lining of the body of the uterus, preparing it for the reception and retention of the ovum should it be impregnated—a pro-

cess, which since the time of Harvey has been likened to the nesting of birds, or nidation—the point which remains to be positively decided by the repeated examinations of different observers is the exact way in which the abortive physiological process is terminated. We commend the definite solution of this problem to our readers. At present the preponderance of evidence is with those who believe denidation to be an act of desquamation. If, however, these observers have been so unfortunate as to have always investigated uteri in an abnormal condition, and the involution theory be right, time will undoubtedly prove the fact, and determine the ultimate acceptance of the truth.

---

### *New Invention.*

#### AN INSTRUMENT TO FACILITATE REMOVAL OF GROWTHS FROM THE FEMALE URETHRA.

MESSRS. KRÖHNE & SESEMANN, of Duke Street, Manchester Square, have submitted to our inspection a simple instrument, made by the direction of Mr. Thomas Bryant, to facilitate the removal of growths from the female urethra by means of the galvanic cautery. The instrument, as shown in the engraving, is a dilator, speculum, and protector.



As a dilator, it is readily introduced without any previous dilatation, when the patient is fully under the influence of an anesthetic ; and it can be rotated, and moved backwards and forwards. As a speculum, it allows inspection of every part of the canal, and exposes to view any growths (the growths falling into the slot of the instrument) which may be situated at the anterior or posterior part of the urethra. As a protector, it covers every part of the canal with a non-conducting surface, leaving only the growths exposed ; these can thus be readily removed with a platinum wire loop, or destroyed



by a larger or smaller cautery, galvanic or otherwise. Mr. Bryant's dilators are made in sets of three different sizes, of boxwood and of ivory. They have been already freely used, and with marked success.

---

## Abstracts of Societies' Proceedings.

---

### OBSTETRICAL SOCIETY OF LONDON.

*Meeting, June 2nd, 1875.*

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., *President,*  
*in the Chair.*

*On the Relation of Puerperal Fever to the Infective Diseases  
and Pyæmia.*

(Continued from p. 202.)

Dr. SAVAGE: I was quite prepared to commence the discussion this evening; but as I hear with great satisfaction that we are favoured with the presence of Dr. Farre, I should much prefer to listen to any observations that he may make.

Dr. ARTHUR FARRE: I am very sorry if I have disturbed the order of the discussion, but I rise in obedience to the invitation conveyed to me by you, sir, and, as I understand, by the Society at large, to take part in the debate. As an honorary Fellow of this Society, I thank you for the compliment you have thus paid me. It has occurred to me to think what are the principal objects which the Fellows of this Society had in view in promoting this discussion. It was hardly, I should think, to elicit anything very new from any of the speakers; but the purpose probably was, that we should compare our observations, that we should as it were take stock of our knowledge, and incite each other to further inquiry and investigation; and, with this idea, I have come to-night to take what little part I may in this discussion. In speaking of taking stock of our knowledge, I have it in my mind that we should not only compare our observations together as men of the present day, but that we should also take into account the labours of men who have long preceded us. I have no doubt that the Fellows of the Society have in their recollection those excellent works of such men as Denman, Lees, Hull, and various other workers at the end of the last century, who gave their attention to this subject. I have no doubt that a great deal of information might be evoked from a reperusal of the works of those men. Actuated by this idea, and looking into some of these

works, I have been much struck by the observations of Dr. Kirkland upon this subject in his most interesting essay, which I have no doubt is well known to the Fellows of this Society, on Childbed Fever. Dr. Kirkland commences his essay by inquiring, What is properly puerperal fever? and here he seems to open up the very subject we have met to discuss to-night. It is interesting to observe how a man more than a century ago took in all the points which we have now met to discuss, or most of them, and valued much in the same way as we now value these very same questions. He begins, for example, by admitting that every disease happening to a woman in childbed, connected with the act of parturition, may perhaps be properly termed puerperal fever, though some writers use this by way of eminence to distinguish that form which they think most dangerous, from milk-fever and other forms of fever connected with child-bearing. Having admitted this, he proceeds to offer the following objections. He says that, if by using the term puerperal fever these writers had chosen to fix upon any particular form, it might have answered a good purpose; but he does not see what good will arise from combining together such diseases as may happen to a woman in child-bearing, as inflammation of the uterus, or putrefaction of clots within the uterus producing fever, or a fever engendered from without by the admission of atmospheric poisons. Then he adduces several such instances as the proposer of this question has adduced. He mentions the case of small-pox occurring to a woman in childbed, hospital fever, and the like; and then he discusses the propriety of considering these as puerperal fevers at all. He points out very accurately that, when we give a specific name to a disease, the name ought rather to indicate the disease than the condition of the individual in whom it occurs. Then he makes another very excellent observation, which I think we should all keep in view—namely, that when we speak of fever, we are not speaking of a disease, but of a sign, or a set of signs, indicative of a mischief going on within. He then sums up by saying, “In short, the writers in the present day seem to include under this name of puerperal fever a thousand disorders.” Now that, I think, is as nearly as possible in accordance with the views of the Fellow of this Society who has brought the matter under our consideration to-night. He objects to the term “puerperal fever,” and thinks that we ought to distinguish the different diseases which have been classed together under this title. I suppose he had in mind some such idea as that which actuated Lord Bacon, in reminding us that we are all too much in the habit of neglecting things, and worshipping specious names. I suppose that, in the use of the term puerperal fever, we have for a long time been worshipping a specious name, and not considering sufficiently the thing which that name indicates. Having asked your attention to the works of this author, and shown how far his idea seemed to be in a measure coincident with those which are now under discussion, I would proceed to refer to a few of the circumstances connected with

the definition of puerperal fever, which has appeared in the work known as the "Nomenclature of Diseases," emanating from the College of Physicians. I should not have done this, if the author of these questions had not done me the honour to mention my name in connexion with the part which I took in the Nomenclature Committee of the College of Physicians in respect to this matter, as one whose business it was mainly to suggest to that Committee the various definitions that should be accepted in regard to those subjects that come particularly under my department. I think some little misapprehension must have arisen as to the objects of the Committee in framing that definition and offering it for the consideration of the profession. In order to make this clear, perhaps you will allow me to call attention to one or two observations that are made in the introduction to this work. Few men, I believe, read the preface to a book, and it may be that many of the Fellows of this Society have not read the few observations that precede the catalogue of diseases known as the nomenclature of the College of Physicians. They begin by saying: "The object of this nomenclature is to aid in perfecting the statistical registration of diseases, with a view to the discovery of statistical truths concerning their history, nature, and phenomena." Beyond this they do not profess to proceed. They then call attention to this fact, that whilst, generally speaking, the name of a disease only is given—as small-pox, for instance, which requires no explanation—there must naturally occur some that would seem to require explanation; and to those they have added a definition. But then they distinctly say that those diseases only have been defined which seem to require it; and the definitions have been framed for the purpose of identification only, not as explanations of the phenomena of disease. Hence, when a definition is offered, it is offered under these circumstances; and there is no attempt at dogmatic teaching, or entering upon any other considerations than those which would enable practitioners, whose business it was to register diseases under their respective titles, to identify the disease that was intended. I think it is as well to keep this in view with reference to the observations made upon this subject by the proposer of these questions. Now, I would like for a minute to draw attention to this definition of puerperal fever here offered. It is defined to be "a continuous fever communicable by contagion." I ask your attention to the word "communicable," for there was a good deal of discussion upon it. It is simply meant to convey the idea that the disease may be communicated, or is capable of being communicated; but that it does not necessarily arise in that way—that it may have a more spontaneous origin. "A continued fever communicable by contagion, occurring in connexion with childbirth, and often associated with extensive local lesions, especially of the uterine system." That word "often" was also made the subject of comment; and I wish to observe, in connexion with what has gone before, that the object of introducing that word was simply to call the attention of

those who would have to register disease to the frequency with which these complications arise ; there was no intention of entering upon any doctrinal question, or expressing any views as to the precise nature of the disease under consideration. Then there is added a note in which those who register diseases are requested to return under the name "puerperal fever" the more important local lesions, such as peritonitis, effusions into serous and synovial cavities, and the like. That is done for the same purpose, so that nothing might escape observation, and that every circumstance connected with the disease should be carefully recorded, for the purpose of collecting facts. I will now allude to the place which this term puerperal fever occupies, because I observe that it has excited much attention. It is not placed among the fevers at all ; it is placed among the general diseases, but it is placed far away from the fevers. First of all come the infective fevers—small-pox, cow-pox, chicken-pox, measles, and the like ; then, lower down in the list, follow pyemia, erysipelas, etc. ; then comes puerperal fever. It was placed in this position, because the Nomenclature Committee were entirely at a loss to know where it ought to come in the catalogue of diseases. It found a place of refuge at last at the bottom of the list, after pyemia, erysipelas, etc. ; and the Committee had it in their minds to indicate by this place which they assigned to the disease the apparent connexion of puerperal fever with those antecedently named diseases. I would further call attention to the fact that, independently of this, there is in the nomenclature a list of those affections which are consequent on parturition ; and this is quite apart from the subject which we have now been considering. Now, in connexion with the same subject, I would ask the Fellows to consider if there be any objection to this term puerperal fever. What is meant by the term fever at all? What are our views with regard to fever? Do we consider fever as a disease to which we ought to attach a special name? or is it only a symptom (as Dr. Kirkland long ago reminded us) of a disease? Do we regard fever—that is, the pyrexial action—as anything more than an indication of an underlying disease? If we keep this in view, we shall find the difficulties that continually occur in framing a nomenclature when we take the name of a disease to indicate that which does not signify its nature. These few observations I thought it right to offer with respect to this part of the subject. And now, as each of us is expected to answer to the best of his ability the questions put to us, I will take them in turn. But first I must confess that, in approaching this subject, I feel a great difficulty on account of the manner in which it is brought under our notice. I observe that these questions, which only profess to be questions, assume rather the form of an argument ; they are rather put to us in a syllogistic form ; and they are intended to lead up to a conclusion, though no conclusion is stated. Obviously, they are questions merely, and they are left to us to consider. But, if we are to follow the order of these questions, I must say it is with some difficulty that I



approach the subject, inasmuch as that which we have to deal with is associated with a great deal of imperfect knowledge, and we cannot precisely formulate all the facts so easily in the shape which is offered to us. Still, with this difficulty before me, I will endeavour to the best of my ability to notice each of these three questions. The first, as you well know, relates to this : Whether there is any form of continued fever communicated by actual contagion or infection, in connexion with childbirth, which is distinctly caused by a special morbid poison, and as definitive in its progress as the diseases which are taken in illustration. To that question I can return a very plain answer, and say that I am not aware of any form of contagious or infectious fever connected with childbirth which is distinctly caused by a special morbid poison (if by that be intended a specific virus), and which has a definite progress. I am not acquainted with such a disease. The next question and the following one must, I think, be taken together. "May all forms of puerperal fever be referred to attacks of some infective continued fever—as scarlet fever or measles—occurring in connexion with childbirth, on the one hand ; or, on the other, to some form of surgical fever, or to erysipelas, caused by or associated with changes in the uterus and neighbouring parts following the process of childbirth?" I am unwilling to give an answer to this question, because I wish to keep open another subject, which seems to be closed by the question which follows it. If I were to give an answer to that question, I should be shut out entirely by the form that is laid before us from giving a satisfactory reply to the third, which is this : "If all cases of contagious and infectious diseases which occur under other conditions than that of childbirth are set aside, does there remain any such disease as puerperal fever?" Now, it is to the form of this question that I wish to take exception ; because it appears to me that, in this question, the several infectious and contagious diseases are treated as if they occurred in the body of a man, a non-parturient woman, or a child ; whereas I wish to keep together the connexion which associates them with the act of parturition, and shows them as occurring in the parturient woman ; and I think it is the separation of these two conditions that occasions the difficulty I now encounter. I cannot, consistently with my experience, agree to consider these diseases as one and the same when they occur to the non-parturient woman and the parturient woman. I think here there are several circumstances that have been taken into account. In the first place, when any of these attack a parturient woman, she is not in the same condition as a healthy person whom they may attack. Scarlatina, measles, and the like, occurring to healthy persons, do not enter the bodies of those individuals in the same conditions as they enter the body of a parturient woman. Let us remember, in the first instance, she has been in some degree weakened by the previous confinement. It may be said, "Well, but in some cases we have a parallel—cases in which the diseases here mentioned follow after an operation." But I

will go further. We have two processes going on in the body of the lying-in woman naturally, which do not occur in the person of one ordinarily struck down by one of these diseases. First, I refer to the milk-process, the milk-secretion, and to that attendant disturbance of the constitution which we term milk-fever. That is one element to be taken into consideration, and one disturbing force. And there is another and a much more important one, and that is the change which goes on in the uterus, and which we all know as the involution process. Nothing of this kind occurs in the body of an ordinary recipient of those diseases. This natural process, as we know, goes on in the body of the woman at a time when she is most likely to be attacked with the diseases that we have met to consider ; and when any of these diseases attack a parturient woman, this process is materially interrupted. Now we know that when this process is interrupted, there is an arrest of all that eliminative action which that involution process implies ; and we may fairly conclude that, in consequence of this arrest, there is some accumulation in the body of those effete matters which ought to be expelled from the system ; and this, added to the already existing blood-dyscrasia, must very much aggravate the disease. Nor is it quite certain that this may not add a new form of sepsis to that which is already in the blood. Before admitting, therefore, that there is no such thing as puerperal fever, I would say that all these things ought to be taken into account. It has been thought by some that such a thing as a pure and independent puerperal fever may be found. I should be sorry to shut out that idea altogether as one that is impracticable, and not to be entertained. I think it is very easy to see where we shall find a puerperal fever, if we are to look for it at all. I mean a fever not caused by any of the circumstances here suggested to us ; it is quite plain that we must look for it here, in the arrest of some of these processes that I have been referring to. If we are to look for a puerperal fever at all, one that can be properly so called, a disease which is *sui generis*, we shall probably find it in the interruption of these healthy processes. Then, if I am not trespassing too long upon your attention, I should like to express a little more nearly my own views with regard to puerperal fever. I very much regret now that, when we were framing this definition, it did not occur to me to suggest that the term "puerperal fever" should be rendered "puerperal fevers," and that a note should be placed to the effect that, under this name, is intended to be comprehended a class of continued fevers communicable by contagion occurring in connexion with childbirth, and so on. I think we should have got over a good deal of the objection taken to this word, if we had considered this not as a special form of fever, as it appears to be when it occurs in the "Nomenclature," but as a group of fevers connected with the act of childbirth. If I were asked how I would classify the diseases that we have been in the habit of grouping together under the name of puerperal fever, I would divide them into these three, as it

appears to me, very natural classes. We have to look at this matter in a practical light, and see how we shall consider this subject, not by the teachings of the dissecting-room, but as we see it in the lying-in chamber, when asked, as we constantly are, to determine the nature of the disease that we are called in to examine and advise upon. I have been in the habit of dividing all cases that can in any shape be termed puerperal fevers into three classes. First, there are those simple fevers which may, for the want of a better term, be called irritative fevers; and under that name I would class that febrile action which results from simple mammary irritation, and is known as milk-fever; those simple febrile consequences of a traumatic origin which result from slight injuries to the soft parts, laceration, and the like, though those are of rare occurrence; and those pyrexial states which are of a fugitive and transient nature (I think, in a second catalogue in the "Nomenclature," we call them puerperal ephemera). First, then, we have those irritative fevers arising from some local irritation, and not implying blood-infection of any kind. In the second class, I would place those infective fevers which are not of a specific origin, and in which the poison or sepsis does not undergo a distinct period of incubation, though I have no doubt there is a poison conveyed into the blood, in what way it is not necessary now to consider. Under the name, then, of the milder forms of infective fevers, I would class all those in which the infection is not of a specific nature, in which the process does not undergo a period of evolution, a period of development, and in which the consequences follow no definite order. Then, in the third class, I would include eruptive fevers, and those which depend upon a blood-infection, the poison following a specific course, having a regular period of incubation, and terminating in those several diseases, eruptive fevers and the like, which occur, of course, to the lying-in woman in common with others. These I do not consider as in any way taking any part in the puerperal fever. The only fevers that I would acknowledge as in any way connected with the puerperal state, are the first two. I would entirely exclude the others from the catalogue; but in the present imperfect state of our knowledge on these questions, I would like to retain these diseases under some name corresponding to that under which we now group them together. If I proposed any alternative, I would change the name. One that I am in the habit of employing is post-partum fevers. I have long discarded the name puerperal fever, and have used the term post-partum fevers, which implies no theory at all, but simply expresses the fact that they occur to women after delivery. I think that, under that head, we might include all these three forms of fever. If, for the purposes of statistical registration, it were desired to know how these names should be registered, I would suggest that those cases should be registered under a separate head in connexion with childbirth, never mind in what relation they stand. I would place, for instance, "scarlatina" and "scarlatina in the puerperal form" in



parallel columns ; “erysipelas” and “erysipelas in the puerperal form.” In this way, we should satisfy the requirements of the Nomenclature Committee, and not foreclose that most important question that we have now under discussion. I dare not trespass longer upon your time ; I cannot venture to offer any further observations upon this subject ; but I may at some future time, if you please, further develop my ideas, and particularly call the attention of the Society to this circumstance, which appears to me to have been left too much out of consideration—it seems to me that, in considering the pathology of these cases, we have given too much attention to the influence of a blood-poison, and have lost sight of that intermediate condition—or, rather, we have not attempted to determine it—which intervenes between the entrance of the poison into the blood and the development of it in the various ways and forms which give significant names to the diseases under which they are classed. I think we have too much left out of consideration the influence of the nervous system, the influence of these poisons upon the nervous centres ; and I think if we were to direct our attention further to this particular, we should be able to throw a great deal more light upon these diseases, and, perhaps, get over some of the difficulties that now surround us.

Dr. SAVAGE : I feel under some embarrassment after hearing Dr. Farre. I certainly came prepared with very decided views, but they have been somewhat shaken by what has fallen from that eminent authority. With regard to these questions of Mr. Spencer Wells, I do not know that Dr. Farre finds any more difficulty in dealing with them than the rest of us. Evidently, the object is to catch us at every corner, so that we might not escape from the subject in any way. I can scarcely agree with Dr. Farre in leaving the subject in so uncertain a state as he proposes. We have already had, in the course of the last two discussions, some positive statements made to us which touch closely on the real nature of the complaint. I was at an early period prepared to argue strongly against any pathological relation or affinity between fevers and these under discussion ; but I listened attentively to what fell from Dr. Squire and some of our country friends, who sustained so well the argument on that point, that I think it scarcely necessary to trouble the Society with any observations of mine. I only wish to say that there seemed to be, on the part of Dr. Barnes and Dr. Braxton Hicks, a clinging to the opinion that there was a sort of connexion between scarlet fever, or the sort of scarlet fever that they had seen, and puerperal septicemia. It could not be the orthodox scarlet fever, because it is clear that you can have in a parturient woman distinct scarlet fever, and she will get over the scarlet fever and the parturition exceedingly well. I suspect these gentlemen mean some other form—a bastard form of fever—that sort of thing spoken of by pathologists, in their new vocabulary, when they say that a thing exists “potentially,” and not actually. I will not criticise anything that has fallen from these



authorities ; I will only say that I find a difficulty in recognising the presence of any disease in the absence of its distinctive symptoms. Now, is this fever in any sense of the word? First, we have decided that it is utterly distinct from such things as we have called fever. Is it fever? We all agree that there must be some heat of the skin, some elevation of the temperature in anything that you can call fever. Now, I know that you may have many of these fatal cases of septicemia in a parturient woman running their course in eight days, without any elevation of temperature from first to last. Or it happens that there is an elevation on the second day after you discover the presence of the poison. I agree with Dr. Farre, that a great deal may happen before you discover the presence of the poison. It is not impossible that, if we kept our eyes open and watched, we might anticipate the effect of the poison ; but it is seldom done—we see the cases late and they are generally fatal. In the matter of temperature, then, I think it is clear that the disease should not be called fever at all. Then, the next point is, whether it is septicemia peculiar to a parturient woman, not like any other septicemia. I have seen septicemia from operations on silly women who are not intended to have a family, who are intended to be barren ; septicemia from the introduction of that most treacherous of all things, the uterine stem ; septicemia from legitimate operations upon the uterus ; septicemia after ovarian operations. I have seen all these, and I have also seen (but I am no authority on that point) surgical septicemia in wards, and I can distinguish no difference between it and septicemia in parturient women. I think it is simple septicemia—that is, a disease which you must set apart from every other sort of disease likely to occur in parturient women, such as the disease you call fever in connexion with the secretion of milk, and other fevers. It stands quite apart and alone, and I think it of great importance that we should consider it as such. I think, if we do not, we shall be likely to overlook it in our patients ; and instead of applying, as we might with some effect at an early date, some remedy, we may let the case go until it is past hope. Now, do we know anything of the poison? Have any of us seen or touched or felt it? Mr. Wells will tell you by and by, if he please, that in the course of ovarian septicemia he sometimes taps the peritoneum with great effect, and lets out a great quantity of it. There must be poison in or about it, because the woman is better when it is let out, is worse again when it accumulates, and again better when it is let out. Then, there are the processes, which it would take too long to enter into, such as washing the peritoneum and the like. If the case be taken in time, the woman recovers. Evidently, in such a case, we are dealing with a poison in some shape or other. Is the whole fluid poisonous, or is there something special in it? Our German *confrères* have helped us a good deal in this way, in a negative sense. The ordinary results of putrefactive changes in dead matter should be carefully distinguished from the changes which occur in fermentation ; they are

two different things, but are often confounded. We have sulphuretted hydrogen, sulphuret of ammonium, and butyric acid ; there are other things no doubt, but these have been discovered. I am referring to the experiments of Billroth and Weber. They take a solution of each of these things and throw it into the cellular tissue of dogs and horses, and produce in each instance septicemia, the animals dying. You could not tell whether they had chills or not ; still they died, and it was clear that they were killed through septicemia. I must not forget to say that sometimes the severity of the symptoms did accord with the offensiveness of the liquid. I have had my fingers in the liquid, and could not eat any dinner for a week or two ; and I have no doubt that if I attended a parturient woman with my fingers in that state she would have had septicemia. In the case of the peritoneum, it is clear that this question can be answered. What are the channels for the introduction of this fluid into the system ? Unless the peritoneum be inflamed in the small vessels, it is clear it must be through the absorbents. There is an instructive experiment by Dr. Sanderson, in his beautiful book lately published, in which he shows that liquids may possibly enter the system through the vessels, but what he calls particulate poisons cannot—they must enter in by the absorbents. I do not know what he alludes to by those poisons, but I refer gentlemen who wish to know more about it to the book itself. It is quite clear, then, that in the case of the peritoneum, the poison must enter in through the absorbents—that is, if it be particulate, because it is asserted by the more recent pathologists that all septic matter is particulate, which, I think, is a mistake. I am sure one of Billroth's experiments shows that it is a mistake, because a solution of sulphuretted hydrogen cannot be particulate. Let us apply this to the uterus. We are first in some difficulty unless you acknowledge the existence of absorbents such as you have them in the peritoneum. I think these absorbents are denied by some authorities. They are denied in a recent communication to our *Transactions*. Still, they do exist. I make this remark the more, because there was a theory started by Dr. Graily Hewitt the other day (as I saw by the report), about what he called the burglar-theory ; that is, he said he had no doubt that in the majority of instances there was an imperfect contraction of the uterus, a clot within it and clots within the veins ; and the locks being taken off the doors, and the doors thrown wide open, the thief stepped in. In the first place, I can scarcely imagine such a state of the uterus without considerable hemorrhage. He also spoke of sinuses of the uterus. Now, there are no sinuses of the uterus in my opinion. I do not know of any open sinuses after parturition at all. I know it is exceedingly difficult in the dead subject to separate the placenta from the uterus without tearing into the veins. Nothing is so easy as to separate the placenta—*e.g.* in placenta previa ; you come into contact with one of the large veins, and you feel the small arteries, veins, and absorbents yielding ; you separate the whole, and there is the uterus in a dilated state, and no hemor-

rhage. That could not be if there were open sinuses. Even if you have clots in the veins, it is a matter of demonstration, since Virchow's discovery of the true mechanism of embola, that clots in the veins do not get into the circulation by themselves—they never break up. That was the old opinion of Dr. Lee, whose name has not been sufficiently mentioned, except by Mr. Hutchinson. Dr. Lee showed incontestably that one of the concomitants of this disease, septicemia, was a clogging of the veins; but it was Virchow who demonstrated (it was quite a demonstration) that the clots will not break up; that is not the way that the clots pass in and disappear. Books on pathology, especially German books, describe how the veins empty themselves, but time will not permit me to refer to that. We have arrived at this, that you cannot get the poison through the veins. We have, then, to do with another channel: the lymphatics. Now, the lymphatics have been shown by Lee to be very peculiar in the mucous membrane of the uterus. The surface is highly absorbent, and the small vessels of the uterus are invaginated in these lymphatics. It is a very curious arrangement shown in Leopold's book. I fancy I have seen them. I do not say anything as to the microscope, since I find a power of fifty talked of, whereas I found a power of twelve difficult to manage. It seems to me, then, that as a matter of demonstration we have brought it down to this—that we have seen the septic matter, we have touched it, and smelled it; I have mentioned experiments in regard to the composition of it; and we now can have no difficulty in believing that some septic stuff will accumulate in the interior of the uterus and enter the circulation, as it did in the case of the peritoneum. Now we come to the difficulty about the fingers. I said that if I attended a woman in her confinement with my fingers in that state I had no doubt she would have septicemia, and I really have no doubt of it. You remember that we had some painful statements from a gentleman in the country, who seemed to have had his fingers in that condition, and who lost case after case, and was obliged to give up his practice. I know it is difficult for most of us to comprehend how it is that the mere approximation of a finger should set going this curious septic thing. Now, every old woman will tell you that, if she puts a piece of fetid meat in the cupboard, though it does not touch the rest, all will be turned in the morning. The other day, when passing a butcher's shop in a large district, I found him hard at work with all his men. Having these perplexing questions in my mind, I walked in to hear what sort of disinfectants he used, and he said, "I use no disinfectant; I wash everything with soap and water every night, hooks, cleavers, knives, and everything, then I admit the meat; if I did not it would all be turned towards the morning." I do not myself profess to understand or explain how it is that the approximation of bad meat to good will turn the good bad; but so it is, and we can apply that fact, though I have no explanation of it, to show how contact with a fetid finger would lead to septicemia in a woman.



Then we come to the question of pyemia and bacteria. Now, the question of bacteria has occupied the minds of leading members of our profession during six hours, at three meetings, in a sister institution, where they all contradicted one another, and arrived at no conclusion, but made shipwreck of the whole affair. Nevertheless, we have to entertain it. Why did Mr. Wells tell us of pyemia? What is pyemia? A woman after her confinement has a pain in the upper part of the thigh, and there is a little swelling; you think there must be an abscess; you open it, and you let out a pint of matter. In a week's time, there is a similar swelling in the calf of the leg; you open that, and there is half a pint of matter; and then she does well. Another woman with septic symptoms dies suddenly with clots in the heart, and numerous abscesses in the lung. Another has not only clots in the lung, but has pus in the kidneys, a breaking down of the spleen, and concretions on the valves of the heart. Now, Cruveilhier long ago injected quicksilver into the medullary cavities of dogs, and they died with all the symptoms of septicemia; then he found numerous abscesses in the body, and in the centre of each a small globule of mercury. I mention these facts in relation to the question of pyemia. I should like to know which you call pyemia and which you do not. In the case of a woman with an abscess in the calf of her leg, you would call it pyemia; but, if you have any septicemia with it, you would call it septicemia; yet, the source of the pus is the same in every case. I am inclined to dismiss pyemia, as I would puerperal fever, entirely from the catalogue; and I believe that Mr. Callender, whom we heard the other night, has the same feeling. He seemed to me to hesitate very much as to the term pyemia.

Dr. BRAXTON HICKS: I have never said that there is distinct scarlet fever producing puerperal fever. If Dr. Savage looks at my paper, he will find it stated that it might permit a condition which we call puerperal fever to come on; and, as a clinical fact, I have no doubt of that.

Dr. SAVAGE: It seemed to me that Dr. Hicks and Dr. Barnes had an idea that there was some connexion between the two, or between some form of scarlet fever and some form of puerperal fever. I say again, it would not be real scarlet fever, because we have had testimony that you can have scarlet fever in very severe form, and the woman will do well; therefore I could not imagine that regular scarlet fever was meant.

Dr. BARNES: I also wish to disclaim the interpretation put upon my words, and I refer the Society to what I have written on the subject.

Dr. WYNN WILLIAMS: In answer to Mr. Wells' first question, I can only say that, in my experience, I have never met with a case of so-called puerperal fever existing as a disease *per se*. I have seen many cases of disease described as puerperal fever, with all the characteristics, which I have been invariably able to trace to a cause



easily recognisable, and capable, in most cases, of being recognised early enough for removal; and, as a matter of course, I do not believe that the disease is due to any special morbid poison. In answer to the second question, I am also decidedly of opinion that puerperal fever cannot be referred to any special disease, although the existing cause of puerperal disease may arise during the progress of any one of the diseases alluded to, and many others—that is, should they happen to be accompanied by any gangrenous or suppurating wound. Women are often confined with the diseases mentioned in the same room with them, and derive no injurious effects therefrom. Again, puerperal women exposed to those diseases, and who have not been previously attacked with them, will take them, as other persons, without any special action on the uterine organs. I could relate numerous instances proving both these statements, did time permit. I shall consider the third, fourth, and sixth questions together; each one is so intimately connected, practically, with the others. I have stated what I believe the disease not to be; I will now state what I believe it is. Every case of so-called puerperal fever—I do not mean peritonitis and such like—is due to septicemia, to septic contamination of the blood; and its injurious effects are first and most severely recognised in the wearied and bruised uterine organs and surrounding parts. The disease, then, is purely septicemic. I agree in the main on this head with what fell from Dr. Graily Hewitt, with the exception of his terming the disease pyemia. It is not pyemia, and I consider that the calling of it so has led to much of the erroneous doctrines that have been promulgated. For instance, a female, three or four days after her confinement, is taken with what is termed puerperal fever, and rapidly sinks in a few hours. It is said she has died of pyemia. A post-mortem examination is made, and no pus is found, only a quantity of bloody sanious serum, with a peculiarly sickening and offensive odour. This is poisonous matter. If the patient had not been prepared, as it were, by the powerful doses of septic poison in her system, and had possessed sufficient strength and vitality to form pus, she would have had a far better chance of recovery. I look on the formation of pus as a healthy effort of nature to surround and isolate the irritating poison. In fact, I do not believe that pus—laudable pus, as it used to be called, whether in or out of the circulation—ever killed any one: it is only injurious when it becomes putrid. I believe that putrid animal matter acts injuriously on the puerperal female in two ways, and that the symptoms vary both in intensity and character. In both instances the septic poison enters the system, but in a different form; and as in the one case the poison is much more concentrated than in the other, so its effects are more overpowering and rapid. In the case of most intensity, the poison enters the system in a state of solution: in the more chronic cases, it enters as vapour. In the first case there is a breach of surface, generally in the vagina; in the other there is no breach of surface. Where there is breach of surface, the septic

poison comes immediately in contact with the fresh wound, and a dose sufficient to knock the patient down at once is taken into the circulation; in the other case, a mass of putrid matter—it may be decomposing blood—is pent up in the uterus or vagina, and penetrates the mucous membrane and deeper tissues more slowly; in fact, it is a case of continuous poisoning. This form frequently terminates in puerperal mania. There is, then, no such disease as puerperal fever; it is nothing more nor less than septicemia, and is always due to the presence of putrid animal matter. Now, this putrefaction may be caused in three ways: by retention of portions of the placenta or blood-clots long enough for them to undergo decomposition; by the bruising and sloughing of vaginal mucous membrane; and, thirdly, where the septic poison is conveyed to the discharges of the patient, and by its presence sets up rapid putrefaction in the lochia—so rapid that, when once the fermentation is set up, the whole of the lochia appears to become putrid at once, like the action of yeast, or wort, or dough. The first is to be prevented by not leaving any portion of the placenta or of blood-clot (as far as practicable) in the uterus or vagina. It must not be overlooked that certain conditions of the atmosphere also tend very materially to promote this putrefactive fermentation. This has been described by Sir J. Paget, who has pointed out that at times the wounds of patients not only in hospital but outside take on an unhealthy character and become gangrenous. Of course, should any portion of placenta or blood-clot be present, your first care will be to remove them, and wash out the vagina, and, when necessary, the cavity of the uterus, with some antiseptic fluid. You must not be satisfied with telling the nurse to do it, you must see her do it, or (as I generally do) do it yourself the first time. The treatment of the second form, when you have a slough, is the same—to syringe out frequently with some antiseptic fluid. To prevent the third cause, every care must be taken to prevent any septic or putrid poison from coming into contact with the lochia. I have not time to point out how this may be best attained; neither is it necessary, as you have already heard all that can be said on the subject, as to isolation, ventilation, and the like. No method, however, has been pointed out, or even alluded to in the course of the discussion, by which the septic poison could be got rid of should it have attached itself in any way to the body or dress of the accoucheur or nurse. We have more than one substance—one in particular, iodine—that will not tolerate the presence of putrid animal matter or septic poison. Let any one who has been in attendance on any case, whether a parturient female or any other, where there is any putrid emanation, wash his hands in water into which he has poured tincture of iodine, and I will answer for it he has no septic matter under his finger-nails. Again, if he fancy that his clothes, hair, and skin are saturated with it, let him go into the water-closet (I mention that as the smallest room in the house), place a few scales of iodine on a plate and put a spirit lamp

under it, and he will soon find himself surrounded by a violet vapour, which will fall upon him in a shower of minute scales, from which he has only to protect his eyes. If he then carry with him any of the puerperal poison, my whole theory of the disease must be wrong. One thing I can assert, that in my own practice I have never had a case of fatal puerperal septicemia since I first used iodine as an antiseptic, now more than twenty years ago. Iodine is equally efficacious in warding off septicemia in other surgical diseases. I have injected solutions of septic poison under the skin of guinea-pigs, and produced death by septicemia. I have also injected some of the same solutions, into which I had dropped a few drops of the tincture of iodine, without producing any ill effects. It is needless to observe that the prevention and the cure of this disease go hand in hand together. Should any septic poison be present in the puerperal woman, wash her out again and again with solution of iodine, until the solution comes back the same colour as it was thrown up. The temperature of the patient will probably be 103 or 104, and it will go down in a very short time to 98. This I have witnessed since the present discussion began. The fifth question is as to bacteria—a delusion and a snare. It is possible that the septic poison may be conveyed by them as by any other substance floating in the air that is called harmless: that is all the harm they can do, as I take it; they live on this putrid matter as mites and maggots on cheese, or vibrios on decayed potatoes during an epidemic of potato-disease: in fact, I am inclined to think that they are present for the purpose of removing the poison, not of generating it.

Dr. PLAYFAIR: If there is one fact to be gathered from this discussion, I think it is that we have remarkably little reliable knowledge about the subject on which we are talking. It seems to me that that is a lesson really of the greatest importance; because I cannot but fancy that a great part of the almost unmistakable confusion that surrounds the whole matter has arisen from the non-recognition of that fact, from the circumstance that systematic writers upon the subject have thought it necessary to give a complete and fully rounded history of puerperal fever without recognising, as I hope they will do after this discussion, that we are only at the threshold of the inquiry, and that we have to build up all our knowledge by unprejudiced and patient clinical investigation. After saying this, I shall not err in the same way by talking too dogmatically in the few observations I have to make, and I will confine myself as much as possible to one question only which Mr. Wells has brought before us, and which has been talked about a great deal to-night—that is, the relation of the specific zymotic diseases to the so-called puerperal fever, a relation which, to my mind, is, in spite of what has been said about it, one of the most obscure subjects connected with puerperal fever. My belief about puerperal fever is very much what I understand to be that of several other speakers. I do not believe that there is any specific condition justifying the name of puerperal



fever ; nor do I believe that there is any special miasm arising from the puerperal patient capable of being conveyed to another patient ; nor do I think that there is any evidence whatever to show that there has been an epidemic of puerperal fever in the strict sense of that word, although we all know how fatally endemic it has been in our large lying-in hospitals. In the second place, I believe that the theory which considers so-called puerperal fever to be practically the same disease as surgical septicemia or pyemia, or whatever we choose to call it, is the one which is most consonant with the facts of the case ; that it arises from the contact of septic matter with lesions of continuity in the generative track, such as exist in every parturient woman ; that there are channels of diffusion through the lymphatics, or possibly the veins ; and that there are after that, just as there is in surgical pyemia, general and local results of great consequence, rendered in the puerperal patient particularly intense and virulent, on account of the particular condition existing after delivery, which was so graphically described at the first meeting by Dr. Richardson. But, sir, the moment that that theory is stated, I think a great number of difficulties and objections naturally present themselves ; and amongst these, I know none more difficult to resolve than that connected with zymotic disease. It has long been a recognised axiom amongst British Obstetricians of the highest eminence, and I think I may venture to include Dr. Farre amongst them after what has fallen from him to-day, that the puerperal patient to whom the poison of some specific zymotic disease, such as scarlet fever, is brought, may be attacked with an intense form of the disease, which does not show the specific characters of the disease that produced the contagion, and which is practically undistinguishable from the ordinary puerperal fever. I know that that view has been strongly controverted. It has been controverted by Dr. Savage, in his somewhat caustic remarks. It was controverted at the last meeting by my friend Dr. Brunton, who brought forward a great many practical facts of great value, as all the facts that he brings before us are ; but I think that Dr. Brunton fell into the common error—he argued from negative observations. I believe that no number of negative observations can outweigh even one single positive fact carefully observed. Dr. Brunton's argument reminds me of those fortunate gentlemen that I occasionally hear of from patients who tell me, “Dr. So-and-so has attended a thousand or fifteen hundred, or two thousand cases of labour, and has never lost a patient.” Now, I do not doubt the fact, and I congratulate the gentleman on his good fortune ; but I must say that that fact does not to my mind vitiate the returns of the Registrar-General, or make me disbelieve the puerperal mortality throughout the whole of Great Britain and Ireland. So I think the facts in regard to these zymotic diseases are really beyond question. There is no one of the diseases about which strong evidence could not be brought forward. With regard to erysipelas, the fact is scarcely questioned



at all. We all know how erysipelas is interchangeable in lying-in hospitals with puerperal fever; we had, as you will remember, at King's College Hospital, frequent opportunities of observing that at the time when we had a lying-in ward there. Then, with regard to scarlet fever, about which Dr. Savage has particularly spoken, I need only refer to Dr. Braxton Hicks's most valuable paper. There are, in that paper, no fewer than seventeen examples, if I remember rightly, of women who had a disease which presented no symptoms of scarlet fever, and which was clearly due to the contagium of scarlet fever. We all know that there is not, perhaps, in the room, a more careful observer than Dr. Hicks; and any man who can read that paper over without a preconceived judgment, and who does not agree with it, has a mind which I believe to be not open to conviction. Then there is diphtheria; we do not see so many cases of diphtheria, so that we cannot judge of it; but I may remind you that the most brand-new theory of puerperal fever, that of Dr. Martin, is that puerperal fever and diphtheria are the same thing. I saw, within the last few months, a case remarkably illustrative of the influence of diphtheria in producing a disease which I was unable to distinguish from puerperal septicemia. A newly married couple at Notting Hill had gone into a new house shortly before the confinement of the lady. The house was in a most unhygienic condition; an untrapped drain opening into the bedroom; an untrapped pipe from the drain and a gully under the window giving off offensive smells. The patient had an attack of intense septicemia, from which she barely recovered with her life; and the husband at the same time was laid up with a well-marked acute attack of diphtheria, the patient presenting no symptoms of diphtheria, and the husband nearly dying. Who could rationally disbelieve that those two diseases were produced by the same septic poison? I do not think there could be any doubt about it. The arguments on the other hand are unquestionably of considerable force. There is one that Dr. Savage dwelt upon; namely, that patients have these diseases in the puerperal state typically developed and running a favourable course. I am well aware of that. There are many examples of that kind on record; and that is just one of the great puzzles which I hope time will explain to us, but which we cannot explain satisfactorily now. My own idea about that is—and I merely suggest it as an idea, which further clinical investigation of the matter may prove to be correct or not—that in the one case possibly the contagious poison was brought immediately into contact with solutions of continuity in the generative track, and therefore produced an intense form of septicemia; and in the other case the poison may have been absorbed by the more ordinary channels. I think that is a sufficiently reasonable hypothesis, but one which, of course, we cannot prove. Then there is another argument, also of some consequence; that is to say, if puerperal fever be pyemia, and if the poison of zymotic diseases can produce a condition which is not to be distinguished from pyemia

under certain circumstances, should it not be the case that, after surgical operations, these zymotic poisons should act in the same way? The whole subject of surgical pyemia, especially the contagious qualities of it, has been so little worked at, that I do not think any surgeon would be justified in saying that zymotic poisons would not act in that way. I should like to know what Mr. Wells would say upon that point; and I hope that, when he comes to his reply, he will be able to spare a word or two with regard to it. But, knowing as I do the great care that Mr. Wells pays to his ovariectomy cases, I should be surprised to learn that he would be happy if he heard that some one was assisting him in ovariectomy who had been just handling either a scarlet fever or a diphtheritic case, whatever views he might form as to septicemia. I have but a minute or two to refer to that burning question, the conduct of the practitioner in cases of this kind, which is one of the most important questions connected with this subject. It is practically impossible, whatever we may think, to give up attending together zymotic disease and fever; nor, indeed, do I think that it is at all necessary. I am quite at one with what Dr. Matthews Duncan has said in the letter that has been read here: that, if proper antiseptic precautions be taken, any risk from this source might be eliminated, whether we use iodine or any other disinfectant that we like. I have no doubt that danger from this source might be obviated; but at the same time I think it quite consistent with common good sense that, instead of blindly shutting our eyes to facts, however unpalatable these facts may be—and no one considers them more unpalatable than I do—we should look them fully in the face. I believe the risk is not from the facts themselves, so much as from ignoring the facts and refusing to take the proper precautions which they would naturally suggest.

Dr. TILT: I am in the unfortunate position of a man who, having put down a few notes, finds himself forestalled by the observations of those who have spoken before him. I rise also under a feeling of some diffidence, inasmuch as I am afraid that I come within the malediction of the last speaker as a man not open to conviction. On the first night of the discussion, Dr. Braxton Hicks attributed puerperal fever to zymotic influence in three-fourths of his cases. I was most interested with his paper; it certainly is a most valuable one; but, then, I found that he put down as cases of puerperal fever due to scarlet fever all those cases that we could trace as having been brought in anyhow into connexion with scarlet fever; and whereas the paper contains twenty cases in which puerperal fever was said to have been caused by scarlet fever in which there was a rash, which we should all admit, there were fifteen cases in which there was no rash; that seems to me a very large proportion of exceptional cases, and I should say that in all probability many of those cases were cases of puerperal fever arising from other causes, certainly not from zymotic influence. If the zymotic influence is to have such an extraordinary effect in the

production of puerperal fever, scarcely a puerperal woman could escape in the densely populated parts of a metropolis like London, where zymotic influences, call them scarlet fever, measles, typhoid fever, or anything else, are crossing and recrossing themselves with a frequency with which we medical men are fully acquainted. I am not alone in opposing this, which I call an extravagant view of the power of zymotic influence in the production of puerperal fever, for Dr. Robertson of Glasgow, who is at the head of a Maternity Hospital, immediately after the discussion, wrote to say that he did not at all participate in the opinion expressed. Several gentlemen who spoke at the last meeting controverted the opinion, and several general practitioners from the country protested against it ; so that, I think, allowing full power to zymotic influence in the aggravation or production of puerperal fever, the theory was pushed a great deal too far on the first night of this discussion. With regard to the germ theory of puerperal fever, I will say just two words. I think the Pathological Society has been kind enough to settle the question for us up to the present time. With reference to bacteria as a post-mortem phenomenon, it is quite an exceptional case. While Heiberg at Christiania was relating in an interesting paper that he had found an enormous quantity of bacteria in the lymphatics of those who died during an epidemic of puerperal fever, other practitioners in Paris were making similar minute microscopic examinations in various hospitals, and found no trace of bacteria. It is one of those exceptional facts that you meet with in all puerperal epidemics ; sometimes one condition will be found and sometimes another ; sometimes pus in the veins, sometimes pus in the lymphatics, sometimes a brown sanious fluid in the lymphatics similar to what is found in the wound ; and I think, if too much importance has been attached to zymotic influence as a cause of puerperal fever, too little has been attached in this discussion to what I should call the autogenesis of puerperal fever, until it was so strongly and ably advocated by Dr. Wynn Williams. I think that, if a woman be sometimes poisoned by others, she most frequently poisons herself, and I do not see how you can find a better arrangement for poisoning a human body than you would find in the womb if once you admit that the womb contains a fetid fluid. You have a large bag, a spongy bag, partly bruised, sometimes torn ; you have lymphatics abundant, lymphatics enlarged by the gravid process ; you have lymphatics whose duty it is to absorb more speedily than at any other time ; they are bent upon absorption ; if you have a putrid fluid, I was going to say it must be absorbed, and, if it be absorbed, it goes into the lymphatics, and, if it go into the lymphatics, we must admit now that it goes into the peritoneum, because it is clearly made out that the lymphatics open into the peritoneum. Here, then, you have not only the lymphatics, but you have the peritoneum liable to be inflamed ; and, being inflamed, and inflamed early, from the fourth or the sixth day, that is what has been observed. Peritonitis has been grouped with



cellulitis and other things as *sequelæ*, but it ought to be considered as something more than that ; or, if it be a sequela at all, it is exceedingly near the beginning of the disease, and is quite sufficient to produce fever. I have shown the ichor of the fetid lochia passing from the lymphatics into the blood—blood which is in a peculiar state. Then, this poison may pass into the lymphatics without inflaming them as others do—syphilitic poisons do so. There are numerous cases in which a woman has died of puerperal fever, in whom nothing has been found in them but a sanious brown fetid fluid similar to what is found in the womb. In other cases they may be, of course, inflamed. With regard to zymotic influence, no practitioner can deny its importance, and it acts in this way. We know that all zymotic diseases—take scarlatina for instance—cause the secretions of the human body to tend towards decomposition. When a puerperal woman is submitted to the influence of scarlet fever, scarlet fever renders fetid secretions which would not otherwise be so ; so, if they be already rendered fetid by a portion of placenta, or blood-clot, or membrane of the womb, it renders them doubly fetid, it renders the poison more virulent, and that sufficiently explains the importance of the case. Now after the practical applications of the theory that Dr. Wynn Williams and myself more particularly advocate, it seems to me that, talking of sporadic cases of puerperal fever, the prevention of puerperal fever will depend to a very great extent upon the belief that a medical man may have in the autogenesis of puerperal fever. If medical men will be on the look out for fetid lochia, I believe they will find them a great deal more frequently than they do. I believe that medical men in this room will bear me out that they have not unfrequently been struck with the fact, that some accoucheurs, and, still more, some nurses, are not gifted with the sense of smell. When we are ushered into a room to consult about a puerperal case, we are often struck with the offensiveness of the atmosphere, which has not struck them. Then it strikes me that, if anything seem wrong, medical men should not trust to the testimony of the nurse, as we too often do, and as I myself have too often done ; we should examine and ascertain with our own fingers whether the lochia be fetid or not. If they be, I think we ought not to be contented with vaginal injections, but ought to resort to intra-uterine injections. I was talking, a few weeks ago, to Professor Stoltz, formerly of Strasburg, whom I met at the President's house, and he was saying that, for fourteen years, he had been in the habit of making intra-uterine injections when the lochia were putrid, and with the best results. There is concurrent testimony in favour of their value, not only amongst ourselves in this country, but in foreign countries ; and not only that : there are many instances of the sudden utility of antiseptic intra-uterine injections ; I mean the sudden abatement of the worst symptoms of puerperal fever, when the womb was washed out twice a day with disinfecting fluid. I know that there are objections made to intra-uterine injections. An interesting pamphlet, which may have been sent to you



as well as to me, has been published by an American physician in Pennsylvania, who states that these injections are unnecessary, because no fetid lochia can occur. He says, if you let the patient use the usual utensil, and let it be emptied four times a day, that will be sufficient to prevent fetid lochia. That is an interesting fact, which would require to be confirmed by our own experience. Then, there are two more important objections: one is, that the nozzle of the instrument that we should use would be itself a medium of infection. Of course, if it were carelessly used and improperly cleaned, one could understand it; but we all use powerful means of disinfection. If the nozzle were steeped in a strong solution, I cannot understand how that objection would hold ground. The assertion is contrary to our European experience. Then there is another objection: the fear that the fluid injected should pass through the Fallopian tubes into the peritoneum. I can only say that strong alcoholic solutions of carbolic acid, of tincture of iodine, have been during the past few years injected hundreds of times in the Paris hospitals, and I ascertained the other day from a friend of mine that they did not know of any case in which passage into the peritoneum had occurred.

Dr. BRAXTON HICKS: I wish to say that my paper has been misinterpreted. My paper was a communication with regard to cases of puerperal disease, not specially stating any particular point but taking all the surroundings of the patients, and, out of those eighty-nine cases that I mentioned, I found that nearly half were associated with scarlet fever; some of them undoubtedly are associated with scarlatina; erysipelas, diphtheria, mental emotions, did the same thing. I stated that erysipelas in lying-in hospitals was most common, perhaps more common than scarlatina. I left it as a clinical point not yet cleared up to explain why and how this came about. That it is associated with the production of what we are accustomed to call puerperal fever, I think there can be no doubt.

The discussion was again adjourned.

---

Specimens of Hindoo pelves and fetal heads were shown and presented to the Society by Dr. Short, of Madras.

---

*Suppurating Tumour of Left Ovary.*

Dr. J. N. J. OSWALD related the particulars of a case, where, from the rapidity of its growth, the emaciation of the patient, and lung complication, tubercular peritonitis was supposed to exist. The patient had been under Dr. Routh's care in the Samaritan Hospital, and died suddenly about a week after her return home.—Dr. ROUTH remarked that the chief point of interest was the rapidity of the growth of the tumour, which led to the supposition of its being a cancerous growth; there was amphoric breathing over the right chest.—Dr. WILLIAMS asked if any microscopic examination had been made. He had witnessed a similar case.—Dr. OSWALD replied that the microscope proved it to be tuberculous.

*Specimen of Extra-Uterine Fecundation, in which a communication existed between the Cyst and the Uterus.*

Dr. GALABIN exhibited an interesting specimen of this nature. In February, when the patient—a multipara—considered herself eight months pregnant, severe abdominal pains and hemorrhage from the vagina occurred, the fetal movements ceasing from this date. The breasts also enlarged. She was admitted into Guy's Hospital in May and attempts were made to deliver the fetus; but the patient died collapsed. At the post-mortem a greenish semi-sloughing membrane was exposed, which formed an entire cyst enclosing the fetus. There was a round opening from the cyst into the uterus.—The PRESIDENT referred the specimen to Dr. Playfair and Dr. Williams, in conjunction with Dr. Galabin, for further consideration.

---

*Intra-Mural Calcareous Tumour impeding Labour.*

Dr. A. WYNN WILLIAMS exhibited a specimen and related the history of the case. Dr. Kirby having called him in consultation to a primipara, aged forty-five, craniotomy was performed, and delivery being still impossible, cephalotripsy was about to be resorted to, when the hard mass was discovered to be movable within the uterus. The cephalotribe was applied, and a portion of the mass broken off. Delivery of the body of the fetus was then effected, and subsequently the tumour, which proved to be an intra-mural calcareous tumour, was extracted. The patient made a good recovery.

---

*Case of Extreme Dropsy—Fatty Degeneration and Friability of the Placenta.*

Dr. JOHN BRUNTON exhibited what had been an enormous placenta, but had unfortunately become much shrivelled up by being kept in spirits of wine. The patient—a primipara—was very stout; labour was premature, about the sixth month. The right hand and left foot presented. The child died immediately after birth. A very large placenta was first expelled, and subsequently three other separate portions. The patient was in an advanced state of dropsy—the urine highly albuminous. She recovered perfectly.

---

*Case of Monstrosity.*

Dr. FREDERICK WALLACE exhibited a fetus, attached to the head of which was a large fluid tumour, supposed during labour to be hydrocephalus. One eye was wanting and the other only rudimentary, and there was no tongue.

Dr. WALLACE also exhibited an enormous uterus removed (post-mortem) from a patient aged fifty-five, who had been tapped over a

dozen times for supposed ovarian tumour.—Dr. HAYES thought the case worthy of further examination.—The PRESIDENT requested Drs. Aveling, Hayes, and Wallace to examine and report upon both specimens.

---

*Placental Polypus.*

Dr. T. C. HAYES exhibited a specimen removed from a patient on the eighth day after delivery, whose labour had been natural and easy, though followed by a smart hemorrhage. Symptoms of septicemia ensued, and a portion of placenta was extracted, the lochia being horribly offensive. Injections of Condyl's fluid were employed, and the patient made an excellent recovery.

---

OBSTETRICAL SOCIETY OF DUBLIN.

*Meeting, April 10th, 1875.*

LOMBE ATTHILL, M.D., *President, in the Chair.*

Dr. MACSWINEY exhibited an ovum of about the eighth or tenth week of pregnancy, which was expelled on the previous morning, with great hemorrhage, from a lady who had had ten children. It was expelled whole, and he was given to understand, by those more experienced in these matters than himself, that an ovum being expelled whole was rather a rare occurrence. The hemorrhage was so great that he was alarmed for the safety of the lady. Upon cutting through the different layers of the ovum, dividing the decidua and chorion, and coming down to the amnion, he noticed half an inch in thickness of coagulated blood interposed between the decidual membrane and the villi of the chorion, and this was, no doubt, what was termed apoplexy of the decidual membrane. The bag of the amnion was clear, pellucid, and contained the embryo unbroken. The remains of the umbilical vesicle were plainly to be seen, occupying a space between the amnion and the chorion, and there was also to be seen proceeding from the umbilical vesicle the long duct which led into the umbilicus of the fetus.

---

*Report of a Case in Midwifery Practice.*

By S. M. MACSWINEY, M.D., M.R.I.A.

Early in March, 1874, a newly-married young lady was brought to my house to consult me. At that time she complained as follows:—She felt, she said, very weak and miserable—and, in truth, she looked so, as she was extremely pale and delicate in appearance. She told me that her appetite for food was well-nigh gone, that she was sleep-

less at night, and very nervous at all times. She referred her chief complaint, however, at this time to her urinary system. She had a very distressing pain, almost constantly, in the region of the bladder, and she had no control over the action of that viscus. Her water came away at intervals, quite suddenly, and without her knowledge or control. This occurred both by day and by night, but chiefly by day, and annoyed and depressed her very much. Its first occurrence dated from a period of about two weeks previous to her consulting me. The affection was daily becoming more troublesome. I ascertained that she had not menstruated for the last two months, and it was, therefore, clear that she might be pregnant. There was nothing, however, available in the way of evidence at this early period to enable me to determine with certainty this point; it was accordingly left for future solution. My present duty was to endeavour to alleviate the troubles which her distressing state brought her. For this, I shall merely remark, she took such remedies as experience suggested. As time passed the probability of pregnancy became greater, and I now looked forward with some confidence to the period when the fetus would have ascended from the bony pelvis as the time when, most probably, an end would be put to her vesical sufferings. These latter had not, indeed, been much benefited by the treatment employed; and they continued a source of constant uneasiness and misery until a little better than four months after her last catamenial period, when they as suddenly came to an end as they had suddenly appeared. This event was coincident in time with a marked enlargement of the abdomen, and the fact of her being pregnant was soon after fully verified. Her general health was now soon completely re-established, and nothing further worth recording occurred until her labour commenced. This event happened in November, 1874, at the full term, or very nearly so. The labour was tedious, and shortly after she entered upon the second stage, meconium, in large quantity, came away. The presentation could not, as yet, be definitely made out, but, as may be supposed, I more than suspected its nature, and, in fact, prepared the attendant nurse and the family for a "breech" case. When some further time had elapsed, and but little progress had been made in the labour, whilst her general state had become somewhat exhausted and unsatisfactory, I called for a consultation, and, at my request, Dr. Atthill saw her with me. He conclusively established the fact that the presentation was, as had been previously suspected, a "breech," and he advised that the lady should still be left to her own exertions for some hours, when, if no decided progress should then have been made, instrumental or other necessary aid might be afforded her. Some time after this consultation the pains came on pretty effectively; the breech was advancing slowly, but still advancing, nothing indicating any danger to be imminent had occurred, when, after a pain, the lady resting quietly and seeming to be dozing, I heard an incoherent sentence uttered by her, in a hurried way and altered tone of voice; this, again, was



quickly followed by a distressed gurgling noise, and immediately she was convulsed in a truly frightful manner. The scene at this moment—the lady writhing in fearful spasms, which distorted her face; the attendant woman rendered useless to assist by terror; and a powerful uterine contraction coming on whilst the convulsion was still at its height—was one not often met with, and not soon forgotten. Unhappily this fit, there was good reason for knowing, was not due to any transitory disturbance of the central organ of the excito-motor nervous system, from irritation in a distant organ such as the uterus, but was only the recurrence of an epileptic attack of which she previously had had several seizures. It was followed on the present occasion by violent maniacal disturbance, which I immediately treated by the free exhibition of chloroform by inhalation. This proceeding, I may remark, had a magical effect in allaying the delirious excitement which agitated the patient. It may be right to say that there could be no mistake as to the nature of the attack; it was not eclampsia. There was not, nor had there been at any time, albumen in the urine, neither was there dropsy in any part of the body. There was no continuity nor prolonged duration in the convulsion; a single violent but short attack came on, and there was no recurrence. And, finally, there was, as has been just stated, a history of previous similar seizures before marriage.

Desiring now to effect delivery as soon as possible, I again sent for Dr. Atthill for consultation. He thought the forceps might be tried, although he doubted if we should succeed in that way. Accordingly we attempted to effect delivery by means of them, but they slipped, and we had to abandon that mode of emptying the uterus. I next essayed to bring down the pelvis with the aid of my fingers, but without success, as it was too distant to allow me to hook a finger round a limb. In this emergency Dr. Atthill took up the band out of which the patient had been “pulling,” and which consisted of a soft cotton (what is called “stocking”) woven texture, and, after some trouble, very neatly and skilfully carried it through the fenestrum of one of the forceps blades, over the thigh, catching it at the other side by a long dressing forceps, and bringing it down and out. A complete soft fillet was thus wound round the limb. Traction being now made upon this band by Dr. Atthill, the trunk was speedily and easily delivered. But the head seemed to be caught somewhat, and its extraction gave a good deal of trouble. Dr. Atthill being rather fatigued—not having, indeed, as yet fully regained his strength after his recent serious illness—I again took charge of the case to effect complete delivery. On accomplishing this, the head, notwithstanding that every effort was made to prevent its delivery doing mischief, ruptured the perineum to a considerable extent in its passage out. Nothing further was done that evening beyond attending to the immediate and ordinary requirements of a woman recently delivered. But I examined the tear in the perineum more particularly the next day, and was a good deal concerned to find that it had quite extended

to beyond the anus. I anxiously explored the state of the sphincter, and was not a little relieved to find that it was safe and intact. The rupture had taken, fortunately, a very unusual and curved course—running, first, directly to the verge of the anus, and then, instead of tearing the sphincter, turning aside, and extending outwards and backwards, towards the angle of the ischium, fully an inch beyond the extreme limit of the opening of the bowel. It was very extensive, and had, in truth, a most unsatisfactory and unpromising look; and yet—with no other treatment save absolute rest in bed and the use of a carbolic lotion, it quickly united, and got perfectly well in a fortnight.

The lady made, in other respects, a good recovery, and both she and the baby are now quite healthy and strong. She has had no recurrence—as yet, at least—of the “fit,” as far as I know.

The case which I have now shortly related affords, I submit, a good example of several of the difficulties sometimes incidental to the state of utero-gestation and delivery, and for which the attendant has to be prepared. These difficulties might have no terrors for the experienced obstetric physician, but they would, probably, not a little embarrass the junior practitioner. To sum them up briefly, in the progress of this case there was met with—1st, the great vesical irritation and incontinence of urine, in the earlier months of pregnancy. This morbid state was, in the absence of a certainty of pregnancy, very puzzling, and very difficult to manage with satisfaction to all parties concerned. Subsequently there were encountered, at the actual confinement—2nd, the abnormal presentation; 3rd, the epileptic attack; 4th, the difficulty of effecting delivery; and 5th, the extensively ruptured perineum.

The PRESIDENT said—The case related by Dr. MacSwiney is, in several respects, interesting, specially so with reference to the occurrence of true epileptic convulsion in labour. He had several times had epileptic patients under his charge, but this was the first occasion in which he had seen an epileptic patient having a fit during labour. Epileptiform convulsions were common enough, but patients subject to epilepsy seemed to have an immunity from a seizure during labour. He believed there was no better treatment for puerperal convulsions than the administration of chloroform, and it appeared that in this case chloroform had produced a good effect. Cases of breech presentation were sometimes very troublesome. In Dr. MacSwiney's case the patient had been in labour twelve hours before he (the President) saw her; the os was not then fully dilated, and the breech was above the brim. There being no urgent symptom, he advised that time be given to see what nature would effect, but the epileptic fit coming on, Dr. MacSwiney rightly wished delivery to be effected, and suggested the use of the forceps. He (the President) had never seen the forceps successfully used in a breech presentation; but in this instance, at Dr. MacSwiney's request, he tried them, and they

were, as he expected, perfectly useless; they slipped off. Here there was a case where delivery was demanded on account of the occurrence of convulsions. The treatment of bringing down one limb was out of the question, the breech being low down in the pelvis. There were three methods by which, under the circumstances, delivery might be effected—first, by traction on the thigh with the fingers; second, by the use of the blunt hook; and, third, by the fillet. He was unable to effect delivery by traction with the fingers, and Dr. MacSwiney, who also tried it, failed. As to the blunt hook, he would never use it on a living child. It was a most dangerous instrument; he had seen it inflict serious injuries on the child, and he thought it ought not to be used unless under circumstances which rendered the birth of a living child impossible. He (the President) then decided to try the fillet; that was to pass a loop round the thigh at its flexure on the abdomen. Fortunately in this case, the pelvis was roomy, and he was enabled to pass the cotton band between the thigh and abdomen, using for the purpose one blade of the forceps, and then pulling the band down by means of a pair of long dressing forceps; the rest of the operation was easily accomplished. There was very extensive laceration of the perineum. His own treatment in these cases was to put in two or three stitches at the time, and he had obtained good results from doing so; but, as was the case in this instance, there might be a considerable amount of union without any sutures being inserted. He thought the Society was indebted to Dr. MacSwiney for bringing this interesting case before them.

Dr. J. A. BYRNE said the case was one of great practical importance, and such as any practitioner might expect to meet. He was surprised to hear of the forceps being used, not being able to see how it could be used in a breech presentation. These cases were very embarrassing where the pelvis was small, but where it was large there was generally not much difficulty. He had seen fracture of the femur produced by the use of the blunt hook, and therefore agreed with the President that it was an instrument which ought to be discarded. There were some cases in which they would not be able to give assistance by traction. In these cases Dr. Barnes recommended what he called "breaking up the wedge," or reducing the breech presentation to half a breech. He recommended that the patient should be fully chloroformed, and the hand of the operator put into the uterus, and one foot brought down. He (Dr. Byrne) had not tried the practice, but he meant to do so in the first case which came under his notice of a breech presentation which demanded interference.

The PRESIDENT observed that in Dr. MacSwiney's case the breech was wedged in the pelvis, so that it was impossible to bring down a limb.

Dr. KIDD said:—There are many points of great interest in this case. The vesical irritation Dr. MacSwiney has described is



not an unusual occurrence in early pregnancy, but the explanation of it is difficult. I have seen it in connexion with anteverted or ante-flexed uterus. When a woman is suffering from anteflexed uterus, she is likely to suffer this vesical distress for a couple of months. I have seen it where there was an irritable ulcer on the os. I have also seen it where I could not attribute it to any flexion or irritation in the os, but where it appeared to be the result of some neurotic derangement. Where vesical derangement depends on anteflexion of the uterus, it may be relieved either by the use of a pessary, such as Graily Hewitt's cradle pessary, or by placing a pledget of cotton, saturated with glycerine, in the vagina. Where it depends on an irritable ulcer of the uterus, we must treat the ulcer, cauterize it, and destroy this great irritability of the surface; but, in many cases, it resists all treatment, and nothing will overcome this vesical irritation but time. Sedative suppositories placed in the rectum sometimes afford great relief; but all treatment often fails. The next point is the breech presentation. I do not know that there is any connexion between that and the vesical irritation. The epilepsy is unusual. It is very unusual to see epileptic fits occurring during labour. It is difficult to distinguish between epilepsy and epileptiform convulsions in a puerperal woman. We very rarely meet with epileptic fits in labour in epileptic patients; and it is almost laid down as an axiom in many of our standard works that it does not occur. If we were quite clear that it was epilepsy, there might not be the urgent need for emptying the uterus. If we have convulsions coming on in labour, the best practice is to empty the uterus as soon as we can; but, if it were a true epileptic fit, I do not see that we should be so urgently called on to empty the uterus. I do not know, however, how we can draw the distinction between the epileptic fit and the epileptiform seizure. Dr. MacSwiney alludes to the absence of albumen in the urine; but Dr. More Madden has brought forward several cases where epileptiform fits had occurred without any albumen being found in the urine. Braxton Hicks, also, brought forward cases where there were numerous epileptiform convulsions and no albumen, and he went so far as to throw out the suggestion that albumen was the result of the convulsion, instead of being the cause of it. I made it a point, for some time, at the Combe Hospital, to examine the urine of convulsive patients, and I have the record of many cases where we had true puerperal convulsions without any albumen being found in the urine. The next point is as to the difficulty of extraction. When the breech is high in the pelvis, and the uterus not much contracted, the best practice is that of Barnes. If the breech can be pushed up before the hand, which can be very often done if the waters have not been fully expelled, then the best course is to break up the wedge or presentation by bringing down one leg. This I have practised myself with success, on Dr. Barnes' recommendation. It not only lessens the bulk of the presenting part, but gives you full control over the child.



I have never used the fillet, but I have used Churchill's forceps very beneficially when the breech was very low down, but the cases in which this is necessary are very few indeed. I quite concur with the President in his observations as to the blunt book. I have seen injury done by it, and I have never used it on a living child; but it is not altogether to be discarded in the case of a dead child. With regard to the laceration of the perineum, the best practice we can adopt is to bring the edges together at once; unless the patient be in a very unhealthy condition, it rarely fails to cause union. I have seen one case where it was a complete failure, but then the woman was in a dying state at the time, and did die very soon. Before death the stitches gave way, and the parts opened widely; but, as a rule, if the perineum be torn to the verge of the anus, if you bring the edges carefully together with sutures, you will obtain good results. You must be careful to insert your sutures deep enough, and I think that the material that is best for this purpose is either thread, hemp, or carbolized cat-gut. In my first attempt to bring together the lacerated perineum, I used wire sutures, but every time the nurse applied the sponge it caught in the points of the wires and caused irritation; and, therefore, I have lately used ordinary housewife thread. I also at first used the quill suture, but have not done so latterly. I find the simple interrupted suture will answer as well, and not give so much pain, and is made more quickly. In using the interrupted suture you must be careful to insert your sutures deeply, so as to bring together the inner margins of laceration as well as the cutaneous margins. The first suture should be inserted as close to the anus as possible, so as to control the action of the sphincter; the second at the middle of the laceration, and the third at its vaginal extremity; and with the exception of the one case I have alluded to, I have never seen this fail in producing adhesion.

Dr. MACSWINEY briefly replied. He said he had given chloroform in this case for the purpose of calming the frightful excitement present, and also of preventing the recurrence of the attack. When they were endeavouring to effect delivery, he administered chloroform a second time, the lady having again become greatly excited, and she was under chloroform when the delivery was effected. The vesical symptoms indicated want of control and not incontinence of urine. She had a distressing pain in the region of the bladder. The fit was an epileptic seizure; she was subject to epilepsy pure and simple. There was, in his mind, a marked distinction between an epileptic fit and a puerperal convulsion. In addition to the absence of premonitory symptoms indicating eclampsia, the suddenness of the fit and its non-continuance would in itself clearly indicate that it was not a puerperal convulsion. It was not usual that one puerperal convulsion should occur and no more. His opinion in this case was that the patient ought to be delivered, and then it was that he urged the use of the forceps. It was agreed that delivery could not be effected

by drawing down a limb, and he could not see any objection to attempt to apply the forceps. With regard to the rupture of the perineum, it did not occur to him to examine it minutely at the time. On the next morning, when he did make an examination, he found it had progressed so favourably by non-interference that he determined to let it alone; and he could assure the Society that no operative proceeding could have been more successful, for the result of the non-interference had been the most absolute union that could be desired.

---

*Case of Ovarian Tumour (Unilocular Cyst); Ovariectomy performed.*

By HENRY GRAY CROLY, F.R.C.S.I.

I bring this case forward, sir, because, although I have not much to say on the subject, I think, as we are collecting statistics of ovariectomy, that fatal cases should be recorded as well as those that are successful. It is clear that we cannot arrive at any sound conclusion on the subject if pet cases only are recorded, and those which have not proved successful excluded from our consideration. It is for that reason I desire to lay before the Society the details of a case of ovarian tumour recently under my care, in which the operation of ovariectomy was followed, unexpectedly, by a fatal result. The following history of the case I take from the notes of my former apprentice, Dr. Hearn, L.R.C.S.I. :—

J. O'K., aged fifty, residing at 61, Watling-street, was never married; always enjoyed good health up to about seven months ago (except a slight pain in her left side, about fifteen years ago, which left her in a very short time). Menstruation commenced when she was fourteen years old, and ceased three years ago; was always quite regular, both as to time and quantity, the catamenia lasting four days, and unaccompanied by any pain. She was admitted to the hospital on the 3rd Nov., 1874, and stated that about seven months previously she felt, for the first time, a darting pain in her left side, accompanied by a swelling, which has been gradually increasing up to the present. Three weeks before admission she was suddenly seized with violent pain in the left side, of the same darting character, which extended over the front of the abdomen to the right side. Since then she has had a recurrence of pains extending over the abdomen, coming on generally during sleep.

On examination a large tumour was found, occupying the entire front of the abdomen, dull on percussion in front, and clear at the sides towards the lumbar region; fluctuating, and with numerous varicose veins ramifying on its surface.

28th.—Was examined, per vaginam, by Drs. Kidd and Atthill, when a small polypus was discovered protruding from the os uteri, which Dr. Atthill removed. The uterus was normal in length and position. The patient was also seen by Drs. Churchill, Denham,

and many other practitioners in this city. The following were the measurements:—At umbilicus, 35 inches; from umbilicus to ensiform cartilage, 6 inches; from umbilicus to pubis, 8 inches; from ensiform cartilage to pubis, 14 inches; from right anterior superior spinous process to umbilicus, 9 inches; from left anterior superior spinous process to umbilicus, 9 inches. The measurements were taken a month or six weeks afterwards, and showed an increase. They were as follow:—At umbilicus,  $35\frac{1}{8}$  inches; from umbilicus to ensiform cartilage,  $6\frac{3}{4}$  inches; from umbilicus to pubis, 8 inches; from ensiform cartilage to pubis,  $15\frac{1}{8}$  inches; from right anterior superior spinous process to umbilicus,  $9\frac{1}{2}$  inches; from left anterior superior spinous process to umbilicus,  $9\frac{1}{2}$  inches.

The patient, on admission to hospital, was carefully examined as to her general health. This examination was made partly by myself, and partly by my colleagues, and we decided that she had no organic disease to contra-indicate operative interference. She had not a strongly acting heart, which was probably the only weak point. It was decided that she should get the chance of the removal of the tumour, which was causing her much distress, and preventing her from taking any part in household duties. Accordingly, on the 2nd of February, I performed the operation, in the presence of a large assembly of practitioners, in a ward of the hospital which had been carefully prepared for the patient. The incision, which was four inches long, was commenced midway between the umbilicus and the pubis in the median line, and extended down to within an inch of the pubis. *My hand was not at any time introduced into the cavity of the abdomen.* I passed Sir Henry Thompson's sound beneath the abdominal parietes, and broke down a few delicate anterior adhesions. I punctured the cyst, and drew off half a bucketful of fluid. The cyst was believed to be single, but at the time of its removal it presented an appearance which, at first sight, made it look like a double cyst—in reality it was unilocular. There was scarcely a drop of blood lost. I was obliged to stop once during the progress of the operation, owing to sickness coming on from ether which the patient inhaled. The tumour having been removed, the pedicle was secured with a clamp, the actual cautery applied, and the wound was closed with carbolic sutures passed from within, and the patient was dressed in the usual way with flannel and wadding.

The operation was thus performed on Saturday morning, the 2nd of February, between ten and half-past ten o'clock. For the first eight or ten hours the patient seemed to suffer so little from the operation, that one looking at her could scarcely think that any operation had been performed. As I said to several practitioners who inquired about the patient, "There is only one fault to find with her, and that is, that she is too well;" for my experience is, that if patients are apparently very well immediately after operation, they do not do as well ultimately as those who suffer more severely from shock in the first instance. Opium was administered in moderate doses. She



took ice and chicken-broth, and went on well until Saturday night, having been carefully watched by Dr. Hearn, the resident pupil, and the dressers. The urine was drawn off regularly. On Sunday the patient was apparently going on well. The only change to be observed was, that the pulse was quicker—a symptom that I should have liked to have occurred a little earlier. On Sunday I was anxious about her, but she had no abdominal tenderness, no jerk in the pulse.

On Monday morning, at three o'clock, I was sent for to see the patient, and found her with a very quick pulse, and râles through the chest. I dry-cupped her on the back of the lungs; there was no pulmonary congestion; her temperature was so good that I had not to employ hot jars; there was a slight distension of the abdomen from flatus. I passed up O'Beirne's long tube, and drew off the flatus without otherwise interfering with the patient. I further observed that her pulse was getting very rapid, and her breathing quick. She gradually sank from that time, the pulse having become so rapid that we could scarcely count it, and at three o'clock P.M. on Monday she died (her intellect being perfectly clear), just sixty-five hours after the operation. A post-mortem examination was made, but there was nothing found to explain the death. There was no hemorrhage into the abdomen; there was only a trace of peritonitis, there being only as much injection of the vessels as would show that there had been an operation, but no effusion of lymph. The heart and lungs were sound. The post-mortem examination proved the accuracy of the diagnosis—namely, that the disease was in the right ovary, although the patient said the tumour commenced at the *left* side. There is one other point I should have mentioned. The *bruit de cuir neuf* was marked at the right side over the region of the ovary, and that was the only place where adhesions were found to exist at the time of the operation. In the medical certificate of death the cause registered was, "secondary shock following ovariectomy, sixty-five hours." I have performed some hazardous operations from time to time, but I never undertook one with such a deep feeling of responsibility as I entertained on this occasion. The patient was fully impressed with the serious nature of the operation, but she wished to get a chance of her life. I was greatly disappointed at the result, for when I cut down on the tumour, and the cyst turned out with such ease, I thought it was a case likely to do well.

The PRESIDENT complimented Mr. Croly on his candour and courage in bringing forward an unfavourable case of ovariectomy. In common with some other practitioners, his opinion had been asked as to the advisability of operating in this case, and he was present at the operation. In his opinion there could not have been a more justifiable case for operation. The patient would have died before very long if the tumour had not been removed. It was only another example of the uncertainty of the results, for he had seen the operation performed where everything seemed unfavourable, and yet



the patients recovered. In this instance the operation was performed with the greatest possible skill and care. No doubt ovariectomy was an operation that ought not to be undertaken without a full appreciation of the responsibility incurred, as it was one that could only terminate in either of two ways—perfect recovery or death. In the present instance that had been done, and the patient had been put in full possession of the dangers of operation, and yet wished it to be performed.

Dr. KIDD.—Along with you, sir, I had an opportunity of seeing this case, and I was present at the operation; and, with Mr. Croly, I was greatly disappointed at the result. The operation was most skilfully performed, and the tumour came away without any difficulty. The hand was not introduced into the cavity of the peritoneum—a matter, I think, of considerable importance. Mr. Croly, on several occasions, asked me about the case. I believe the opinion I uniformly expressed was—that it was a case in which, considering the rapidity of the growth of the tumour, and the continuance of that growth, death was inevitable if the operation were not performed. There are a few cases of ovarian disease where the tumour does not grow rapidly, and in which the life of the patient may last for a very considerable time, she being only inconvenienced by the bulk of the tumour. In a case of that kind my own feeling is that we should not recommend an operation; but here was a case where the tumour had attained a very considerable size in a few months, in which the woman's health was sinking under it, and where all our experience indicated that in a very few months the case would terminate fatally if the tumour were not removed—so that, in my opinion, the operation was one that was perfectly justifiable. At the same time I felt the woman was a bad subject for an operation, and that was Mr. Croly's opinion too. Though there was no disease of the chest or kidneys, yet her aspect and condition were such as made the case not a desirable one for operation, and the opinion I expressed, and which I believe was identical with Mr. Croly's, was—that if the woman, having all the circumstances laid before her (which was done), elected to have the operation performed, it was right to do it. It is a very remarkable fact that cases in which the tumour comes out most easily are not the most successful ones. I have seen a case where the tumour came out as easily as a glove comes off the hand, and in which the patient began to die on the table. I remember being present at an operation performed by one of our leading surgeons in which that was the case. There was no adhesion, no solid matter. The incision was not more than two inches in length. The cyst was tapped, the fluid drawn off, and the tumour turned out with ease, and yet the patient never rallied, and she died in a short time afterwards. The first case of ovariectomy in this country died somewhat in this way. The woman was a patient of mine, on whom Dr. Clay, of Manchester, operated. She seemed to do well at first, but at the end of twenty-four hours she began to sink, and died, apparently, simply

from secondary shock. That is, I believe, not an uncommon form of death in these cases.

Dr. DARBY thought too much stress could not be laid on the points that should guide them in diagnosis. Some individuals having ovarian tumours might live for years, and it was a serious thing to undertake an operation in cases that might live for a long time without interference, but might die in a few hours after operative proceedings. He had three cases under his observation at present, where he knew ovarian tumours to have existed for upwards of twenty years, and the individuals were in perfectly good health at present, and likely to live for years to come. One was a patient in hospital, admitted twenty-five years ago. She went on for years with an enormous swelling in the abdomen, evidently an ovarian tumour. When Spencer Wells came to Dublin, he (Dr. Darby) was urged to interfere in this case by operation, and he asked his friend, Dr. Kidd, to examine the case. Dr. Kidd did so, and advised him not to operate, thinking that there was solid matter in the tumour. He acted on the advice, and the woman was not only in perfect health, but she was getting quite small, and one would hardly know she had a tumour at all. Another case was that of a lady, wife of a medical man, who was very bad some years ago. He was consulted about her, and thinking, at first, that it was a disease of the womb itself, he advised the husband not to meddle with it, and she was now in good health and getting quite small. There was another case of the same kind which he had asked Dr. Beatty to see, and he advised that there should not be any operative proceedings, and the woman was now getting small. At the time Dr. Beatty saw her she could not move from one room to another, but during the last twelve months he (Dr. Darby) saw her after she had driven twenty miles in a gig, and she was not in the least injured or fatigued by the exercise. With regard to rapidity of growth, the last-mentioned case was a remarkable example. The woman grew to an enormous size within twelve months. He had performed the operation of ovariectomy only once, and it was unsuccessful. It did not appear to him at the time that there was any novelty in the case to justify him in bringing it under the notice of the Society, but he regretted now that he had not done so. The patient was a small woman, but was upwards of sixty inches in girth. Dr. Beatty and Dr. Robert M'Donnell saw her, and, in talking over the matter, it was decided that if the adhesions were extensive they should be content with tapping. However, when the fluid contents, filling two washing tubs, were evacuated, the adhesions were found to be very extensive. Dr. Beatty was in favour of proceeding with the operation, and, yielding to his experienced judgment, he proceeded to break down the adhesions and remove the sac. The woman sank in forty-eight hours, and evidently died from the effects of the shock.

Dr. J. A. BYRNE said that some members might remember a woman suffering under ovarian disease, who, some years ago, used to

go about the town hawking articles for sale. She had a very large ovarian tumour, and at intervals it burst spontaneously into the vagina, when large quantities of fluid would be discharged, and the abdomen would become small, and then it would re-fill. Three cases of spontaneous reduction of the tumour were mentioned by Dr. Darby. He never knew any case in which it had occurred except the one he had just mentioned. In that case the abdomen would grow very large, and suddenly a discharge would take place through some opening in the vagina. He could not find where it came from, but probably there was some fistulous opening.

The PRESIDENT said, with reference to the escape of fluid per vaginam, that a case had come under his notice of ascites from cardiac disease. The patient was apparently dying, and was of enormous size, having anasarca of the limbs, as well as dropsy of the abdomen. On visiting her one day, he found her sitting up in bed, the swelling having entirely disappeared. She stated that on the previous day she had been seized with severe pains of an intermittent character, very like labour pains, that a large quantity of fluid was after a time discharged per vaginam, and that immediate relief followed; the fluid, however, continued to trickle for some days. After a time the fluid accumulated again, and the same phenomena occurred a second time, she being then a patient in the Meath Hospital. It occurred, also, a third time, and then she died; and, on a post-mortem examination, not the slightest opening into the vagina could be discovered. He thought the fluid must have escaped through the Fallopian tubes.

Dr. KIDD.—As to the diminution of ovarian tumours, it is a matter of rare occurrence, and it would be very interesting if Dr. Darby would place on record those three cases to which he has referred. One of them he allowed me to see, and as far as my recollection serves me it was a fibro-cyst of the uterus. Now it is common for a fibro-cyst of the uterus to diminish in size as the patients advance in life; but I am not aware that it has been observed that ovarian tumours diminish. I remember some years ago, the physician of the Morningside Asylum of Edinburgh, placed on record a case which underwent remarkable changes in size, and without any apparent reason, but that is almost the only case of the kind I remember. I myself published, in the *Dublin Quarterly Journal*, an abstract of that paper, and drew attention to the case as one of very unusual occurrence.

---

*Meeting, May 8th, 1875.*

FLEETWOOD CHURCHILL, M.D., *in the Chair.*

Dr. M'CLINTOCK laid before the meeting the following old obstetrical works, making numerous quotations, and offering some analytical and critical remarks upon each as it came under notice:—



1. "Eucharius Rhodion," *De Partu Hominis*. Frankfort: A.D. 1563.

The first edition of this work was published in High Dutch about 1519; was translated into Latin 1532, and subsequently into German, French, and English. The style of this work is remarkable for conciseness and method. It is illustrated with some very rudely executed woodcuts, and is provided with a very good alphabetical index, all which tended to make the book a justly popular one, as we know it to have been.

2. "The Byrthe of Mankinde, otherwise named The Woman's Booke," set forth in English by Thomas Raynalde, Physitian. London: 1565.

This professes to be a translation of Rhodion's work, but is considerably enlarged. The first edition appeared about the year 1540, and was followed by many other editions; and for nearly one hundred years, according to Denman, it was the popular treatise on midwifery in England. Dr. M'Clintock has in his possession a copy of this work, printed in 1606, but it is word for word the same as that of the year 1565. Both are printed in black letter.

3. "The Expert Midwife." London: 1637.

This, as stated on title-page, is a translation of the treatise of James Rueff, a surgeon of Zurich, "*De Conceptu et Generatione Hominis*," the first edition of which appeared in 1554, and at Frankfort in 1587.

It is printed in Roman character, and the name of the translator or editor is not given. It contains some woodcuts, closely resembling, but not quite so rude as those in Rhodion's and Raynalde's works. The most interesting point connected with this work is the description it contains of pudendal hematocoele, occurring at the time of labour, which is (as Velpeau asserts) undoubtedly the earliest recognition of this accident by any obstetrical author that we know of.

4. "The Childbearer's Cabinet."

This very brief little book, written in a popular style, the authorship of which is unknown, was one of four treatises that were published in a work entitled "*A Rich Closet of Physical Secrets*," published at London in 1653.

It contains nothing but practical directions for the management of pregnancy and child-birth, of a very short and meagre kind, and the only novel point to be found is the recommendation of a binder to the abdomen immediately after delivery.

5. "Chamberlen's Translation of Mauriceau's Treatise on the Diseases of Women with Child and in Child-bed." London: 1672.

This translation, by Dr. Hugh Chamberlen, went through many editions—seven at least—and the copy before the Society is one of the first edition, which is extremely rare; in fact, Dr. Robert Lee never saw one, and has expressed a doubt whether there be such extant. The translator of this work was the same Dr. Chamberlen



who is mentioned by Mauriceau, in his twenty-sixth case, as having come to Paris and vainly tried to deliver a woman with distorted pelvis by some secret means.

From this work Dr. M'Clintock passed to the consideration of the discovery of the midwifery forceps, and the respective claims of Dr. Paul and Dr. Peter Chamberlen to be the inventor of this, the most valuable instrument in the whole range of surgery. He assigned various cogent reasons for coinciding in the conclusion which Dr. Aveling has arrived at—viz., that the honour of the discovery belongs to Dr. Peter Chamberlen (the father of Dr. Hugh Chamberlen, who translated Mauriceau), and not to his son, Dr. Paul Chamberlen (as stated by Ramsbotham, Churchill, Leishman, and many other writers of the highest character). Dr. M'Clintock exhibited models of the original Chamberlen forceps, now in the possession of the Royal Medico-Chirurgical Society of London, and also showed a print (belonging to Dr. Churchill), with the legend underneath, "Dr. Paul Chamberlen, 1658," which print, Dr. M'Clintock thought, had probably given rise to the idea that he (Dr. Paul Chamberlen) was the original inventor of the instrument. But, for reasons which have been very fully and plainly stated by Dr. Aveling, he (Dr. M'Clintock) entirely rejected this evidence, and regarded the print as being that of Dr. Peter Chamberlen, the father of Hugh and Paul, and who, in the year 1658 (that on the picture), was fifty-seven years old; and of such age the original of the picture appears to have been. *Apropos* to this subject, Dr. M'Clintock exhibited a genealogy (in MS.) of the Chamberlen family, which had been presented to him by Dr. Aveling, who had exercised a great deal of patient research in making out these interesting particulars.

## *Obstetric Summary.*

### *A Case of Extra-uterine Pregnancy—Successful Operation.*

By Dr. G. DRESSELHUYNS.\*

Mrs. Van H., married, aged forty, was in her seventh pregnancy when my friend Brinkerink, surgeon-apothecary and accoucheur at Beekbergen, was called to her. Although her first two children had been born alive and still lived, her third, fourth, fifth, and sixth labours were terminated by turning and exhaustion. Of the third and fourth labours, nothing more is known to me; the fifth I myself completed, and during the process recognised some shortening of the conjugate diameter; the sixth was terminated by artificial means by Herr Brinkerink. After the fifth confinement, an abscess formed in

\* From the "Weekblad van het Nederlandsch Tijdschrift voor Geneeskunde," No. 21, 1875.

the abdominal wall ; it healed without treatment, and, from what was told me, it may have been an anthrax. Herr Brinkerink was called to her on January 27th, 1873, although she expected to be confined in March. She had pains in the abdomen, which was reddened but flabby ; on the left side Herr Brinkerink felt a mass, which he thought to be the induration remaining after the former suppurative swelling. He informed me that the os uteri felt as in the unimpregnated state. The pains ceased, and the woman resumed her ordinary occupations.

In March, Herr Brinkerink was again called to her, and then found an abscess to the right of the umbilicus. On 2nd April, he received a letter from the patient's husband, stating that the abscess had burst, and that something had escaped, which certainly no one had ever seen.

Our friend Vlaanderen, formerly sanitary officer at Apeldoorn, being in the neighbourhood of Herr Brinkerink, was invited to see the case with him. Herr Vlaanderen thought that there was an extra-uterine pregnancy. Both gentlemen in consequence invited me to meet them the next day at the patient's house, to examine the case and bring it to an end.

On April 4th we all met. I found the poor woman lying on a bed in her room, which was scarcely large enough to hold it, in a hut on the heath. She appeared to be much emaciated, was hectic, and extremely weak ; the pulse was frequent and small. It seemed as if her sufferings must speedily end in death. To the right of the umbilicus was a sore, with an ichorous discharge, having a sickly odour ; the base was hard, and covered with hair. A portion of the abdominal wall formed a bridge over the sore. At first sight, we were involuntarily led to think it a hairy cyst ; but to make the diagnosis sure, I introduced my finger into the sore to ascertain if I could penetrate into the abdominal cavity at the brink of the hairy base. My finger passed immediately into the abdomen, and the supposed base presented the feeling of the hard and bony surface of a child's head, of which I could also feel one ear. Herren Vlaanderen and Brinkerink each made an examination, and confirmed my observation.

On vaginal examination, the os uteri could be felt lying high, above and behind the symphysis pubis. By means of the sound, I examined the position, direction, and capacity of the uterus, and by the injection of water I endeavoured to obtain more accurate information as to the extent of the uterine cavity, and to ascertain whether there was a communication between it and that in which the child lay. A very small quantity of fluid only could be injected with difficulty, and we found no communication between the two cavities.

In view of the hectic condition of the woman and her extreme weakness, and of the evident endeavours of nature to get rid of the dead child, it was not difficult to decide on the course which should be followed. Division of the bridge of integument would perhaps

enlarge the opening sufficiently to allow the escape of the child. I accordingly did this, and extracted the child through the opening. When the cavity was emptied of the child, it was sponged out and cleaned ; and we found that it was about 15 centimetres in diameter, and was lined with a smooth, pale red membrane. To the left (near the umbilicus) the wall of the sac presented a portion of the size of a teaspoon, thicker and firmer than the remainder, and having an umbilical cord proceeding from its centre. This is considered to represent the placenta. When we had satisfied our curiosity, we closed the wound with thread sutures, leaving at the lowest part an opening for the escape of fluid, the separation of the funiculus, and the administration of any injections that might be necessary. The wound healed rapidly to a small portion ; the funiculus disappeared on the fourth day ; the walls of the sac contracted, and it became smaller daily. There was no appearance of throwing off of the placenta.

The patient's general health improved under the use of a nutritious diet, Peruvian bark, and mineral acids. Diluted solution of chloride of lime was daily injected into the sac.

The woman is now (1875) quite well ; but there is still an opening of the size of a pin's head, from which a few drops of blood escape at each catamenial period. This escape of blood from the opening at each catamenial period evidently shows the connexion with the genital organs, and the continuity between the wall of the sac in which the child lay, and the uterus, ovary, or Fallopian tube ; that, however, there was no ovarian, tubal, or interstitial uterine pregnancy, is evident from the absence of laceration and internal hemorrhage.

Busch says, "Rupture takes place generally between the sixth and sixteenth weeks ; but ovarian pregnancy may reach a more advanced stage." Rupture is also preceded by syncope and convulsions, which were altogether absent in the present case. It seems to me that, through the preceding difficult labours and other causes, inflammation and narrowing of the Fallopian tube or tubes took place, so that in the seventh pregnancy the ovum remained outside the uterus. Perhaps the *fimbriae* aided in piercing the sac in which the child was contained, and thereby the connexion was maintained.

That we made no attempt to remove the placenta will be regarded as a right proceeding by any one who reflects that the sac containing the child did not present contractions like the uterus, and that therefore the removal might have been followed by fatal hemorrhage in consequence of the vessels remaining open. Besides this, the placenta was clearly a portion of the wall of the sac, and so intimately connected with it that peeling it off was not to be thought of.

That Herr Brinkerink did not feel any part of the child on January 27th, is not much to be wondered at, when it is remembered that Chevillon failed to detect the presence of a fetus in a case of extra-



uterine pregnancy, in which delivery subsequently took place *per anum*. An earlier recognition of the condition of our patient would perhaps have led to no more fortunate result. W. Cambell has shown by statistics that leparotomy, without the endeavours of nature to free itself, yields less favourable results than so-called secondary operations—that is, artificial aid employed when nature is already making attempts to throw off the child.

This case is related in honour of the powers of nature, not as a brilliant instance of our treatment.

## Gynecic Summary.

### *Physiological Changes in the Uterine Mucous Membrane.*

By JAMES BARNSFATHER, M.D., Cincinnati.

I have from month to month examined microscopically not only the menstrual discharges from healthy women, but also those from females suffering from uterine difficulty. For some time past my mind has been gradually turning towards the fact that the knowledge we receive from gynecological works is very defective, certainly the theories now promulgated are not in accordance with my observations.

The statements I am now about to make are founded entirely upon microscopical examination, as I have not been in a position to get fresh post-mortem material to work on, but will try to get it for future investigation. First, I find in all menstrual discharges I have examined, exfoliations from the mucous membrane, even from females perfectly healthy; but from those suffering from dysmenorrhea, I find the mucous membrane in a state of hypertrophy and extravasation, and evidently also in a degenerated condition; it also passes in larger pieces than it would from a healthy uterus. I also find quantities of round cells and blood-corpuscles. From observation, month after month, and finding the same products reproduced regularly, I am led to the conclusion, that at every menstrual period the uterus, by a peculiar physiological action, throws off the old mucous membrane and forms a new one, to be again thrown off at the next catamenial epoch.

If this be so, I think it is a wise provision of nature to give to the descending impregnated ovum a young and healthy mucous membrane, that is capable of performing the natural functions so essential to its existence and further development. Again I find that those women who have a scanty menstruation, generally suffer from a profuse uterine catarrhal discharge immediately after the catamenia, continuing for a few days and then ceasing. I also find this fluid highly charged with débris of the mucous membrane. This is evidently the effort of nature to get rid of the old and useless membrane, so that



the parts may be cleansed and ready for formation of the new and healthy membrane which can alone perform the functions for the reproduction of the species.—*Cincinnati Medical News*.

---

*On the Use of Salicylic Acid.*

Professor Crede gives a short note (*Archiv für Gynäk.* Bd. vii. Heft 3) on the use of salicylic acid in gynecological practice. He has employed it for the last six months, on the recommendation of Prof. Kolbe, instead of carbolic acid, as a disinfectant for the hands, as a vaginal injection in puerperal women, and for sprinkling over puerperal ulcers, &c. The strength of the solution is from 1 in 300 to 1 in 900, or as a powder mixed with starch, 1 in 5, or it may be used as salicylic-acid wool. Most favourable results have followed its employment, and Prof. Crede desires strongly to recommend its use in midwifery practice.

---

*Vaginismus : its Causes, Nature, and Treatment.*

Dr. Lutaud, in *Thèse de Paris*, No. 405, concludes that vaginismus is always symptomatic. It is a common affection which manifests itself chiefly after the first sexual congress. Vaginismus is often allied to dysmenorrhea, and to the troubles of general innervation. It is always curable, the cure being easier if the malady is recent. By the obstacle which it offers to the accomplishment of sexual functions, vaginismus is a frequent cause of sterility. The treatment of vaginismus is simple : cauterization and dilatation are the principal means to overcome it.

---

BOOKS, PAMPHLETS, AND PAPERS RECEIVED.

"Cyclopædia of the Practice of Medicine," By Dr. H. von Ziemssen. Vol. III. "Chronic Infectious Diseases." London: Sampson Low and Co. 1875. Pp. 672.

Communications have been received from Mr. Spencer Wells, C. J. Cullingworth, Esq., Dr. Henry, Dr. Edis, Dr. Carter, Dr. F. H. Grosholz, Dr. John Brunton, Messrs. Kröhne and Sesemann, and Dr. Pauly, of Zduny, whose paper we shall be glad to receive.

---

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.

THE  
OBSTETRICAL JOURNAL

OF  
GREAT BRITAIN AND IRELAND.

---

No. XXIX.—AUGUST, 1875.

---

AN ADDRESS ON OBSTETRIC MEDICINE AND  
ITS POSITION IN MEDICAL EDUCATION.\*

By ROBERT BARNES, M.D., F.R.C.P.  
Obstetric Physician to St. George's Hospital, &c.

THE honourable office to which your kindness has called me involves a duty which, I fear, I shall very inadequately discharge. It is no light task worthily to address so distinguished a body as the Metropolitan Counties Branch of the British Medical Association. It is not, indeed, difficult to find a theme deserving your attention ; for that is dictated by the fact of your having chosen me as your President. I understand that your choice has fallen upon me, because you wished to recognise the claim that Obstetric Medicine has to a share in the representation and in the work of our great Association. I am grateful for that recognition. It is an omen, I trust, of fuller and more equal participation in the honours and in the government of our profession.

The actual position of obstetrics in relation to medicine and surgery is a scandal to the profession, and a grievous injury to the public. I offer no apology for asking, In what respect is obstetric medicine intrinsically less worthy than so-called pure medicine and pure surgery? Is not its object

---

\* Presidential Address delivered at the Annual Meeting of the Metropolitan Counties Branch, June 28th, 1875.

at least as important to mankind ? Does its exercise demand less scientific knowledge, less trained skill, less devotion to duty ? Are those who teach and who practise it inferior in education, in attainments, and in capacity to those who teach and practise medicine and surgery ? Flattery was never my forte or weakness ; and I should be guilty of flattery most gross, were I to affirm that all the physicians and surgeons of our hospitals were men superior in merit or in usefulness to the teachers and practitioners of obstetrics. Many, I presume, would not themselves lay that flattering unction to their souls. In the name of justice, then, how can they reconcile it to their conscience to usurp almost exclusive possession of the clinical opportunities, honours, and emoluments of the hospitals, and the government, honours, and other benefices of the colleges ? In the name of common sense and in the public interest, why is it that obstetric medicine is all but absolutely unrepresented in the General Medical Council, in the Senate of the University of London, in the College of Surgeons ?

Is the answer to be, that medicine and surgery confer upon their votaries the miraculous gift of capacity to legislate for that department of our profession of which they are practically, if not also theoretically, ignorant ? It can hardly be contended that medicine and surgery, in the special sense in which they are cultivated by our hospital physicians and surgeons, include a knowledge of obstetrics and gynecology sufficient for the purposes of government. If there be one department of the healing art which can show a better founded pretension to universality than another, it is surely obstetrics, to understand and to practise which we must combine a liberal knowledge of medicine and surgery.

The best argument in favour of the actual state of things that occurs to me, is the great advance that obstetric medicine has made in spite of every discouragement and the studied indignity to which it has been subjected. But success so achieved is not evidence of the wisdom of those who inflict that discouragement and indignity. It is evidence of the inherent scientific and practical importance of the subject that can

enlist able men in its service, and find fitting material for the exercise of their faculties.

It is worth while to take a rapid survey of the actual position occupied by obstetrics and gynecology in relation to medicine and surgery in our London hospitals, schools, and governing bodies. At this moment the eleven hospitals to which schools are attached have each one obstetric physician, and not all have an assistant obstetric physician. One of the largest has but just been roused to a perception of the fact that a second officer was wanted. Now, in all these hospitals, I need not say, there is a large staff of physicians and surgeons and assistants in training and in full occupation. And not only is there an enormous preponderance of medical and surgical *personnel*, but the distribution of material is so arranged that each physician and each surgeon has a very much larger share than has the solitary obstetric officer. A few beds at most are allotted to him. In all the eleven hospitals there are 4118 beds. Of these, 141 only are devoted to gynecology. Does this beggarly proportion fairly represent the relative importance of the three departments? Does it adequately meet the public need? Does it supply sufficient opportunity for clinical instruction? I will not venture to assert that no one will be found bold enough to answer these three questions in the affirmative; but I may confidently assert that no one will be able to make good the affirmative in a single point. Any one who may make the attempt will have to answer a few more questions. Is disease in women a rare thing? Are the sexual organs and functions in women in themselves unimportant? Is their influence upon the economy and upon other organs insignificant? Is it possible to understand aright disease in women, without taking into account its interaction with the physiology and pathology of the generative system?

I will not now attempt to pursue these questions throughout all their bearings. It is enough to point out how painfully inadequate is the provision in our hospitals and schools for the investigation and teaching of subjects so extensive in themselves, and capable of throwing so much light upon



many problems regarded as simply medical or surgical. It is inconceivable that the provision for this purpose could be so unworthy, so niggardly, were it not dictated by sheer ignorance; for I will not suppose that it is the result of culpable indifference. This scanty provision, then, must be taken as the measure of the estimation in which the pathology of women is held by those who govern our hospitals, who frame our educational curricula and examinations for diplomas, and who generally legislate for the profession.

The same spirit that is seen in this scanty allotment of beds is manifest in the relative position awarded to the teachers. I believe it is still true that in some of our London hospitals the obstetric physicians hold the position of supernumeraries rather than of recognised members of the staff. They are not in all cases elected in the same manner as the ordinary physicians and surgeons: and these last are especially careful not to admit their obstetric colleagues to share in the fees.

It is not wise for any community to brand a section of its members with the stamp of inferiority, or to inflict upon them the injury of representative and administrative disability. But this we might bear with more or less patient acquiescence, if it could be shown that the actual system worked well for the public interest. *A priori*, it might be safely inferred that injustice cannot work well for the public interest; but it is a matter of easy demonstration that the actual system does not work well. I may appeal to every young man leaving our schools, armed with a diploma, therefore conventionally and legally qualified to practise, for testimony in support of my argument. He must needs admit that he enters upon practice very imperfectly prepared to encounter the difficulties and responsibilities of obstetric work. Those who out of their own mere motion and imperfect knowledge rule these matters, have declared that to have attended twenty cases of labour is clinical experience enough wherewith to enter upon obstetric practice; whilst as for gynecology, the clinical instruction he has had is not less absurdly insufficient. I know that many of the best and ablest of our young men keenly feel their deficiency in this respect. They find that

they must by slow and painful steps, under innumerable difficulties, pick up the knowledge which they ought to have acquired during their career as students. Nor are they to blame for this. Students notoriously concentrate their attention upon that which "pays" at the examination-tables. And there, excepting for the licence of the Royal College of Physicians and the degrees of the University, obstetrics and gynecology are of no use; indeed, even certificates of practical study, that dangerous substitute for the examination-test, are hardly required. If proof of practical study and a sufficient examination-test were instituted, then it would follow as a logical consequence that the opportunities for clinical study would be extended, and that students would "get up" this subject.

It is a painful fact, that the greater number of charges of malapraxis brought against medical men relate to obstetric practice. In many cases it could be shown that the fault imputed was the result of ignorance—that is, of imperfect teaching and experience. Unhappily, the law is too apt to declare that ignorance is criminal; and thus an honest man is sent to gaol as a felon for not possessing that knowledge and skill which our governing bodies have denied him the opportunity of acquiring.

It may be said that the great scheme for a joint examination-board will remedy some of the evils to which I have referred. I hope it may be so; but I have little faith in protracted gestation. There is generally something doubtful in the legitimacy of the conception; and the progeny seldom comes to good. It is true that after many false throes, such as climacteric women big with wind are apt to feel, actual parturition seems near at hand. The University, the Apothecaries' Society, and the College of Surgeons, having called in the aid of Parliament, some of the causes obstructing labour have been removed; but still the progress is slow. I am afraid anesthesia is carried a little too far, and that further operative measures may still be necessary. And when the long-promised birth is accomplished, shall we be happy? will our expectations be realized? This is a question which, of course, time and experience only can answer.

It is at any rate premature, if not fatuous, to dote upon that which at best has only a potential existence.

Amongst the burning questions of the day is the propriety or the right of admitting women into the professions. I say the right or the propriety, because even if the right were made good, the propriety would not follow. Why it is that the women have selected medicine as the especial point of attack, is not quite clear. They are far better fitted by organization, by natural aptitude, to shine in the pulpit and at the bar. And in the career of arms, if we may trust legendary or mythic history, they have earned no mean distinction. But medicine, whilst demanding physical power not less than the other professions, is essentially based upon science.

Now, the women who have been distinguished for scientific power might be counted on the fingers of one hand. There seems to be a natural incompatibility between science and the female brain. There is, indeed, no such inherent incompatibility between science and religion or law. Theology and law that are not in harmony with true science must rest upon a very insecure foundation. But the clergy of all denominations, and lawyers as a rule, assume a direct antagonism to science. They set themselves above it; they would trample it down as something in chronic rebellion against their authority. In this antagonism they resemble the women; in these they find their most useful allies. The Church and the law, then, are the professions most congenial to the somewhat arbitrary character of the female intellect. Now this question a committee of the Medical Council have handled in its broad aspect in a recent report submitted to the Council, with a view to transmission to the Privy Council, in a masculine and sensible manner.\* But there is a sub-

---

\* I am sorry to be obliged to qualify this statement. I had not taken into account the probability that the Council would stultify its Committee. The Committee reported as follows: "After due deliberation, the Medical Council have to express their opinion that the study and practice of medicine and surgery, instead of offering a field of exertion well fitted for women, do, on the contrary, present special difficulties which cannot be safely ignored, and some of which cannot be obviated. Instead of medicine offering more facilities and less difficulties for women than other professions, the Medical Council believe that, as the whole question is looked into, there will be found peculiar hindrances, moral and physical, to the successful pursuit of medicine by women. Moreover, they desire to

ordinate question, which, from an organic defect in their constitution, they are not likely to handle satisfactorily—I mean the education, qualification, and registration of midwives.

It is not for want of information, or advice entitled to respect, that the Medical Council and the governing bodies have so long maintained this deplorable state of things. In July, 1868, a Committee of the General Medical Council, of which the late Mr. Syme was chairman, addressed inquiries to the teachers in the various schools, asking for their opinions on the following points: 1. The topics which might most advantageously be included under the subject they taught; 2. The period in a four years' course when that subject could be most profitably studied, and the length of time which should be devoted to it; and 3. The manner in which it could best be taught—that is, by lectures, practical instruction, or otherwise. Upon this invitation all the obstetric teachers in London met in deliberation, and agreed to the following proposition: "The lecturers, considering the great extension which has been made in recent years in the knowledge of obstetrics and the diseases of women and children, the deep interest of the public in possessing practitioners duly skilled in this department, and that it is of paramount importance to the comfort and success of the young practitioner to possess sound theoretical and clinical training in it, are unanimously of opinion that the actual means of study, and the share devoted to it in the present curriculum, are insufficient." Mr. Syme's Committee frankly acknowledged the necessity of extending the instruction in midwifery. "We are decidedly of opinion," they say, "that the present space allotted to midwifery in the regulations of some of the licensing bodies is too short, and that it should, as was formerly the case, extend over one winter session, and that instruction in practical midwifery should also be extended." But nothing has come of this "decided opinion."

---

add, that if it be admitted that women should enter the medical profession, the existence of an equal fitness for other professions must be assumed." This report, after long discussion by the Council, was effectually emasculated.



A body of men, each of whom is a representative of the governing bodies which have affirmed that attendance on twenty cases of midwifery is basis enough to qualify for obstetric practice, can hardly possess the requisite knowledge to legislate upon this question.

The practical question, of course, surges up : But how will you find more time for the study of obstetrics ? The answer is not difficult. Time may be gained by the simple and common-sense process of striking out of the curriculum teaching that oppresses and bewilders the student, that distracts his attention from the real object of his study, that loads his memory without training his intellect. Without hesitation or misgiving, I would abolish the lectures on Forensic Medicine. This course seems to have been designed chiefly in order to train the lecturer in the great art of teaching what he does not know, without exposing his ignorance. I speak from experience, having gone through the ordeal as a very young man. I am afraid I did not succeed in concealing my ignorance from my class. And just when, by dint of teaching, I had begun to learn a little of the subject, I turned it over to a junior to go through the same experience. I could not help thinking how, when *delirant magistri, plectuntur alumni*. I hope the alumni have forgiven me, reflecting that I, not less than themselves, was the victim of an absurd and arbitrary regulation.

The course is usually committed to the youngest lecturer in the school. His "prentice hand" is exercised in teaching the last and most difficult applications of medical knowledge. To do this effectually demands the most matured experience that long practice in medicine, surgery, and obstetrics can supply. No young man can possibly teach efficiently any one of these subjects. It is beyond the power of mortal man efficiently to teach all three. And, practically, this is acted upon by the public. In any case coming before the law-courts, in which a question of medicine, surgery, or obstetrics is involved, do we ever see a simple lecturer on forensic medicine summoned ? Is it not the invariable practice to summon those who have had special experience in the subject ? So again, where a chemical question is involved, is not the

professional chemist appealed to? Society has no longer faith in medical admirable Crichtons: your only admirable Crichton nowadays is the lawyer. He indeed, is privileged to decide in the last appeal upon every question of medicine, surgery, obstetrics, chemistry, and every other science; and the less he happens to know of the science, the more ready he is to cut the knot with the sword of blind justice. The public naturally invoke the aid of those who have had the largest special experience bearing upon the question in dispute. It seems a logical necessity, then, that the physician, the surgeon, the obstetrice, the chemist proper, should each work out his teaching to its applications to the law—that is, to the limited extent which it is possible to attain in a course of lectures.

I could not cite higher authority in support of this view than that of Sir Thomas Watson. In his reply to the Committee he says: "I doubt whether forensic medicine should find a place in a course prescribed for all medical students in common. Its singular interest and its vast importance are unquestionable. But not one in a hundred of those engaged in the practice of medicine in this country can ever become safe guides, or capable of the more difficult and abstract experimental investigations which from time to time are required in judicial proceedings incidental to our social life. These inquiries should be committed to recognised professional experts, who ought not to be regarded as mere witnesses in any case, but as medical assessors to the appointed authorities."

As to the more strictly legal part of forensic medicine, that which teaches the forms and practice of the law-courts in dealing with ordinary and skilled evidence, the relations between true science and that which lawyers construct out of their inner consciousness, and what constitutes malapraxis and other offences in the eye of the law, all this can be better studied in books, and in the courts themselves. This part of forensic medicine should, in short, be studied as medicine proper is best studied—that is, where it can be seen in practice. It is an essential part of the education of every man to learn how the law is administered. In this country

most of us may be called upon to take part in its administration. Observation in the law-courts is the best training for this duty. I have often doubted whether the exemption we enjoy from serving on juries is an unalloyed advantage to ourselves. I am sure it is a disadvantage to the public. The jealousy of the law will hardly bear the presence of scientific men as assessors to the court ; but a skilled jurymen would virtually exercise a similar function : and thus the public would sometimes enjoy the benefit of law enlightened by science, which is now carefully excluded.

It is impossible to resist the opinion of Mr. Simon, " that a good deal of time is wasted in our medical schools, and also much confusion caused, by lecturers on surgery and lecturers on medicine both considering themselves required to teach such general parts of pathology and practice as are common to both branches." A good deal of needless overlapping also occurs between the courses of anatomy, physiology, and surgery ; and almost all the most practised teachers concur in the opinion, that clinical instruction is so incomparably superior to formal lectures in the class-room, that the systematic courses on medicine and surgery might be usefully curtailed.

Applying these principles to the teaching of obstetrics and gynecology, I think I may say that we do not indulge in overlapping. Our time and opportunities are so restricted, and our subject so extensive, that we are often compelled to omit altogether special topics of urgent practical importance which rightly belong to us, and which no one else cares to touch. We can barely indicate the special relations of the anatomy and physiology of the female organs to pregnancy and labour. We must needs assume that the lecturers on medicine and surgery have, out of their greater store of time, instructed our class in the general doctrines of pathology. But still, doing the best we can in the three months' summer course of about forty lectures at our disposal, we find it simply impossible to trace satisfactorily the history of pregnancy, parturition, and childbed, including that of the embryo and the new-born child. It must be remembered that the lectures on obstetrics eminently admit of and require

illustration by specimens, drawings, practical demonstration, and practice in operations. If the class be ever so small, this practice takes considerable time. And after all, the extensive and growing subject of the diseases of the non-pregnant woman is untouched. This is relegated to the winter, when it is taken up in the clinical lectures, which, again, are too few by far to treat it effectively. This would offer less reason for regret if we had adequate clinical material at our command; if we could supplement at the bedside the defects of our systematic lectures, as the teachers of medicine and surgery can and do. But we find ourselves hampered at every turn; told, like the Israelites of old, to make bricks without straw; the difference being that nobody cares whether the bricks be made or not.

---

### Original Communications.

---

#### SOME CASES OF PUERPERAL ILLNESS IN PRIVATE PRACTICE.

By CHARLES J. CULLINGWORTH,  
Surgeon to St. Mary's Hospital, Manchester.

(Continued from p. 237.)

CASE III.—*Natural Labour. — Severe Rigors on Third Day.—Rise of Temperature.—Peritonitis.—Death on Seventh Day.*

Mrs. —, aged thirty, the mother of several children by a former husband, and the subject of chronic uterine disease, was confined after a natural and easy labour on the 11th December, 1873, at 1.55 P.M.

At eleven o'clock on the 13th she had a rigor, and this was followed by a series of rigors of great severity, so that the bed shook. At 2.15 A.M. on the 14th the patient expressed herself as feeling extremely ill, as if she could not live. Her head and limbs were aching, the breathing was



oppressed, and there were pain and tenderness over the lower part of the abdomen. Temp.  $104^{\circ}9$ ; pulse 148.

14th.—10.0 A.M. Temp.  $104^{\circ}6$ ; pulse 140; feels a little better; frequent paroxysms of pain in lower part of abdomen; bowels have acted twice since 2.0 A.M. Ordered a saline mixture with nepenthe. 6.0 P.M. Temp.  $103^{\circ}0$ ; pulse 122; everything looks yellow and misty to her.

15th.—10.30 A.M. Temp.  $103^{\circ}7$ ; pulse 134. A little diarrhea; lies sunk in the bed; skin moist; tongue covered with white fur; suffering a good deal of pain, and troubled with distension from flatus. Ordered brandy, arrowroot, and milk and soda-water. 6.30 P.M. Temp.  $103^{\circ}4$ ; pulse 118; no more diarrhea; abdominal distension; tenderness over the whole surface of the abdomen.

16th.—9.30 A.M. Temp.  $101^{\circ}9$ ; pulse 136; diarrhea and vomiting. 6.30 P.M. Temp.  $103^{\circ}4$ ; pulse 126; face pinched and sunken; vomiting.

17th.—8.30 A.M. Temp.  $101^{\circ}6$ ; pulse 122; no diarrhea; flatulence; running pulse. 12.45 P.M. Temp.  $102^{\circ}0$ ; pulse 128; running and weak. 6.0 P.M. Delirious; picking at bedclothes; very excited; surface cold and clammy. Dashed away the brandy and water offered to her. Died at 6.20 P.M. There was no post-mortem examination.

*Remarks.*—This case differs from the non-fatal cases, in the number and severity of the rigors and the rapidity of the pulse, rather than in the height of the temperature.

I had been attending a case of idiopathic erysipelas of the head and face for several days when called to this patient; a fact which I feel bound to place on record, whatever may be its bearing on the etiology.

Three patients whom I attended in confinement during the same month—namely, one on the 20th, and two on the 21st, made excellent recoveries. In the meantime, I had thoroughly disinfected myself, and had transferred the erysipelalous patient to the care of a friend in consulting-practice.

CASE IV.—*Easy Labour.—Aching of Limbs and Abdominal Pain Forty-four Hours after.—Rise of Temperature.—Enlargement of Uterus.—Peri-uterine Inflammation.—Illness for one Week.—Recovery.*

Mrs. A., aged twenty-five, a spare, light-complexioned woman, in comfortable circumstances, was confined of her fifth child at 2.45 A.M., August 4th, 1874, after a natural and easy labour.

The patient felt remarkably well until 11.0 P.M. on the 5th, when she began to suffer from aching of all her limbs and pain in the lower part of the abdomen. She passed a sleepless night, and at 3.0 P.M. on the 6th I found her still in great pain. Temp.  $104^{\circ}0$ ; pulse 128. The breasts were normally full, the lochia healthy, the tongue clean. Complained of headache and great thirst. The uterus was large and tender, and the tenderness extended to the parts on the right of that organ. A quarter of a grain of morphia was administered subcutaneously. 8.0 P.M. Temp.  $105^{\circ}6$ ; pulse 128. The subcutaneous injection having afforded great relief, was repeated.

7th.—8.30 A.M. Temp.  $101^{\circ}0$ ; pulse 112; resp. 28; had a good night, and looks better; distinct tenderness over the region on the right side of the uterus. A vaginal examination revealed nothing abnormal, save a little tenderness on right side. 3.0 P.M. Temp.  $101^{\circ}6$ ; pulse 104. 8.45 P.M. Temp.  $101^{\circ}8$ ; pulse 96; bowels have acted after a rhubarb draught and enema. Subcutaneous injection of one-sixth of a grain of morphia.

8th.—9.30 A.M. Temp.  $100^{\circ}7$ ; pulse 98; turns over on her side more easily, and has had a good night. 9.45 P.M. Temp.  $103^{\circ}2$ ; pulse 110; quite comfortable.

9th.—9.15 A.M. Temp.  $100^{\circ}1$ ; pulse 88; rather restless night; no pain; no tenderness or fulness about the uterus. 10.0 P.M. Temp.  $103^{\circ}8$ ; pulse 112; had a little fish to dinner. No morphia given.

10th.—8.15 A.M. Temp.  $101^{\circ}6$ ; pulse 100. 9.0 P.M. Temp.  $102^{\circ}7$ ; pulse 100.

11th.—8.0 A.M. Temp.  $98^{\circ}8$ ; pulse 80; had an ex-

cellent night. 7.45 P.M. Temp.  $99^{\circ}8$ ; pulse 72; feels quite well and hungry. The patient went on perfectly well.

*Remarks.*—The very high temperature on the evening of the 6th, and the general appearance of severe illness, naturally gave rise to considerable alarm. The rapid subsidence of the symptoms forms the remarkable feature in this case.

CASE V.—*Quick Labour.—Chill on Fifth Day.—Rise of Temperature. — Metritis. — Peritonitis. — Bronchial Catarrh.—Seven Weeks' Illness.—Recovery.*

Mrs. G., aged twenty-eight, a well-nourished woman, of strumous habit, and in comfortable circumstances, was confined of her fourth child at 10.15 A.M., August 5th, 1874. Labour was rapid, and the child was already born on my arrival; the removal of the placenta was accelerated by external pressure, and accompanied with rather more hemorrhage than usual. No introduction of the hand, or even the fingers, into the vagina was required.

The temperature at the evening visit was  $100^{\circ}2$ , Fahr.; the following morning  $97^{\circ}8$ ; and the morning of the 7th,  $99^{\circ}3$ . In short, the patient went on quite well up to the morning of the 9th—*i.e.*, for four full days, when she complained of feeling chilly and of headache.

*August 9th.*—5.30 P.M. She expressed herself as feeling very ill indeed; her face was flushed, and she was perspiring profusely. Headache, which had been severe all day, had somewhat abated; breathing was gasping and difficult; intellect clear; pulse at the wrist thin, thready, running, and uncountable; temp.  $105^{\circ}5$ ; number of pulsations of the heart, 132; number of respirations, 32. Condition of breasts, bowels, and lochia unaffected.

A subcutaneous injection of morphia (gr.  $\frac{1}{4}$ ) was given; and a small and definite quantity of brandy and water ordered to be administered every quarter of an hour when awake.

*10th.*—9.0 A.M. Temp.  $104^{\circ}$ ; pulse 128. Slept through the night until 4.0 A.M.; since then restless and ill. Is

perspiring profusely. Pulse a little firmer than last night, and regular. Repeated the subcutaneous injection. 8.30 P.M. Abdomen generally tender and somewhat distended. Ordered a mixture containing ten-drop doses of turpentine every two hours, and linseed-meal poultices to the abdomen. Gave  $\frac{1}{3}$  grain of morphia subcutaneously. Temp.  $104^{\circ}7$ ; pulse 128.

11th.—8.30 A.M. Temp.  $104^{\circ}$ ; pulse 116. Less distension; nausea. Ordered ice and iced champagne. 8.0 P.M. Sickness increased by the champagne; milk and soda-water more agreeable; no vomiting since noon. Looks a little better; is perspiring profusely; has much less abdominal tenderness and distension, and a fuller and firmer pulse. Temp.  $105^{\circ}1$ ; pulse 120.

12th.—9.0 A.M. Temp.  $102^{\circ}8$ ; pulse 116. Slept well. Complains of irritation in the throat, producing nausea. 8.30 P.M. Temp.  $103^{\circ}8$ ; pulse 128. Asks for porter and fish, which are refused.

13th.—10.0 A.M. Temp.  $102^{\circ}5$ ; pulse 104. 8.30 P.M. Temp.  $102^{\circ}9$ ; pulse 120. No sleep for twenty-four hours; manner excited; hallucinations; abdomen again tender and distended. Gave  $\frac{1}{3}$  grain morphia subcutaneously, and ordered brandy to be administered freely.

14th.—9.0 A.M. Temp.  $101^{\circ}7$ ; pulse 114. Restless night; less tympanitic distension; great tenderness over whole abdomen, especially in right iliac region. 8.30 P.M. Temp.  $104^{\circ}4$ ; pulse 132. Very quiet; sinks deeply in the bed; is tired of brandy and beef-tea, and asks for raspberry-vinegar; tongue continues moist. Gave morphia (gr.  $\frac{1}{3}$ ).

15th.—9.0 A.M. Temp.  $101^{\circ}4$ ; pulse 110. Slept fairly; takes a quiet interest in all that is going on. 8.30 P.M. Temp.  $102^{\circ}2$ ; pulse 126, weak and irregular; retention of urine; catheterization.

16th.—9.0 A.M. Temp.  $101^{\circ}$ ; pulse 120. Has been very ill during the night; no delirium; profuse perspiration. Ordered enema and vaginal injection. 11.0 A.M. Consultation with Dr. James Whitehead. We found very little abdominal tenderness, and not much distension; uterus



enlarged and tender, extending to midway between pubes and umbilicus. Ordered vaginal injections with McDougall's disinfecting fluid, and a mixture containing *liquor ammoniæ acet.* with ammonia. 9.0 P.M. Temp.  $103^{\circ}2$ ; pulse 126. Complains of mouth feeling "full of bits;" has vomited; passed urine voluntarily after enema this morning. Subcutaneous injection of morphia, nearly  $\frac{1}{2}$  grain.

17th.—9.45 A.M. Temp.  $100^{\circ}7$ ; pulse 110. Very quiet night; no ill effects from morphia. 9.0 P.M. Temp.  $102^{\circ}7$ ; pulse 132. Weak and very quiet.

18th.—9.0 A.M. Temp.  $102^{\circ}7$ ; pulse 122. 9.0 P.M. Temp.  $104^{\circ}$ ; pulse 126. A little more abdominal swelling and tenderness.

19th.—9.30 A.M. Nausea; continuous epigastric pain; temp.  $102^{\circ}8$ ; pulse 130; resp. 40. 9.15 P.M. Dry tongue; great prostration; temp.  $101^{\circ}5$ ; pulse 120; resp. 34.

20th.—9.30 A.M. Temp.  $102^{\circ}3$ ; pulse 124; resp. 34; gave subcutaneously  $\frac{1}{4}$  grain of morphia. 9.0 P.M. Temp.  $103^{\circ}3$ ; pulse 124; resp. 32. Nausea; perspiration; no unconsciousness. Half a grain of morphia subcutaneously.

21st.—8.45 A.M. Temp.  $101^{\circ}7$ ; pulse 108. An enema, administered this morning, brought away lumpy feces. 9.0 P.M. Resp. 36, laboured; temp.  $103^{\circ}1$ ; tongue desquamating down centre.

22nd.—9.0 A.M. Delirious during the night; temp.  $100^{\circ}8$ . 9.30 P.M. Feeble, running pulse; temp.  $101^{\circ}5$ ; resp. 34. Ordered to increase the quantity of brandy (12 ounces given during the next 12 hours).

23rd.—Had a troublesome cough. Temp. in morning,  $98^{\circ}6$ ; night,  $104^{\circ}2$ ; resp. 34.

24th.—Ditto. Temp. morning,  $99^{\circ}$ ; night,  $99^{\circ}9$ .

25th.—Ditto. Temp. morning,  $98^{\circ}8$ ; night,  $99^{\circ}1$ .

26th.—Cough relieved.

30th.—10.0 A.M. Temp.  $99^{\circ}2$ ; pulse 108. Sits propped up in bed. Cough quieter.

31st.—10.0 A.M. Tongue cleaning. Ordered a cinchona mixture with potassium chlorate.

September 1st.—12.0 noon. Temp.  $98^{\circ}6$ ; pulse 88. To be moved to a couch while the bed is made.

3rd.—Allowed to sit in an easy-chair for a few minutes.

5th.—10.30 A.M. Temp.  $100^{\circ}4$ ; pulse 88. Looking and feeling much better; a good deal of cough and expectoration. Ordered half an ounce of cinchona wine (Malaga) twice a day, and two of Savarasse's copaiba capsules twice a day.

11th.—12.0 noon. Temp.  $103^{\circ}4$ ; pulse 120; was up yesterday a little for the first time; passed a restless night, and has lost her appetite; vaginal mucous membrane feels unnaturally hot and cushiony; some induration of the tissues around the uterus, which is enlarged, and not easily movable. Ordered absolute rest in bed.

12th.\*—Temp.  $103^{\circ}5$ ; pulse 120; vomiting.

13th.\*—Temp.  $99^{\circ}0$ ; pulse 90; slight hemorrhage taking place with a few clots. Is feeling much better.

21st.—Hemorrhage ceased three days ago. Temp. to-day,  $99^{\circ}0$ ; pulse 98.

24th.—Continuing better, and allowed to get up a little.

28th.—To go out on the first fine day.

October 1st.—The patient having quite recovered, the attendance ceased. She has remained up to the present time (May, 1875) in good health.

*Remarks.*—I was not able to trace here the influence of any infectious disorder. It will be observed that the confinement took place on the day after Case IV.; whether there was communication of infection from the one to the other, I do not know. The child was born in this instance before my arrival, and the placenta was removed without the introduction of the fingers into the vagina, so that I can scarcely reproach myself with having carried contamination by touch.

During that part of the illness in which the breathing was accelerated, a careful examination was made daily as to the condition of the heart and the anterior part of the chest, and nothing was found to account for that symptom. Examina-

---

\* These notes (September 12th and 13th) were made by my friend Dr. Thorburn, who kindly saw the patient during my temporary indisposition.

tion of the back of the chest was deemed inadvisable, owing to the patient's general condition.

The fact of the temperature rising suddenly on the 11th and 12th of September, and subsiding again with equal suddenness, so that, on the morning of the 13th, it had fallen from  $103^{\circ}5$  to  $99^{\circ}$ , is difficult to account for, unless it was due to some retention of clots in the uterus. The appearance of slight hemorrhage from the vagina, and the passage of some clots, on the day on which the temperature fell, seem to be somewhat in favour of this hypothesis.

It may be interesting to note that I ceased from obstetric practice for the period during which I was in attendance upon this case; and that the next patient whom I attended, two months after the date of Mrs. G.'s confinement, made an excellent recovery.

CASE VI. — *Placenta Prævia.* — *Turning.* — *Rupture of Vagina (?)* — *Collapse.* — *Rise of Temperature next Day.* — *Offensive Vaginal Discharge.* — *One Week's Illness.* — *Recovery.*

Mrs. A., aged forty-one, the wife of a journeyman painter, and the mother of six children, sent for me on account of profuse uterine hemorrhage, on the 21st March, 1874. Her previous health had been good, and all her labours natural and easy. She had already had a smart hemorrhage a month ago, and considered that she was now at full term. On my arrival I found that she had lost about two pints of blood (which were shown to me), and that blood was still trickling from the vagina. There was complete placental presentation, and no uterine pains had yet occurred. The general condition of the patient was good, and the hemorrhage was arrested by plugging the vagina.

The plug was removed on the evening of the 23rd. There was no further hemorrhage until between 6.0 and 7.0 A.M. on the 25th; when she lost a considerable quantity of blood (about two quarts), and became weak and pale. As there was still some loss going on when I arrived, I introduced my hand into the vagina, found the placenta adherent all round

the os uteri, which was only beginning to dilate, separated the placenta on one side, dilated the os manually, ruptured the membranes, turned, and delivered the patient of a still-born male child at 8.30 A.M. There were no uterine pains. Some force was requisite to bring the breech through the os uteri; no other difficulty was encountered; the placenta followed immediately, and there was no further loss.

After delivery collapse gradually set in, and in half an hour became most alarming. The patient was cold and pulseless; the features were pinched, pale, and livid; there were sighing respiration and vomiting. The uterus remained all the while firmly contracted. Brandy was freely administered, and my friend Mr. Smart, Surgeon to the Manchester Southern Hospital, came to my assistance. On vaginal examination, we both of us thought we could detect some laceration of the vaginal wall, high up, posteriorly. A third of a grain of morphia was given subcutaneously. After a little time the pulse became just perceptible, but the patient's general appearance did not improve for two hours. At noon she had rallied considerably; the pulse then was 80 in the minute, and firm. Vomiting continued throughout the day. The urine was withdrawn by the catheter every eight hours, and ice was given freely.

26th.—9.0 A.M. Temp.  $99^{\circ}5$ ; pulse 84. Slept well. Subcutaneous injection of  $\frac{1}{4}$  grain morphia. 5.0 P.M. Temp.  $102^{\circ}3$ ; pulse 96. Cheerful and comfortable; no vomiting. 10.30 P.M. Temp.  $102^{\circ}$ ; pulse 100. Some pain and tenderness above the pubes. There had been no rigor. Linseed-meal poultices were ordered, and a quarter of a grain of morphia injected subcutaneously.

27th.—8.30 A.M. Temp.  $100^{\circ}8$ ; pulse 96. Slept well, and felt much relieved by the poultices. Tenderness still present, and pain felt on breathing deeply or coughing. Iced soda-water and milk were alone allowed, and another  $\frac{1}{4}$  grain of morphia administered subcutaneously. 4.30 P.M. Temp.  $102^{\circ}6$ ; pulse 104. 10.30 P.M. Temp.  $100^{\circ}5$ ; pulse 102. No rigor; no abdominal distension; tongue clean and moist. Half a grain of morphia injected subcutaneously.



28th.—10.30 A.M. Temp.  $101^{\circ}7$ ; pulse 104. Slept well; thin offensive discharge from vagina. Injected  $\frac{1}{4}$  grain morphia. 4.30 P.M. Temp.  $100^{\circ}8$ ; pulse 96. 10.30 P.M. Temp.  $103^{\circ}$ ; pulse 104. Complains of pain in the bowels, and of some watery discharge from the rectum (?). Washed out the vagina with warm water containing Condyl's fluid, and administered an enema. Some flatus passed, but no feces. Injected  $\frac{1}{2}$  grain morphia subcutaneously.

29th.—10.30 A.M. Temp.  $100^{\circ}3$ ; pulse 95. Rested badly for the first time.  $\frac{1}{4}$  grain morphia given subcutaneously. 4.30 P.M. Temp.  $102^{\circ}2$ ; pulse 100. Slept a little; no pain; very little tenderness; no action of bowels; free and somewhat offensive discharge from the vagina. A little beef-tea is being administered. 10.30 P.M. Temp.  $101^{\circ}9$ ; pulse 90. Vagina again washed out, and  $\frac{1}{4}$  grain morphia injected subcutaneously.

30th.—9.30 A.M. Temp.  $100^{\circ}6$ ; pulse 100. Feels much better. No morphia. 8.0 P.M. Temp.  $100^{\circ}3$ ; pulse 86. Vagina washed out. No morphia.

31st.—9.0 A.M. Temp.  $99^{\circ}2$ ; pulse 86. Restless during early part of night. Bowels acted for first time at 3.0 A.M. 9.0 P.M. Temp.  $100^{\circ}7$ ; pulse 88. Discharge still abundant and offensive; vagina again washed out.

April 1st.—9.30 A.M. Temp.  $99^{\circ}3$ ; pulse 80. Ordered a meat diet. 10.0 P.M. Temp.  $100^{\circ}3$ ; pulse 84. Very little discharge.

2nd.—9.30 A.M. Temp.  $99^{\circ}$ ; pulse 74. Vagina again washed out. 9.0 P.M. Temp.  $99^{\circ}3$ ; pulse 75. No pain; no offensive discharge.

4th.—9.0 A.M. Temp.  $98^{\circ}2$ ; pulse 72. Vaginal washing repeated, as there has been rather more discharge.

6th.—11.0 A.M. Temp.  $98^{\circ}4$ ; pulse 64. No pain; a little tenderness on deep pressure in left iliac region. Appetite good; bowels regular; urine loaded with lithates.

8th.—Scarcely any discharge. Sleeps and eats well. On vaginal examination nothing abnormal is found in the state of the vaginal walls; the only portion of the cervix projecting into the vagina consists of the lips of the os, which are

thick, soft, and separated, leaving the os patulous. Allowed to sit up a little. The patient from this date made a good recovery.

*Remarks.*—I have, in this series, hitherto alluded to the character of the labour itself in the briefest possible manner ; in this instance, however, I have departed from that rule for two reasons :—1. Because it is desirable that in every case of *placenta prævia* the details should be placed on record ; and, 2. Because in this particular case the subsequent illness was due to, or at any rate influenced by, the character of the labour.

It will be noticed that throughout the series free use was made of the subcutaneous injection of morphia. No ill effects were encountered, and more than once I am convinced that the patients were thus helped to pass safely over exceptionally hazardous periods in their illness. In illustration of this, I would draw attention to the quiet sleep induced by its means during the first night of the illness in Cases IV. and V., and to the result of its administration during the stage of collapse in Case VI.

It is, however, beyond the scope of this paper to make any lengthy observations either on the nature, course, or treatment of the cases I have related. Nevertheless, I trust that they will not be wholly without value, simply as a series of clinical histories.

---

## NOTE ON THE SPONTANEOUS DISAPPEARANCE OF ASCITIC FLUID.

By ELIZABETH GARRETT ANDERSON, M.D.

AT the meeting of the Dublin Obstetrical Society recorded in the "Obstetrical Journal" of July, mention was made by Drs. Atthill and Byrne of the occasional spontaneous disappearance of ascitic and ovarian fluid. A case of ascites in which this appears to have happened several times has recently come under my notice.

A lady, aged sixty-nine, consulted me for prolapsus uteri. On examination I found the abdomen contained a large quantity of fluid. I told the patient it would probably be necessary at no very distant date to remove this by tapping; but on hearing this she said very placidly, "I think not; I have been quite as big as this three times before, and each time the water has come away 'of itself.'" On inquiry, it seemed from her account as if the water had come from the bladder. She said it had not been passed involuntarily; she had suddenly felt an urgent and frequent desire to micturate, and had passed enough to fill five or six utensils at intervals in one day. This had happened three times. The history seems to point to the existence of a minute fistulous opening into one of the ureters or into the bladder. The enormous flow of watery urine so often seen in hysteria, suggests that it is not impossible for the action of the kidneys suddenly and spontaneously to increase to the extent required for the removal of a large quantity of ascitic fluid; but of the two explanations, this appeared in the case I have mentioned to be the least probable.

---

A LETTER IN FACSIMILE,  
AND OTHER HITHERTO UNPUBLISHED  
MEMORIALS OF HARVEY.

Collected by J. H. AVELING, M.D.

Physician to the Chelsea Hospital for Women, Honorary Librarian of the  
Obstetrical Society of London, &c.

(Continued from p. 221.)

ANOTHER specimen of Harvey's handwriting is to be found amongst the State Papers in the Public Record Office, Domestic Series, Charles I., vol. xlvii. No. 9. It is a certificate relating to the health of Sir William Sands.

"I doe hereby certefye of a thruth that S<sup>r</sup> Will Sands is in body infirm & suiect to those diseases in the cuntry he

cannot reseyve remedy for ; nor vndergoe & performe that coors of phisick w<sup>ch</sup> is fitting for his recovery.

*W<sup>th</sup>. Harvey*

The following document from the same source shows that at this period the country gentry were not allowed to remain in London during the winter without special permission.

"18th Nov<sup>r</sup> 1624.

"MR. ATTORNEY

"His Ma<sup>tie</sup> is graciously pleased in regard of the indisposician of health of S<sup>r</sup> William Sandis and his Ladie and the great danger of their remove into the Countrey as appears by the inclosed certificate of Doctor Haruie to dispense w<sup>th</sup> their stay in London this winter season not w<sup>th</sup> standing the proclamation. And accordingly requiries yo<sup>n</sup> to take present order for their indempnitie that noe charge or troble come upon them for their stay in London this winter for which they have his Ma<sup>ties</sup> leave."

Another certificate signed by Harvey, but not written in his own hand, may be found among the State Papers, Domestic, Charles I., vol. cclxxiii. No. 8.

"Having had experience of the disposition and weakness of the body of S<sup>r</sup> Thomas Thynne Knight (who hath beene and is still our patient) Wee testifie that wee are of opinion that it will be dangerous for the health of his body to travell this winter into the Contrie and place of his usuall abode untill hee hath better recovered his health & strength.

"Dec. the 2<sup>nd</sup> 1637.

*W<sup>th</sup>. Harvey*

"DANIEL OXENBRIDGE."



The Album of Philip de Glargis, which was a short time since in the possession of Mr. Dawson Turner, but is now in the British Museum among the MSS. Jure Emptionis, No. 23,105, contains the following entry by Harvey.

"Dii laboribus omnia vendunt. Nobliss<sup>e</sup> Juveni Medico Phillipi d Glargi amicitiae ergo.

"Libente scrips.

*Gal: Harbeus*

"Anglus Med. Reg. et Anatomie professor Londini.  
"May: 8: A.D. 1641."

The entries in this little book were made between the years 1636-1641. The first is by "Jacobus Primirosius, Medicinæ Doctor," who practised at Hull, and enriched Obstetric literature by his work "De morbis mulierum," published in 1665.

---

The next memorial consists of a letter from Richard Cave to Prince Rupert concerning the health of his brother, Prince Maurice. It is preserved among the Rupert Correspondence in the British Museum, MSS. Jure Emptionis, 18,980, fol. 125.

"May it please yo<sup>u</sup> Hig<sup>s</sup>

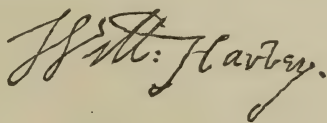
"This last night arrived here att Millton Doct<sup>r</sup> Harvey and Doct<sup>r</sup> Smyth and this morning they with the other two Doct<sup>rs</sup> having seene and spoken with his Hig<sup>s</sup> yo<sup>r</sup> brother intreated me to write as followeth.

"That his sicknesse is in the ordinary raging disease of the Army, a slowe feaver with great deiection of strength, and since Fryday he hath talked idly and slept not, but very unquietly, yet the last night, he beganne to sleepe of himselfe, and tooke his rest soe quietly that this present morning when Doct<sup>r</sup> Harvey came to him he knew him and wellcomed

Doct<sup>r</sup> Smyth respectively, and uppon Doct<sup>r</sup> Harvey's expression of his Ma<sup>ty</sup>s sorrowe for and great care of him he shewed an humble thankfull sence thereof: Doct<sup>r</sup> Harvey asking his Hig<sup>s</sup> how he did, he answered that he was very weake, and he seemed to be very glad to heare of and from yo<sup>r</sup> Hig<sup>s</sup> as was delivered by Doct<sup>r</sup> Harvey.

"Now the Doct<sup>rs</sup> having conferred, and computed the tyme, have good hopes of his recovery, yet by reason that the disease is very dangerous and fraudulent, they dare not yet give credite to this allteration. And concluding the disease to be venemous they doe resolve to give very little phisick, only a regular dyett and Cordyall Antidotes. The Doct<sup>rs</sup> present theyr most humble service to yo<sup>r</sup> Hig<sup>s</sup> and subscribe themselves

"Sr. yo<sup>r</sup> Hy<sup>s</sup> most humble servants



"ROBERT VILVAIN

"EDMUND SMITH\*

"THO. KING.

"Millton, Octob. 17<sup>th</sup> 1643."

The treatment by "very little phisick" and "only a regular dyett" seems to have been successful, for Cave, writing soon afterwards to Prince Rupert, says, "Maurice is not able yet to write letters but hath this day taken physick and so intends to bid his physicians farewell."

For other little-known documents relating to Harvey, the reader is referred to vol. i. p. 23 of this Journal.

---

\* This is the same Dr. Smith mentioned in Harvey's letter.

## Reports of Hospital Practice.

---

### THE HOSPITAL FOR WOMEN.

#### CASE OF CYSTOCELE—STOLTZ' OPERATION—CURE.

By HEYWOOD SMITH, M.A., M.D.

Physician to the Hospital for Women, and the British Lying-in Hospital.

I CONSIDER this case worthy of record because the operation here described has, as far as I know, not been performed before in England, and because its success may encourage others to perform it, as it appears to afford a greater prospect of cure in similar cases than those methods of procedure that are usually had recourse to.

E. S., aged twenty-eight, married first  $2\frac{1}{4}$  years ; widow one year ; married, secondly, six years. Catamenia commenced at fifteen years of age : regular, scanty, with pain. One abortion ; one child eight and a half years ago. Had good health until a fortnight after her confinement, when she noticed that "something came down." Was an out-patient at a general hospital, where the physician said she was suffering from weakness. Afterwards saw Dr. —, who said that the swelling in the vagina "was owing to dead skin that got filled with wind, and that it ought to be removed," but she would not submit to the operation. About four years ago the physician at another general hospital said she had prolapsus, and gave her an instrument to wear.

She was admitted into the Hospital for Women, January 25th, 1875, under my care.

Has been much worse the last nine months. At present complains of pain in the region of the bladder and a dragging pain in the bowels. The pain is worse when the patient moves about or sits up. There is leucorrhœa, and a difficulty and pain on passing water. When the patient walks, the urine passes involuntarily. Bowels regularly open, appetite not good. There is no history of hereditary disease. On further questioning, she reluctantly said that six weeks after her last marriage her husband in a passion forced his knees

deeply into the hypogastrium, so as to cause her to defecate and micturate ; ever since which she has felt the forcing of the bladder.

*Vaginal Examination.*—Uterus prolapsed, and, on the patient straining, the bladder is forced down to the size of a small orange. Uterine sound passes  $3\frac{1}{2}$  inches to the right, and slightly forwards, with fundal pain. The cervix was freely scarified. At a consultation it was advised, before operating, to try rest and mechanical support.

*February 2nd.*—A glass stem was inserted, together with Dr. Protheroe Smith's "elastic" pessary.

*5th.*—Uterus in very good position.

*9th.*—Feels as if the bladder still comes down when she walks. *V. E.* Uterus *in situ*. Strong solution of tannin to be applied to vagina. This was done frequently, and on

*12th.*—Pessary removed, and a plug soaked in the glyc. of tannic acid introduced into the vagina in front of the cervix, the stem being still retained.

*16th.*—Elastic pessary re-introduced.

*19th.*—Period on, with much less pain.

Discharged for a time ; to attend as out-patient.

Re-admitted *April 19th.*

The irritation in the vagina has been less since the application of the tannin, but the bladder still comes down in front of the cervix.

I then purposed to perform Emmet's variation of Sims' operation of Elytrorraphy—*i.e.*, the contraction of the anterior vaginal wall by the trowel-shaped denudation. But Professor Stoltz of Nancy being in the theatre with several other foreigners, he explained to me an operation he had himself devised and practised successfully, and I determined to try it.

The prolapsed cervix was held down firmly by a vulsellum and a circular portion of the anterior vaginal mucous membrane was removed, of about the size of a crown-piece, or rather larger ; a single ligature of stout silk was then "run," (*i.e.*, the stitches made in and out), about one-eighth of an inch from the margin of the wound, all round it, in the same way



as a thread would be run round the edge of a bag to draw its mouth in, the end of the ligature being brought out close to the place where it was first inserted. The two ends were then drawn quite tight and tied, the wound appearing as a small corrugated point. In performing this operation, the chief point to be attended to is, that the ligature should be passed at a sufficient distance from the edge of the wound, and that each stitch should be of sufficient depth to prevent its soon cutting out.

The water was ordered to be drawn off, and the patient to be kept quite quiet in bed.

*May 5th.*—The suture came away to-day.

*11th.*—*V. E.* Wound healthy and contracted.

*18th.*—Has no pain. Arg. nitr. applied to the granulations.

*21st.*—Patient discharged cured.

This patient has been seen lately, and the uterus remains in its natural position, and neither it nor the bladder has at all prolapsed since the operation. I can therefore confidently recommend this operation of Professor Stoltz in similar cases, and also in cases of severe retroversion, on the posterior wall of the vagina, as being more easily performed than either Sims' or Emmet's operations, and because there is no undenuded portion left. For in these latter operations not only is a pouch of free mucous membrane shut up in the wound, but the denuded portion is necessarily somewhat narrow, and so the operation sometimes fails from the incomplete union of the whole of the raw line ; whereas in Stoltz' operation the whole raw surface (the interior of the bag, as it were), is brought into contact, and the result is a greater amount of contraction than is afforded by other methods.

---

## General Correspondence.

---

### GANDER MONTH.

*To the Editor of the Obstetrical Journal.*

DEAR SIR,—I have just read in your valuable Journal a letter from one of my old professors asking for an explanation of the term “Gander Month.”

I have always understood it to mean the month subsequent to confinement in which a woman keeps her room. The origin is taken, I believe, from the fact, that whilst the goose is incubating, the gander wanders round in a disconsolate manner, or may be seen standing on one foot near the nest, awaiting her appearance at the end of the hatching month, with her downy tribe.

I think this is a truer interpretation, and certainly a more charitable one of “Gandering,” than that which Dr. Smith gives.

I am, yours truly,

J. S. NIVEN.

London, Ontario, June 18th, 1875.

---

DEAR SIR,—The lying-in month is called in Welsh “Mis Ceiliog-gwydd,” or shortly “Misclac-gwyll,” the literal translation of which is “month of the cock goose,” or Gander month. I had never heard or seen this expression in English until I read Dr. Smith’s letter in the Journal.

Very truly yours,

JOHN WILLIAMS.

28, Harley-street, W.

---

# THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

AUGUST, 1875.

---

## THE STATUS OF OBSTETRIC MEDICINE.

IT would be well for the Medical Profession and the general public if the address delivered by Dr. Barnes at the annual meeting of the Metropolitan Branch of the British Medical Association could be heard or read and fully digested by all those whose position will entail upon them the task of conducting future professional legislation. Clearly and vividly he has given us in one picture the many retarding influences which delay the advance and full development of Obstetric Medicine, and fearlessly he has portrayed the inferiority of the position which it still holds, and the injustice of those who, whether from interest or indifference, acquiesce in allowing it to remain thus degraded. He asks very pertinently whether there is anything peculiar in the practice of physicians and surgeons which renders them superior in education, attainments, and capacity to those who practise as obstetricians, and why it is that Obstetric Medicine is all but absolutely unrepresented in the General Medical Council, in the Senate of the University of London, in the College of Surgeons, and, he might have added, in the Executive Council of the British Medical Association. But there is a point of far greater importance than that of professional consideration and position—one which affects the interests and happiness of the people to a degree little appreciated at present. The utterly inadequate instruction in Obstetrics which students now receive is a subject than which there is none graver or more urgently demanding reform. It is unjust and productive of evil both to the profession and to the public. The young medical man, whose comfort and success in practice depend mainly upon the completeness of his

obstetric education, finds himself only too often miserably wrecked at the onset of his career for want of it, and the confiding public, trusting the medical teaching and examining bodies, and believing them incapable of sending forth amongst them ignorant obstetricians, fall victims to their misplaced confidence, and suffer even greater mental torture than the inexperienced practitioners who have been the cause of their misery. To remedy this, a longer course of instruction and more extended clinical teaching is absolutely necessary. Some or all of the time devoted to Botany and Forensic Medicine might with advantage be given to the further study of Obstetrics and Gynecology, and a larger proportion of beds in the general hospitals ought to be filled by patients suffering from the diseases peculiar to women. These and other desirable changes can only be effected by bold and energetic action. To be successful it must also be persistent and controlled by suitable organization. The influence of an address, even as pointed and eloquent as this of Dr. Barnes, has its potency devoured by time. Were the obstetric teachers to associate themselves with a determination not to disband until they had obtained what they consider they have a fair right to demand for their branch of Medicine, we believe they would have the sympathy and assistance of the bulk of the profession, and would most certainly in due time rectify their present anomalous position, render the instruction of the student more complete, and materially assist in increasing the safety and happiness of the public.

---

### Abstracts of Societies' Proceedings.

---

#### OBSTETRICAL SOCIETY OF LONDON.

*Meeting, July 7th, 1875.*

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., *President,*  
*in the Chair.*

The following gentlemen were elected Fellows of the Society :—  
John W. Mason, M.R.C.S. ; John Powdrell, M.R.C.S. ; Mathew Reid,  
L.R.C.P. Ed. ; and W. K. Walker, M.R.C.S. (Calcutta).



A letter from Dr. Arthur Farre was read by the President offering a large collection of casts showing the various conditions of the uterine organs, including the placenta, in health and disease, for the acceptance of the Society. Three casts were shown merely as specimens of the collection. These were beautifully modelled and painted, representing as naturally as possible the appearance of morbid specimens.

After a few words from the President, the offer of Dr. Farre was accepted by the Society by universal acclamation.

---

*Specimen of Blighted Ovum.*

Dr. EDIS showed for Dr. Stothard of Hull, a shrivelled fetus of about the fifth month of utero-gestation that had been expelled at full term together with another living fetus. The case having been one of twins.

Both placentas were attached to the same bag of membranes, the one being considerably smaller than the other.

The PRESIDENT suggested whether the case was one of superfetation.

Dr. EDIS replied that the position of the placentas scarcely warranted such a conclusion.

---

*Specimen of a Uterus removed from a Patient Post-mortem in which Cæsarian Section had been performed eight years since on account of extensive Disease of the Os and Cervix Uteri.*

Dr. NEWMAN exhibited the above, thinking it would be of interest to many to know the issue of a case that had been previously reported to the Society.

In 1872 the patient was delivered with the aid of long forceps of a living child at full time. In August, 1874, the integument of the abdominal wall suddenly gave way, and a large coil of intestine protruded. This was returned by Mr. Row, but she died within a week. A large dissecting sub-peritoneal abscess was discovered lying mainly between the under surface of the rectus abdominis and transversalis fascia.

The PRESIDENT observed that it was in reference to this case Dr. Farre had desired to show one of the casts present.

Dr. PLAYFAIR inquired if anything had been discovered in the condition of the cervix which would explain the difficulty for which Cæsarian section had been resorted to.

Dr. NEWMAN replied that he had only made a slight examination of the uterus. When pregnancy occurred in 1871 there were distinct traces of scar tissue, the cervix being deeply indented and changed in structure.

---

*Dermoid Cyst.*

Dr. BARNES exhibited a specimen containing hair and several well-developed teeth, and a firm jaw plate of osseous substance. The cyst had been removed by ovariectomy. It was filled with pus at the time of the operation. The patient died on 2nd day.

---

*Venous Coagulum.*

Dr. BARNES also exhibited a uterus, the internal surface of which was covered with dark decomposing mucous debris, the left femoral vein being blocked with coagulum; there having been diffuse cellulitis of left thigh. The patient, aged forty-five, married, was admitted May 8th, with an abscess under left arm which had burst before admission. She had been subject to eczema for three years. She was delivered on June 19th at about the seventh month, and died on July 1st. The case was of interest as throwing some light on the discussion at the present time.

---

*Colloid Tumour.*

Dr. BARNES also exhibited a large conglomerate of colloid tumours, weighing 28 pounds, which had grown from the omentum. It was supposed to have been ovarian, and had been removed by gastrotomy twenty-five days previously, the patient doing well. The few large vessels connecting the growth with the omentum had been tied by silk ligatures and left in the abdomen. Mr. Haward had examined the tumour and pronounced it to be malignant.

Dr. SNOW BECK inquired as to the condition of the uterine veins.

Dr. BANTOCK asked if there had been any history of inflammation previous to the operation, as pus was discovered.

Dr. BARNES replied that he knew little of the case previous to the operation.

---

*Extra-uterine Fibroid removed by Gastrotomy.*

Dr. ROUTH exhibited a specimen weighing  $17\frac{1}{2}$  pounds. His object was merely to show the tumour in its recent form. On a future occasion he would give full particulars both as to the history and treatment.

Dr. HEYWOOD SMITH inquired if the clamp had been used, and how much of the body of the uterus was involved in the tumour.

Dr. ROUTH replied that no clamp had been used.

The discussion on puerperal fever was then continued.

*On the Relation of Puerperal Fever to the Infective Diseases and Pyemia.**(Continued from p. 268.)*

The PRESIDENT: We now proceed to the discussion on puerperal fever, which has been adjourned on two or three occasions. It is the general feeling that the discussion should close this evening, and if necessary the time can be prolonged for the purpose. If Mr. Spencer Wells is present, he will have an opportunity of replying. I may add that we are honoured to-night with the presence of two of our distinguished honorary fellows, Dr. Fordyce Barker (who has come from America especially to take part in the discussion) and Dr. Charles West. Dr. Fordyce Barker has explained to me that he has much to say on this subject that may be somewhat at variance with what has been said by previous speakers, and he fears he will scarcely be able to conclude within the prescribed limits. He therefore throws himself on the indulgence of the Society if he transgresses.

Dr. SNOW BECK: With your permission, sir, I shall be very happy to concede any right of priority that I may have to the distinguished visitors who are present this evening.

Dr. FORDYCE BARKER: Mr. President and Fellows of the Obstetrical Society,—I feel that I have no right to waste your time in personal topics, except to express my warm appreciation of the honour I have received at your hands, and to appeal to your courtesy to excuse any deficiencies which may be charitably ascribed to weakness of the vocal organs or to the bashfulness which a stranger must feel in addressing such a body of obstetrical men on such a topic as puerperal fever. I may be permitted to say that this discussion is watched with great interest by your brotherhood on the other side of the Atlantic, but that, so far, it does not seem to have led to the happy result spoken of by the Psalmist, "He maketh men to be of one mind in a house." I shall aim, in the remarks which I am about to make, to confine myself closely to a discussion of the questions now before us, and to state my opinions, and the reasons which I have for holding such opinions, in the most terse, compact language compatible with clearness. If these opinions seem to be in antagonism with the great majority of those which have been expressed, I ask that they may not be regarded as partisan in their tone, but as a presentation of views necessary for a full and comprehensive study of the subject. I ask that the arguments urged may receive that candid analytical sifting requisite for a sound decision as to what is the true answer to the questions now before us. I concede to all, and I ask all to concede to me, that it is the truth, and not forensic success, which is sought for by all who speak in this discussion. I cannot suppose that the proposer of the six questions intended that all of us should accept each of these questions with all the qualifying phrases. Of the six questions proposed, the second and third only admit of

the categorical answer of "yes" or "no"; and this will be one or the other, depending upon the answer given to the first question. If the first question be answered in the affirmative, the answer to the second will be in the negative, and the answer to the third will be in the affirmative; or the answers to all three of these questions may be exactly the reverse. I suppose the meaning of the first question, as defined by the second and third, to be: Is there a disease peculiar to women after childbirth never met with under any other circumstances, as distinct as typhoid or typhus fever, scarlet fever, measles, or small-pox? I do not suppose that the proposer of these questions intended or thought that all who believed that the first question could be answered in the affirmative, that puerperal fever is a distinct disease, must therefore accept all the qualifying phrases. "Distinctly caused by a special morbid poison" is one of these qualifying phrases. Is this considered an essential characteristic of a distinct disease? Has science yet determined what the special morbid poison of typhoid fever is? If the phrase had been "*often* caused by a special morbid poison," I should have accepted it as true of puerperal fever, as I believe it to be true of diphtheria and several other diseases. A disease caused by contagion or infection is distinctly caused by a special morbid poison; but there are many diseases which result from multiple causes, of which I may mention diphtheria, typhoid fever, and others. It is just this phrase which seems to have debarred Dr. Farre and Dr. Richardson from answering the first question in the affirmative; and it is just the idea involved in this phrase which seems to have caused much of the obscurity and ambiguity in its discussion. It implies that a given cause—a special morbid poison, if you please—which produces a distinct disease, must always result in this distinct disease. Is this the case with numerous other diseases? In a wealthy family belonging to my clientèle I saw, a year ago, a lady dangerously ill with pyemia, in consultation with Dr. Sayre, who was attending her for a severe traumatic injury. At our suggestion a careful examination of the house was made by a plumber, but no defect could be discovered which could explain the source of the blood-poisoning. Soon after, three of the family, one after another, were taken ill with a typho-malarial fever. I even insisted that the walls of the room should be torn down, when it was discovered that a leaden waste-pipe had defects which permitted noxious gases to permeate the house, but which were not sufficient to permit the escape of liquids to stain the walls. During the past winter I attended a gentleman with severe typho-malarial fever. His residence was in a large house constructed on the plan of the French apartments. This house was entirely occupied by families of wealth and refinement. During his convalescence diphtheria appeared. In another family in the same house I saw in consultation the first child attacked; afterwards the father, and two



other children, one of whom died, were attacked with the same diphtheria; and it is worthy of mention that the mother, who was removed from the house during the illness of the others with diphtheria, was confined with her fourth child under the care of Dr. Stone, one of our prominent physicians, and that her convalescence was not impeded by any puerperal disturbance either from diphtheria or typho-malarial or puerperal fever. On examination, it was found that the waste-pipes in this house were in precisely the same condition as the house I before mentioned. At the last meeting of the Society Dr. Playfair related some facts of recent occurrence at Notting-hill, where septicemia in the wife, from which she barely escaped with her life, and diphtheria in her husband, seemed to be due to the same class of causes as that which produced the disease in the case I have mentioned. I ask you to note the language used by Dr. Playfair in relation to these cases. He says, "Who can doubt that these two diseases were caused by the same septic poison?" One of these diseases he names, from his theory in regard to the cause of the disease, septicemia; the other, from its clinical phenomena, he names diphtheria. The idea involved in this phrase, "distinctly caused by a special morbid poison," implies that it results from only one cause, and that this cause must always be followed by identity of result—that if the poison is known to produce any distinct disease, then this disease must always follow the cause. The second qualifying phrase, to which I will simply allude and not discuss in detail, is, "distinct in its progress and the local lesions associated with it," as certain diseases specified. I have only to say in regard to this, that it seems to me that the several diseases mentioned differ in degree as to the definiteness of their progress, and as to the definiteness of their local lesions, and that consequently puerperal fever cannot be compared with these diseases as a group in these particulars. I will now give my reasons for believing that puerperal fever is a distinct disease occurring to women after childbirth only. I think the definition given in the nomenclature of diseases emanating from the Colleges of Physicians and Surgeons is absolutely perfect: "a continued fever, communicable by contagion, occurring in women after childbirth, and often associated with extensive local lesions, especially of the uterine system." It is a disease which results from the absorption of poison causing various blood changes, independently of local lesions, which produces certain general characteristic constitutional symptoms. I need not discuss this point in detail, for from what has been said it is evident that localism or Broussaisism has no status in this Society; and those who believe that this is only a disease resulting from septicemia and those who believe it is a continued fever are in agreement on this point. We can arrive at truth in medical discussion only by using language accurately. There can be no accuracy of idea without accuracy of language. The established usage of standard authorities in medicine is to designate as fevers all that class of diseases

which, from the absorption of some poison in the system, independent of local lesions, produce certain blood changes, and also cause certain general characteristic symptoms; so that under this designation are properly and legitimately included not only those diseases severally called typhus, typhoid, relapsing, intermittent, remittent, and yellow fever, but all those constitutional and infective diseases which occur epidemically and endemically. When the specific poison is known which causes these constitutional diseases as the result of blood changes, then the name of this poison is given to characterize the disease, and we have as accepted terms, uremia, septicemia, pyemia, and others of a like character. I will simply remark here that none of this class of diseases, so far as is known, ever occur as an epidemic or are contagious. The point that we are trying to settle is not a question of name, except so far as this: I take it that we are all agreed that the name given to a disease should be significant and appropriate. The gist of the matter, stripped of all its superfluous and obscuring elements in the inquiry, is whether there be a distinct disease which occurs in puerperal women, and in puerperal women only. The answer to this question can be derived only, as I believe all will agree, from three sources—from studying the causes, the clinical phenomena, and the pathological anatomy. I do not think that much information can be derived from studying the causes of this disease in order to settle the question under discussion, any more than there can be in settling the question from studying the causes whether typhus fever, relapsing fever, and diphtheria are distinct diseases. The study of the causes is of vast importance in a sanitary point of view, and hence I regard the paper which was read by Dr. Braxton Hicks before this Society several years since as a most valuable contribution to the subject. I think no one doubts that if a woman after confinement be exposed to the poison of scarlet fever, or that of any other infectious constitutional disease, she will have scarlet fever, or whatever disease she absorbs the poison of. I do not understand Dr. Hicks to claim that puerperal fever is only scarlet fever in the puerperal woman, but names this as one of the causes in the same category with diphtheria, erysipelas, mental emotion, and so forth. I think the evidence is conclusive to the vast majority of minds in the profession, established by numerous incontestable facts, observed not in sporadic cases only, but in numerous epidemics, and in epidemics which have not occurred alone in hospitals and large cities, but which have ravaged large tracts of country in sparsely populated rural districts, that the poison of erysipelas may cause in women after childbirth a distinct disease, which some of us choose to call puerperal fever, and that the poison of puerperal fever, if absorbed into the system of a man or a child, will cause in that man or child erysipelas. I need not refer to the medical history of Great Britain in proof of this assertion, but I perhaps may be permitted to refer to that of my own country for corroborative evidence. A reprint of "Nunneley on Erysipelas" was

published some thirty years ago, with notes by Dr. John Bell, of Philadelphia, in which he gave facts showing the intimate relationship between erysipelas and puerperal fever in numerous epidemics which prevailed extensively over the United States. During the past year a valuable monograph on erysipelas and child-bed fever, by Dr. Thomas C. Minor, of Cincinnati, Ohio, has been published, which I had the opportunity of reading on my voyage over. The book is based on a most painstaking and laborious study of the vital statistics of the Census of the United States for 1870 and an epidemic of puerperal fever which prevailed in South-Western Ohio in 1872, and I think it is worthy of careful perusal by all who seek to influence medical opinion on the subject of puerperal fever. A careful study of these statistics does not seem to show any intimate relation between typhus fever, scarlet fever, pyemia, septicemia, and puerperal fever. An epidemic of scarlatina was rarely followed by an outbreak of epidemic puerperal fever, but an epidemic of erysipelas was invariably attended by an outbreak of epidemic puerperal fever. The argument that puerperal fever is not a distinct disease rests mainly on the ground that it is simply due to a septic poison. Septic poison is a term now frequently used, and often very vaguely, but those who claim that puerperal fever cannot be regarded as a distinct disease do not claim that all disease which results from the absorption of septic poison must be one and the same thing, and that this should be called septicemia. Is there anything improbable or unphilosophical in the hypothesis that septic poison acting on the system in a peculiar state, which never occurs in the human system under any other conditions—that state so graphically described by Dr. Richardson and Dr. Farre in this discussion—should result in a distinct disease which never occurs except when the system is in a special condition? The answer to this question is to be found in the study of the clinical phenomena and pathological anatomy of the disease. If septic poison never occurs as an epidemic (I do not say an endemic) among those who are suffering from traumatism, and is never contagious under these circumstances, and if septic poison in women after childbirth does occur as an epidemic, and is contagious and infectious, are not these two elements sufficient to warrant us in regarding this as a distinct disease? Those who believe that puerperal fever is only septicemia have seen that they must accept this issue, and have been driven to deny that puerperal fever ever occurs as an epidemic or that it is contagious. If in a given territory women after childbirth die in proportion to the births in numbers fiftyfold or a hundredfold greater than they have died for several years previously, or than they die for several years following, would we not in common language say that an epidemic prevailed in that district? When, in four months of the year 1873, in that part of New York which is in the best sanitary condition of any part of the city, in which the most wealthy portion of the population reside, the number of deaths from a disease—call it puerperal pyemia, puerperal septicemia, or puerperal fever, as you



please—is twenty times greater than it had been for twenty-five years previously, and double what it is in other portions of the city in much worse sanitary condition, where the poor women are crowded together in their houses, and quadruple what it is at the same time in the lying in hospitals of the city, are we not justified in saying that an epidemic prevailed in the city at this period? When in Cincinnati in 1872 the deaths from this disease were 122, while the annual average of deaths from this cause for the five previous years had been 13 $\frac{3}{4}$ , can we not properly say that an epidemic prevailed in Cincinnati at this time? In view of all the material abounding in medical literature, and such facts as these. I will not borrow a phrase from one of the speakers at the last meeting, and say that those who deny that this disease occurs as an epidemic must have minds which are not open to conviction; but I will say that they must attach a subtlety of meaning to that word “epidemic” not consonant with common sense. Please to observe that I do not use the definite article, and say “the common sense;” otherwise the phrase might be discourteous. I will now submit the three following propositions:—1. The clinical phenomena of puerperal fever are quite different from those of surgical septicemia or surgical pyemia. 2. These affections do occur in puerperal women, and the result is a disease which does not constitute a continued fever communicable by contagion. 3. When either of these affections complicates puerperal fever it modifies the clinical phenomena by symptoms which can be distinctly appreciated and accurately described by any careful observer. I will not give the evidence on which these propositions are based, because I think that this has been done in a work on Puerperal Disease which it would be indelicate in me to speak of in more definite terms. I will say but a few words with regard to the pathological anatomy of the disease. Dr. Richardson argued, at the first meeting of this discussion, that there are no local lesions which will permit us to say of a case in a dissecting-room, “This is a case of puerperal fever,” as we would say, “This is a case of scarlet fever, or typhus or typhoid fever.” I think it would be extremely difficult to prove that there are local lesions which have established the fact that scarlet fever, typhoid fever, and relapsing fever are distinct diseases. I will not illustrate this by the subject of scarlet fever, for I find that my time is fast going; but I will simply ask, Would any of the most recent authorities on the continued fevers—would Sir William Jenner, or Dr. Murchison, or Hoffman, or Lebert—insist that a disease could not be registered as typhus or as relapsing fever, except certain anatomical lesions could be demonstrated in the dissecting-room as determining this disease? All these authors agree that these diseases, which we accept as distinct diseases, have no pathognomic anatomical lesions. The author of the address on Obstetric Medicine before the British Medical Association last year asserts that pathologists have torn in tatters the view that the disease which we are discussing is an essen-



tial fever peculiar to puerperal women, as distinct as typhus, or typhoid fever, or scarlet fever. When? Where? How? Give us the proof that any such pathologists have ever studied this disease, except in hospitals and large cities, where it is probably complicated with pyemia and septicemia. Of what value would be the researches of the ablest pathologists of Germany or England on questions relating to the pathology of yellow fever if such pathologists had no opportunity of studying this disease except in Germany or England? Has Spiegelberg, or Schroeder, or Schruenchberg, or Mayrhofer, or Orth, or Heiberg, or Olshausen, or Fehling, or Cohnstein, or Breisky, or von Haselberg, ever studied puerperal fever as it is found in epidemics in rural districts where pyemia and septicemia are rarely met with? German is a difficult language to acquire, and it gives an attractive appearance of learning to introduce such names, even if nothing be quoted from them. I am familiar with their writings, and would not undervalue their researches; but so far as the elucidation of the question now under discussion is concerned, I think it of equal importance that we should carefully study, for the clinical phenomena of the disease, your English classical writers—Hulme, Leake, Kirkland, Clarke, Gordon, Hey, Armstrong, and Robert Lee. The hint on this point thrown out by Dr. Farre at the last meeting seemed to me most appropriate and timely. What progress is made in science—how do “we free ourselves from error,” or gain in scientific precision or accuracy of description, in giving to this disease a new name—pyemia, as Dr. Duncan would call it, while he confesses that it has no etymological signification in this disease, and in fact has no definite, positive meaning? What propriety is there in giving to an obstetrical disease a name significant and appropriate to a surgical disease, unless it can be demonstrated that the two diseases are identical in their clinical phenomena, and their anatomical lesion? The burden of proof to show this identity belongs to those who would call the disease puerperal septicemia. Some think the negative has already been established. I have neither the time nor the voice to discuss the other questions which have been proposed. With my warmest thanks for your courtesy in listening to me so patiently, allow me to close with the suggestion that it may be well for all of us who discuss puerperal fever to remember the exhortation of Oliver Cromwell, when he lost patience with a Scotch Assembly, “I beseech you, brethren, by the mercies of God, conceive it possible that you may be mistaken.”

Dr. WEST: I came here to-night because I felt that honour implies duty, and having now for some years had the honour of being one of your fellows, I felt that if on the occasion of so important a discussion as this I stayed away, it would seem either as if I cared nothing about the exercise of my profession or about the furtherance of it, or as if some other motive—I know not what—had kept me away from your assembly. What I have to say I will say in as few words as possible. And first of all I have but one criticism

to offer, and that bears on one of the questions which have been suggested with reference to the share that certain microscopic organisms have, or may be supposed to have, in the production of the disease called puerperal fever. It appears to me, with all respect to the gentleman who in his able paper proposed that question, that it is somewhat out of time to raise it. We have not as yet a sufficient amount of knowledge on the subject to be able to apply it or to draw from the observations that have already been made any correct and useful inference; and no one, I think, can have read with care the very interesting discussion on the subject of the germ theory of disease without being struck by the way in which men of the greatest talent and the sincerest love of truth differ, not merely with reference to the conclusions that they draw, but also with reference to the very facts that they thought they had observed. I think, sir, if this matter is to be settled, it must be settled, in the first place, in the case of some disease simpler in its nature than puerperal fever. I pass, then, from that in these few words, in which I would wish still to convey the very high respect I feel for a kind of knowledge to which I can make no pretension, and to assure you that what I have said means no disparagement to the study carried on by means of the microscope, nor any doubt of the great results that hereafter we may arrive at from them; but I refer to it from the conviction that the question so raised is, in the case of puerperal fever at least, premature. I now approach the other and more practical questions. It may be allowed me, perhaps, to remind the Society of what I doubt not they have not forgotten—that some thirty-six years ago a physician, now taken from among us, but whose professional chair it was my honour for a short period to occupy, wrote a work on the very subject which we are now discussing, and I do not myself see that we have advanced far, if at all, beyond the conclusions which Dr. Ferguson had laid down, that the phenomena of puerperal fever depend upon a vitiated state of the fluids, and that in the condition of a woman in the puerperal state, such vitiated state of the fluids is specially apt to arise. I do not think, therefore, sir, that we can in any way assimilate the condition of what is called puerperal fever to typhus, typhoid, or scarlet fever. The disease wants those characteristics which more or less distinctly prevail in the case of the specific fevers. It has not the same regularity of course, the same regularity of incubation, the same regularity of outbirth. It has not the same diagnostic marks which we find in the case of the specific fevers, such as the pustules which appear, even though in small number, and characterize even the slightest case of small-pox. Nor is this all; but with reference to these fevers we know further that, for the most part at any rate, they produce a disease like to themselves, and that, though with many exceptions, they have a tendency not to recur where once they have happened. Now all these characters are absent in the case of puerperal fever; while the very circumstance that a far greater proportion of cases of puerperal fever occur in

*primiparæ* than in those who have given birth to many children shows that there is a marked connexion between the liability to an outbreak of the disease and the difficulties attendant on the process of labour. If, further, one exposes a man to the contagion of puerperal fever, he does not get any disease as the result of it. If we expose a woman in the puerperal state to the contagion of scarlet fever, measles, or some other disease, she may have that disease, even though its characters may be masked and altered in some degree by the puerperal state. But it does not necessarily follow that she will have that same disease. In the cases related by Dr. Collins, of Dublin, with reference to two epidemics of puerperal fever, the introduction of a case of typhus fever into the hospital was followed on each occasion by an outbreak of puerperal fever. So it would seem that a cause distinct from anything which we can identify as the special poison of puerperal fever, may yet, by the influence which it exerts upon the woman in this peculiar condition, produce a disease not distinguishable by any of its phenomena from that assemblage of symptoms which we ordinarily call puerperal fever. It seems as if the puerperal state was the condition of the development of this set of symptoms. Moreover, we must remember that there is not one single solitary cause to which we can refer the symptoms of puerperal fever; that it occurs now from one cause and now from another. I remember at this moment the case of a lady in a state of perfect health who was delivered at a time when puerperal fever was prevalent. She was a person in that position in society in which she was shielded from every possible danger. Soon after her delivery she was exposed to a current of air and caught cold. In a short time the symptoms of puerperal fever developed themselves, of which she died. Here was a case in which there was no special morbid poison existing—nothing but what could be derived from her own fluids or brought about by some impression on the system. Moreover, we know that mental shock will have the effect of producing the symptoms of puerperal fever, leading to a fatal issue. I remember the case of a lady who progressed perfectly well after an easy labour, but owing to some violent scene of domestic strife with her husband, she was seized almost immediately with symptoms of puerperal fever, at a time when no such disease was prevalent, and of that disease she died. I think, then, we are to look for the cause of puerperal fever rather to the condition of the person after delivery than to any special poison. Poisons of various kinds may produce it. We know that in Vienna puerperal fever was exceedingly prevalent, owing, as was discovered, in a great measure to the fact that the students left the dissecting-room and went with their hands imperfectly cleansed to attend the women in their confinement; and when greater care was taken, when careful ablution and disinfectants were insisted upon, puerperal fever diminished in its frequency to a very great extent. We know that local violence may produce it; in short, we know that a great number of causes may give rise to it, and that the only one



factor in the production of that class of symptoms consists in the special state of the woman after delivery. That state I need not recall. It is a state in which the fluids are laden with old, effete, and useless materials, out of which the old uterus is removed and a new one is to be built up; they are laden also with those elements out of which eventually the new secretion of milk is to be established. None of us, except those few who, like Dr. Richardson, have given great attention to these inquiries, can describe at all wherein the alteration of the blood consists, yet, without indulging in any wild theory, we may say that there is a something—people used to call it a ferment; I do not care what it is called, provided we use the name, not as defining the thing exactly, but as a name only to which we attach no other meaning than that of a something by which we are to distinguish this or that from the other, while as yet we know not its real nature. For instance, in the time of the old alchemists, calomel and corrosive sublimate, the chemical composition of which was unknown, were respectively called *Draco magnus* and *Draco mitigatus*; and if we use names as the old alchemists used theirs, we shall be much more likely to keep from the error of hasty conclusions or rash generalizations than if we use a name to which we tack a definite scientific meaning while as yet we have no sufficient grounds to rest that meaning upon. Those were the chief things which, it occurred to me, might perhaps be worth the saying. Take a woman in the puerperal state in whom is checked in any manner, whether by poison from without, by mental shock, by cold, or injury, the regular performance of those functions that ought then to go on, and you have the condition thereby produced out of which the assemblage of symptoms that we call puerperal fever—and all the world is pretty well agreed to give it that name—arises. The only other word I would utter is, to quote the saying of a great, wise, old physician, who said that the highest aim of our art must be the greatest possible generalisation of diseases and the greatest possible individualisation of our patients, so that we have in each case to look to the prevention of puerperal fever, not simply to the removing of the possibility of contagion, not simply most carefully watching the local condition of the uterus and removing from its inner surface any possible decaying matter, not simply to the taking care that the air that surrounds the patient is good, but we are bound also to consider the patient's mental state, to regard minutely her individuality, and, if we wish to put a stop to the disease, to remove from around or from within our patient anything which can interrupt those important processes the interruption of which gives rise to those symptoms which we designate by the name of puerperal fever.

Dr. SNOW BECK: I never had so strongly the feeling that I would have been glad to escape from the position in which I may have been placed as I have this evening, after hearing the very learned and eloquent addresses with which we have been favoured; but I feel emboldened by the sentiment with which Dr. Fordyce Barker con-



cluded, that possibly we may yet all be mistaken, to state the conclusion to which I have been led, chiefly from my own observations, upon the nature of the diseases which at present engage our attention. These observations have led me to a conclusion directly opposed to the opinions expressed by the previous speakers, for I am unable to perceive that there is any special or specified disease connected with childbirth, or to which the term puerperal fever can be properly applied. I must be also bold enough to contend that the definition published by the Nomenclature Committee of the College of Physicians cannot be maintained in practice, for I have never yet been able to make out "a continued fever, communicable by contagion, occurring in connexion with childbirth, and often associated with extensive local lesions, especially of the uterine system," which is essentially connected or associated with the condition of the system after parturition; or, in other words, which is not met with at any other period of the woman's existence. With reference to this definition, Dr. Arthur Farre informed us that the word "communicable" was only intended to convey that it might be communicated, but that it might also have a spontaneous origin, and the word "often" was only to call attention to the frequency with which these complications arise. Dr. Farre added that the Nomenclature Committee were entirely at a loss where the term puerperal fever ought to come in the catalogue of diseases. It was not placed among the fevers at all, but amongst the general diseases far away from the fevers, finding a place of refuge at last at the bottom of the list, after pyemia, erysipelas, &c. But why should the Nomenclature Committee be in any such difficulty if there really existed any continued fever, communicable by contagion, specially connected with childbirth? Whilst admitting this difficulty, however, Dr. Farre said he was not prepared to admit there was no such disease as puerperal fever; he should be sorry to shut out the idea as one that is impracticable, and not to be entertained. If we are to look for a puerperal fever at all—one that can be properly so called, a disease which is *sui generis*—we shall probably find it in the interruption to those healthy processes going on in the lying-in woman: first, the milk process, the milk secretion, and that attendant disturbance of the constitution which we term milk fever; and, secondly, a much more important one, the change which goes on in the uterus, and which we all know as the involution process. "Now we know," continued Dr. Farre, "that when this process is interrupted there is an arrest of all the eliminative action which the involution process implies; and we may fairly conclude that in consequence of this arrest there is some accumulation in the body of those effete matters which ought to be expelled from the system, and this, added to the existing blood dyscrasia, must very much aggravate the disease." But may it not be replied that this sounds very like admitting that up to the present time no such special or *sui generis* disease has been recognised? If such a disease be developed in the changes which take place after the birth of the child,

why has it not been found and described during the years of diligent study which so many of our most able and acute physicians have devoted to the elucidation of these diseases? and why is it that we have got no further than the unsatisfactory statement, that if we are to look for a puerperal fever at all we may probably find it in some arrest of these healthy processes? What proof, it may be asked, is there that in the serious and fatal diseases following childbirth there is any interruption of those healthy processes which go on in the lying-in woman—any arrest of this assumed eliminative action? Supposing, however, for a moment that such an interruption or arrest should take place, what proof is there that this arrestment would create in the system so serious a disease—a disease *sui generis*, and capable of being communicated by contagion? It must not be forgotten that the growth of the uterus is a healthy physiological process, that the tissues which are formed are perfectly healthy tissues. When and where have there been any facts observed which justify, or even countenance, the supposition that the retention in the system of any portion of a healthy tissue will generate such a disease that the majority of the women in whom it becomes developed will die? I am not aware of any facts upon which the least reliance can be placed which justify the inference that such a disease-creating action is even probable; and I must add that, instead of speaking of it as possible or probable that such a *sui generis* disease would be found if looked for, would it not have been more satisfactory, and certainly more conclusive, if the symptoms which would indicate such a disease during life had been given, or the appearances in the dead body described, by which its previous existence might be affirmed. So far as my knowledge extends, no reliable facts have ever been recorded which show that any such *sui generis* disease has been observed in practice, nor can I think that anything is gained by substituting the term post-partum fevers for that of forms or varieties of puerperal fever, for these terms are evidently intended to denote the same class of morbid phenomena, whatever their nature may be. In further considering these questions, I must contend that the process of childbearing is a natural function in the female, and that nature has provided her with all the powers requisite for the complete performance of this function as a part of her natural endowments. Every day's experience appears to exemplify this fact, not only in the human female, but also in the female of all other animals; yet, on the other hand, no one doubts that the peculiarities of the system after parturition modify to a certain extent any disease which may have existed previous to the confinement, or which may have commenced subsequently. But I am not aware of any observations which support the notion that there is anything in these conditions which, in a healthy female, is the source or element of disease, or which in any way can alter, or has altered, the essential nature of any disease with which she may be attacked. All women, however, are not in health when they become pregnant, and consequently are not in health at the birth of the child;

and many women are not endowed by nature with the constitution and formation favourable to the propagation of their species. Women in either of these categories must necessarily suffer from different diseases, and from various causes, after parturition; but the fault is here with the individual, and not with the natural and physiological process of procreation. But if it be true, as I believe it is, that there is not any disease specially connected with childbirth, it must not be forgotten that the converse is equally true, that there is not any disease from which the lying-in woman is exempt; in other words, that the condition of the system induced by pregnancy, or that which follows after parturition, does not prevent the existence of any other disease which may attend the female at any other period of her existence. Hence we must be prepared to meet with, in the lying-in woman, any of the diseases which flesh is heir to, and the diseases so met with become naturally divided into two classes—one class arising from the general system, and being the result of changes and accidents to which all women are liable at any period of their existence; the other class arising from the uterine system, and being the consequence of accidents incident to pregnancy or childbirth, yet not comprising any disease specially connected with either of these states. When speaking of these diseases, Dr. Farre observed that he had been in the habit of dividing what he termed post-partum fevers into three classes. First, those irritative fevers arising from some local irritation, and not implying blood infection of any kind, pyrexial states of a fugitive and transient nature, resulting from simple mammary irritation, slight injuries to the soft parts, laceration, and the like. Second, under the name of the milder forms of infective fevers would be classed all those in which the infection is not of a specified nature, in which the process does not undergo a period of evolution, a period of development, and in which the consequences follow no definite order. Thirdly, the eruptive fevers, and those which depend upon a blood infection, the poison following a specific course, having a regular period of incubation, and terminating in those several diseases, eruptive fevers, and the like, which occur of course in the lying-in woman in common with others. In glancing at the different diseases met with after childbirth, it may be well to do so under these heads. First, then, it would appear evident that those pyrexial states of a fugitive or transient nature, which are met with in almost every woman who has been confined, cannot be classed as belonging to the same category as those serious and often fatal diseases which follow parturition or abortion. We might as well include all the diseases in the nomenclature of diseases, for every one of these might, by possibility, be met with in a woman recently confined; yet it is singular how often these trifling and transient affections have been considered as the commencement or form of serious disease. Dr. R. Ferguson, in his admirable essay on Puerperal Fever, wherein he correctly attributes the most serious diseases to a vitiation of fluids, though the nature and source of this vitiation are not so clearly stated as perhaps they



might have been, remarks (p. 101) that a dose of salts-and-senna so invariably brought on metro-peritonitis that he forebore the use of this drastic irritant as a routine dose ; also (p. 16) that during a reputed epidemic of the peritoneal form, after two doses of Dover's powder of ten grains each the patients did not require any other treatment. Secondly, it is well known that the lying-in woman is often exposed to, and not unfrequently attacked with, the eruptive fevers or acute specific diseases. I believe that every one of these diseases has been met with at one time or another, and we cannot deny the proofs that they have been communicated by the nurse in attendance, and possibly, though very rarely, by the medical attendant. We all know that these diseases exercise a great influence, though in different degrees, upon the pregnant uterus. The rules under which women are received into the different lying-in hospitals when the pains of labour have commenced, and often during the night, give great facilities for the introduction of these diseases. Various instances are recorded where typhus and other fevers have been so introduced into lying-in hospitals, and have spread from patient to patient, constituting a so-called epidemic. It is recorded that on one of these occasions, after the introduction of a case of typhus fever into the Dublin Lying-in Hospital, "puerperal fever appeared in forty-eight hours afterwards, although the hospital had been free from it for a year before." But the disease which thus became developed was no doubt typhus fever, for evidence is entirely wanting to show that any of these infective or acute specific diseases have ever been known to change their nature. Some of the symptoms usually characteristic of these diseases may have been masked, or possibly absent, in some instances, but their specific nature has in no way changed. I cannot place any confidence in the statement that all these infective fevers "are excessively prone, if brought near the lying-in woman, to originate puerperal fever," or that, by some wonderful and potent influence in a woman recently confined, all these different specific diseases are transformed into one and the same disease. Dr. Braxton Hicks appears to draw some subtle, and to me incomprehensible, distinction between puerperal diseases and puerperal fevers, and has carried this transmutation of diseases even further than those who have preceded him. He imagines that a disease not only changes its nature when it enters the system of a woman recently confined, but when it again leaves her system and enters into that of another—to quote his own words—"it becomes reconverted into the zymotic form whence it had sprung." The experience, however, which has been adduced by the different fellows during this discussion appears conclusive that these eruptive fevers do not become changed in any respect when developed in a woman recently confined ; and I feel assured that Dr. Arthur Farre correctly stated the fact when he said, "I do not consider these diseases as in any way taking any part in the puerperal fever." Nor is there any scientific evidence, such as we have a right to require, that



the same cause can produce directly different results in different individuals—such, for example, as that mentioned by Dr. Playfair, where the same cause, included under the very vague term of “unhygienic condition” of the house, is said to have produced diphtheria in the husband and puerperal fever in the wife. I must admit to having a mind “not open to conviction” by such tales, where the effects supposed to have been produced are contrary to everything that has been observed in the course of nature or in disease, and where the statements rest only on the imagination of the narrator, unsupported by sufficient facts. Thirdly, all these diseases of the general system which have been adverted to are of minor importance, as only a small minority of them prove fatal. They evidently do not constitute that serious class of diseases which come on so insidiously, continue gradually from bad to worse, and so frequently end in death, that, as Dr. William Hunter said, “treat them in what way you will, at least three out of every four will die.” These diseases, which have invested the term “puerperal fever” with such fearful significance, arise from the uterine system, and are included in Dr. Farre’s second class, though I feel assured he has erred in supposing they are “infective fevers,” or can be termed “the milder forms of infective fevers.” They really constitute the serious, and too often fatal, diseases which are met with after childbirth. Let me sketch a case as illustrative of the class of diseases referred to. A healthy woman, after a natural confinement, with or without hemorrhage, remains very well for two or three days, then has a rigor, which is followed by a very frequent and feeble pulse, a feeling of great prostration and sinking, some pain across the lower part of the abdomen, with considerable tenderness when pressed, gradual swelling of the abdomen and tympanitic resonance, considerable thirst, some sickness, with loose, offensive motions. Gradually the patient becomes weaker and weaker; some shortness of breath, with slight cough and expectoration, is noticed; and she sinks after two, or it may be three, weeks without more marked symptoms. After death the peritoneal cavity contains more or less turbid serum; some soft, friable lymph is deposited on the peritoneal surface, particularly in the pelvis, with more or less injection of the peritoneum or the omentum. The uterus is large; when cut into, the tissues are healthy, the inner surface covered by a mucopurulent, somewhat reddish secretion; more or less pus-like fluid is found in the uterine sinuses, which are pervious throughout to fluid injected into the pelvic veins. What is the origin and nature of an illness such as this? A great variety of names have been applied to this condition, according to the view which authors have taken of its nature. It has been called peritonitis, puerperal peritonitis, peritoneal inflammation, metro-peritonitis, &c., under the belief that the effusion of turbid serum in the peritoneal cavity, the deposit of soft, friable lymph on the surface, and some injection of or under the peritoneum, were unquestionable signs of previous inflammatory action. But these morbid signs differ much from the appearances of undoubted

inflammation of the peritoneum, and are now recognised as exudations which result from the impregnation of the general circulation with some noxious fluid or material. It has been termed phlebitis, uterine phlebitis, suppurative phlebitis, in consequence of the pus-like fluid found in the uterine sinuses being considered unquestionable evidence of the previous existence of inflammation in these canals. Dr. Robert Lee has recorded several similar cases under the name of uterine phlebitis, and has described the pus-like fluid as suppuration resulting from previous inflammation; but it is extremely doubtful whether inflammation can ever take place in the uterine sinuses. These canals are only channels formed in the contractile tissue itself, and lined by a delicate and apparently non-vascular membrane. It cannot be supposed that inflammation could occur in this delicate membrane; and if it occurred in the adjoining tissue it would be inflammation of the contractile tissue of the uterus itself. There has been considerable difference of opinion expressed as to whether the pus-like fluid found in these canals was the result of inflammatory action, or had been taken up from without through the open orifices of these vessels at the inner surface of the uterus. But this question has been definitely decided by the injection of the perchloride of iron into the gravid uterus to arrest post-partum hemorrhage. Where death has followed in these cases the uterine sinuses have been found gorged with black grumous fluid containing an abundance of iron, and as this iron could not be formed by any secretion from these canals, it follows that it must have been taken up from the inner surface of the uterus where it had been deposited after injection. Cases similar to the one sketched have also been described as puerperal fever, the typhoid form of puerperal fever, low typhoid pneumonia, typhus, &c., when the symptoms presented during life have alone been taken into consideration. They have also been termed pyemia, septicemia, ichoremia, &c., from the symptoms and course of the illness being evidently similar to those which follow the introduction of noxious fluids into the general circulation. It is to be regretted that so much confusion has been introduced respecting the meaning of these terms; but so far as the present subject is concerned, it does not appear to be of much consequence which term is employed. Mr. Spencer Wells, in the opening address, says: "Supposing that the blood in the uterine veins clots, softens, breaks up, is the seat of chemical and vital changes, is detained in or near the pelvis, or is carried away to distant parts, or alters the composition and properties of all the blood in the body; that we have purulent infection or pyemia, putrid infection or septicemia; can all this arise in a healthy woman placed in favourable conditions if she be not exposed to some morbid poison?" Now the first thing essential to such changes must be the presence of clots in the uterine veins or sinuses, and as these clots have never yet been shown to exist, it is difficult to see how they could be the cause of so much that is supposed to follow. I am aware much has been said respecting the formation

of clots in the uterine sinuses after parturition, and that these clots mainly contribute, in many cases, in arresting any escape of blood; but I am also aware that the blood during hemorrhage does not come from these sinuses, that it is not a venous hemorrhage by retrogression, and that these clots, about which so much has been said and repeated, have never been shown to be present in the canals themselves. Much, I am also aware, has been echoed from one writer to another, as to the influence of hemorrhage in predisposing the system to those conditions known as pyemia or septicemia; but the only relation between uterine hemorrhage and septicemia is one of sequence of events. Want of contraction of the uterine tissues allows the blood to escape from the utero-placental arteries; the same want of contraction leaves the canals of the sinuses pervious, allows noxious fluids to be taken up through their open orifices, and to be conveyed into the general circulation. These facts appear to be the answer to Mr. Spencer Wells's question, Can all the consequences which he has depicted arise in a healthy woman, placed in favourable conditions, if she be not exposed to some morbid poison? There cannot be a doubt that they can arise, have arisen, and will again arise under the conditions named; indeed, it has always appeared to me one of the extraordinary circumstances in relation to this subject that such questions should ever have been entertained, for every one knows that isolated cases of the most serious affections, ending in the death of the individual, every now and then occur in all parts of the country as well as in large towns, with ladies in good health, placed in the most favourable conditions and surrounded by every care and attention that art can devise. If the succession of these cases occurring year after year be not sufficient to answer this question, I know not what evidence can be required. The same succession of cases appears to be an undeniable answer to the supposed influence of a vitiated atmosphere, of overcrowding in any institution, or the supposed benefits to be derived from cottage hospital isolation, &c. All that is required for the production of these serious conditions of the system is a means by which these noxious fluids can be readily conveyed into the general circulation, and this means exists when the uterus remains imperfectly contracted, or afterwards becomes partially relaxed, so that the canals of the uterine sinuses continue pervious, and any fluids formed at the inner surface, or which may have been injected into the uterine cavity, may enter at the open orifices of these canals, and be conveyed into the general circulation. This want of contraction, or subsequent relaxation, which practically is the same thing, may occur in the most healthy individual, living in the most healthy situation, and separated from actual or supposed injurious influences, whilst the simple means to avoid the whole of these evil consequences consists in procuring and maintaining such an amount of contraction as will render these canals impervious and prevent the passage of any fluids along them. When the want of contraction or the subsequent relaxation is considerable, a portion of the placenta is often



retained, or coagula form in the cavity of the body from the blood which has flowed from the pervious arteries. In these cases the sinuses also remain patent, and the usual noxious fluids—rendered more injurious by the decomposition of the retained placenta or coagula—are taken up in larger quantity, when the symptoms become more marked, and the course more quickly fatal. Whilst dwelling upon this cause of the serious condition of the general system, arising from the uterine organs after parturition or abortion, and which I am under the impression has been much overlooked, I do not wish to infer that it is the only cause, or that other causes may not induce similar results. Severe laceration of any of the parts concerned in childbirth, followed by inflammation—inflammation arising from long-continued pressure of the head forced into the pelvis,—the formation of abscesses in the cellular tissue outside the vagina or in the pelvis, and other accidents, may, any of them, produce the condition of septicemia; but, except in rare and exceptional cases, I cannot think that slight lacerations are followed by such serious consequences as have been attributed to them. Laceration of the perineum may exist at the same time as want of contraction in the uterus, but it is the want of contraction in the uterus which admits the septicemic condition of the system to be induced, not the laceration of the perineum. Once the way is opened to the system being impregnated with noxious fluids or materials, the effect which is produced greatly depends—(1) upon the nature of the fluids introduced, some being followed by no perceptible influence, others by serious and fatal consequences; (2) upon the amount, as in the experiments upon animals—a small amount producing disturbances from which the animal recovers, whilst a large amount always causes death; (3) the state of health of the individual at the time of impregnation—for what to one appears a poisonous dose, to another causes only general disturbance; (4) the continual renewal of the infecting matters. As Cruveilhier observed, the chief cause of the fatal result was that the purulent infection was incessantly renewed from the original source. Moreover, purulent infections have not the same decided course of action as the poisons, whatever these may be, which originate any of the infective fevers or acute specific diseases. And when we take into consideration the serious factors of disease which have been glanced at, combining together in very varying proportions in women recently confined, it is not difficult to perceive that we may often meet with combinations which puzzle our powers of diagnosis, and render it impossible for us to determine the exact nature of the illness. But because we are unable to unravel some of these puzzles, or to determine the precise nature of any complicated or obscure illness, is that any reason why we should rush into the regions of romance, or by concocting some high-sounding word or words imagine we have solved a substantial difficulty, or have explained a pathological entity? The question of contagion has been made the starting-point from which many have endeavoured to maintain the existence of a special



and essential disease occurring in connexion with childbirth; but it would be impossible to enter upon this question at present. Some of the acute specific diseases, after being conveyed into the lying-in hospitals, have spread from patient to patient, and have thus been mistaken for an apparent epidemic; but the reliable evidence is not sufficient to show that any other disease than these infective fevers or acute specific disease has been propagated, either epidemically or by contagion. Most if not all of the recorded histories on this point appear to admit of another explanation, and many to have their foundation in the management of the woman during the period of parturition, or in the circumstance that some minute yet essential particulars in the history of each case have been overlooked in the course of the illness. To quote the words of Dr. Fleetwood Churchill, "I rather think these [accounts] prove too much, for *post hoc* is not always *propter hoc*;" and, so far as bacteria are concerned, these bodies, we are told, are found in the interior of the organs in every dead body, from whatever cause the death may have arisen, their development being only a question of time and temperature.

Dr. W. T. GREENE: Assuming that the object of the present discussion is to clear up what has hitherto been confused and doubtful in post-partum pathology and therapeutics by a comparison of the records of individual experience, and not to establish any particular theory, I trust I may not be thought too presumptuous for desiring to bring before you and the Society the result of some ten years' observations extending over nearly two thousand cases, of which a proportion of about 10 per cent. suffered in a greater or lesser degree from one or other of the various affections to which parturient women are peculiarly liable, and which have so frequently been confounded together under the general name of puerperal fever—a disease of which I had for some time doubted the existence, but which I am now satisfied has yet to be differentiated as a malady *pet se*. So far from post-partum febrile affections being identical, I have observed that they invariably assume one of three distinct forms, differing essentially in their origin, progress, duration, and gravity; in other words, the exciting cause may be local and transitory, local and persistent, or altogether extraneous. In the first two forms the disease may be, and often is, autogenetic, but in the third this is necessarily impossible. Firstly, very alarming symptoms may be excited by the presence of a decomposing clot in the uterus or even in the vagina, or by an overloaded state of the bowels, but yield rapidly to an aperient or the use of the syringe; and I may be permitted to observe it is unpardonable in an accoucheur who fails to have recourse to such simple remedial measures on, or even before, the first appearance of the enemy. Some time since one of my patients on the fourth day after a comfortable delivery had rigors, and when I saw her had a pinched, suffering expression of countenance, considerable abdominal tenderness, suppression of the lochia, recession of the mammary secretion, high temperature (104°), rapid pulse (130),

and in fact all the symptoms of so-called puerperal fever ; but she also, as I was informed by the nurse, had begun to suffer from diarrhea, which, on investigation, turned out to be the ineffectual efforts of nature to relieve the loaded intestines. I at once washed out the vagina, as there was rather more odour present than was usual, with a diluted solution of chloride of lime, my favourite antiseptic, and ordered five grains of calomel. Next morning, though informed by the nurse that she was weak and sinking, I found my patient wonderfully improved in every respect, but very angry with me because the powder I had prescribed had, as she said, purged her instead of stopping her diarrhea, which, however, had then ceased to be troublesome. I ordered nourishment and a due proportion of stimulants, and she made an excellent recovery ; whereas, had I acceded to her wishes, and checked instead of promoted the alvine discharge, she would in all probability have died ; for I have, I believe, in one or two instances, seen a patient lost because the attendant had an unreasonable dread of cathartics. Secondly, a foul, ill-ventilated, crowded, or over-heated room, by promoting putrefaction of the lochial discharges, will often give rise to septicemia, and, unless the cause can be removed, to death, which the most earnest efforts on the part of the medical attendant will, in such a case, be utterly powerless to obviate ; or hysteritis, rapidly involving the adjacent parts, may result from injury to the uterus, by long-continued pressure of the fetal head in tedious labours, by the unskilful or incautious use of instruments, or by the well-meant but frightfully injudicious attempts of the accoucheur to pass the swollen lips of the os uteri above the head of the child during the continuance of a pain—a very common practice, and often, I am convinced, productive of extremely disastrous consequences. A primipara, aged thirty-three, who had suffered a good deal throughout her pregnancy from sickness and general debility, was delivered naturally after a favourable labour of a healthy living child, and progressed fairly for the first couple of days. She was nursed by her mother-in-law, who persisted in keeping the room at stove-heat. On the third day the patient had rigors, and severe symptoms soon set in, but subsided on the arrival of a more intelligent nurse. Unfortunately this person was soon called away ; the mother-in-law became once more in the ascendant, stove-heat was renewed, all my orders disregarded, and the patient died on the tenth day after her confinement, with every symptom of blood-poisoning. I might multiply instances, but *cui bono ? Ab uno disce omnes*. In every such case—that is, ending in the death of the patient—I have no doubt (and this constitutes the essential difference between what I may call the first and second forms of post-partum febrile affections) careful inquiry would elicit the fact that the woman's health had been for some time in a more or less unsatisfactory condition, and to that cause, rather than to any accidental lesion, the unfavourable ending of the case must be ascribed ; just as we not unfrequently find in the other sex, where the health has not been all

that could have been desired, the most trivial injuries giving rise to septicemia and death; otherwise nine-tenths of all women must sink after their confinements. In the third form, if I may so term it, of post-partum fevers, which comprises at least six-eighths of all cases, the contagion is conveyed to the patient from without, in the shape of germs of one or other of the zymotic class of diseases, by the medium of some one—doctor, nurse, or friend—recently in attendance on a person suffering from the same complaint; and of this fact I have had positive proof, although the possibility of its occurrence has been denied, and it has been even attempted to be proved that because some women resist the contagion none can suffer by it—a mode of reasoning, however, undeserving of further notice. Of all the exanthemata, scarlatina, in my experience, most frequently attacks the parturient female, and though giving rise to very alarming symptoms, is happily rarely fatal. Some years ago, during the earlier part of my professional career, I was attending a lad with scarlatina anginosa of a severe type, and at the same time nearly all my puerperal cases exhibited febrile symptoms and had more or less sore-throat, although the characteristic rash did not in every instance make its appearance. Several of the children sickened as the mothers were recovering, and exhibited every feature of scarlet fever. Fortunately none of them—parents or children—died, but I received a lesson I shall not soon forget. Since then, at different times, I have met with other similar cases, and have always been able to trace the contagion to its source. Post-partum small-pox I have met with in two instances where the preliminary symptoms were identical with those usually ascribed to puerperal fever. In both of these the patients recovered. Of post-partum measles I have had no experience, but I have met with several cases of erysipelas occurring in recently-confined women, who all succumbed to the disease. In one of these cases I was able positively to trace the disorder to its source. The nurse had come straight from the bedside of a man suffering from phlegmonous erysipelas, of which he afterwards died, to attend upon my unfortunate patient, in the raw placental surface of whose womb the insidious disease found too ready a nidus, and thence spread rapidly to all the adjacent parts, terminating fatally on the tenth day after the poor woman's confinement. The infant also died of the same complaint, thus confirming the diagnosis in the mother's case. I have had one case of post-partum typhoid fever, occurring soon after delivery in consequence of the patient inhaling the noxious emanations of a drain under the window of the breakfast-room in which she had been confined. After a hard struggle for her life, this woman recovered, but her infant died with every symptom of typhoid fever. If it be asked why scarlet fever, &c. assume so formidable a character in parturient women, I reply that in them the absorbents are in the highest state of activity, and the poison in consequence is conveyed with extreme rapidity to all parts of the system, and produces a correspondingly grave effect. With regard to the *animalcule* which by some are

supposed to play so important and destructive a part in septicemia generally, and notably in the case of parturient women, and by others to be a delusion and a snare, I cannot boast of any great experience, but I can imagine that their presence within the body would have a tendency to cause increased irritation, just as maggots in an open wound would certainly retard if not prevent its closing. Hence the value of antiseptic lotions, especially the chlorides, which are fatal to bacteria and their congeners. To sum up, there is no such disorder as puerperal fever, and therefore the erasure of the term from the nomenclature of diseases is very urgently demanded, as utterly misleading, and in lieu thereof might be advantageously substituted the name of the complaint from which the patient had suffered, with the qualifying epithet "post-partum," thus:—Post-partum septicemia, post-partum scarlatina, erysipelas, &c. ; and a little care in analysing the symptoms and the history of each case would almost always render the diagnosis absolutely certain.

Dr. ROUTH: I am anxious to make a few remarks in this discussion, since in early life it occurred to me to have seen perhaps a greater number of cases than any person in this room, having been present during part of that terrible epidemic which took place in 1846 and part of 1847 at the Vienna Hospital. During the first of those years 456 women died of the disease, and in the second 176. The greater number of those cases I saw, and therefore I had an opportunity of studying the disease very closely. On my return to England I published a paper, which is extant, and is probably known to many members present, therefore I shall only refer to it, in which I showed that the disease was communicated directly by inoculation from the hands of the medical students who came from the Coroner's Court, and satisfying themselves by simply washing their hands in a little water, made examinations of women about to be delivered. One point in regard to the disease was clearly brought out—namely, that it was not contagious from one person to another. That fact I took a great deal of trouble to find out, and I think it was proved in the most satisfactory manner that the disease was not communicable from one person to another, except the discharges were in some manner communicated, or except in the case of examinations made by the students under the circumstances I have mentioned. Many of the nurses and midwives who attended those patients were married women, and many of them had children, but not one of those women caught the disease. In the division where the labours were attended by women who had nothing to do with dead bodies, the disease did not exist, or if it existed at all the cases were so few and far between that there was no belief that it could be produced in any way by the attendants. And after the plan had been adopted of compelling every student to wash his hands in a solution of chloride of lime before he attended a midwifery case, the mortality in the first division, where about 3500 women were annually confined, diminished exactly to the same figure as in the second division, where a similar



number of women were attended by midwives only. Well, that disease I showed from the evidence was not contagious from one person to another. That is exactly what is true of surgical fever. We all know that very often we communicate a disease to a lying-in woman if we come from a case with our fingers contaminated by some discharge. The records of medicine abound in cases of the sort. The cases published by Dr. Storr some years ago in the "Transactions of the Provincial Medical Association" showed how often the disease was produced in that way. Therefore I say it is clear from the evidence at Vienna upon a large scale, and from what I have seen and read upon a smaller scale in my own practice and that of others, that one variety of puerperal fever can be and often is communicated by direct inoculation of the practitioner. In the case of the fever at Vienna the children died generally a little before the mothers, and in all those cases it was found that peritonitis existed in the child. In Dr. Storr's cases it was found that many of the husbands of these women contracted peritonitis; and we have heard to-night of some cases in which the disease communicated was erysipelas. Therefore we must admit that under certain circumstances there is some contamination that takes place between parties coming in close contact, probably analogous to that which takes place with typhoid fever, which may be said to be communicated by discharges or by the powerful odour emanating from the patient. No man can read all the literature upon the subject, I think, without being convinced that there is another variety of puerperal fever which is eminently contagious. Whether it be communicated from erysipelas direct, or from typhoid, or from any of these diseases, it is communicable from woman to woman, even from woman to man. There is, I think, a point in which we have not made a clear distinction. There is indubitably a peculiar point in the contagion of several different fevers which, when they attack a lying-in woman, will develop in her a particular disease, call it puerperal fever, or call it a modification of the disease which originally infected her, but which, applied to another woman, would extend, and thus produce an epidemic. If we look at that question on a broad basis, we must come to the conclusion that some cases are not contagious and some are, and that those that are contagious are communicated by infection from other cases.

Dr. GREENE: I may mention a fact which I omitted to state, that a midwife, whose son was suffering from erysipelas, communicated puerperal fever, or some febrile affection, which undoubtedly was erysipelas, to a number of her patients, many of whom died. That occurred recently.

Mr. WALLACE: I am pleased to hear what Dr. Routh has said about the contagiousness or non-contagiousness of some cases of puerperal fever. Up to last October I was in the happy position of having attended over two thousand cases without having lost a woman from any disease whatever; like some of those gentlemen who write to the papers and say they have attended so many cases without a

single death, and therefore think they are never to have one. However, my turn came at last. My locum tenens attended a primipara on October 5th. The house was in a bad hygienic condition, the rooms being ill-ventilated and most offensive, and the drains smelling. The woman died after six days of what I should call typhoid fever modified by the puerperal condition. The most singular part of the case was this. I should explain that I came back from the country the very night the woman was confined, and I attended her afterwards myself. During the first two days after my return I attended four women in their confinement, and they all did badly; none of them died, but they were all more or less ill for some time, and two or three of them barely escaped with their lives, so much so that I was compelled to give up midwifery practice for three weeks, when my troubles ceased. About two months afterwards a medical friend of mine attended a case for me, in which the woman died after five days of metro-peritonitis. While this woman was ill I attended several cases, and they none of them exhibited any bad symptoms whatever. I continued to attend midwifery cases, and all my cases did well; but after two months there was another case where the woman died. I may mention that in the second case the woman had a dead child, and there was decomposition of a portion of the placenta. What I wish especially to say is, that what would be called the pyemic cases were non-contagious. I may also state that during 1870 in my neighbourhood there was an epidemic of scarlet fever. I had many cases to attend, and I also had a good deal of midwifery, but none of the women were taken ill. On several occasions children with the rash out were in bed with the mothers when they were confined. The same has been the case with measles. During the epidemic of small-pox which occurred in my neighbourhood in the East End of London in 1871 those puerperal women who took small-pox had small-pox simply, not at all modified by the puerperal condition.

Dr. GRIFFITHS: In the discussions which have preceded to-night's there seems to be some discrepancy as regards the relation of scarlet to the so-called puerperal fever, which, with the greatest diffidence, I submit may be explained in the following way:—Some gentlemen have seen and described true scarlet fever running the regular course, the subject of it happening, in the chapter of accidents, as it might be said, to be the lying-in woman in whom that fever which we know by the name of puerperal fever was not induced. Other gentlemen have seen after labour symptoms which they have considered scarlatina, but which really do not constitute that affection, being simply the outward and visible signs of the inward autogenetic poison of which—as, indeed, the word “autogenetic” implies—the woman is herself the factor and subsequently the recipient—recipient not alone because the poison generated in utero lies in or is received by the uterus, but because it is received into the lymphatics, the veins, the peritoneum, and into the body generally, as described by Dr. Tilt in his paper on lymphangitis in *Pelvic Pathology*, in the sixteenth

volume of our Transactions, and alluded to by him in his recent speech. With the train of symptoms to which I refer, and which has been termed scarlet fever, who practising obstetrics has not had experience of it in cases of what, for the sake of distinction, we must term puerperal fever? But, sir, is it scarlet fever at all—at least, that form contracted from another affected with the ailment, and which, in the usually accepted meaning of the name, constitutes the true disease? The conditions alluded to, and which are identical with scarlatina,—nay more, are in some instances, perhaps, the same—are the result of pyrexial action, the indication of latent mischief, and have their origin, not from scarlet fever contagion—that is, from a scarlatina-infected person, the woman never having been exposed to the scarlatina virus—but from the presence of some putrid or offending matter in the uterus or vagina. The sickness, the headache, the hot, pungent skin, the sore-throat and rash, congested eyeballs, the jaundice, the delirium, the oppressed heart and breathing, and, later, the diarrhea: these are not scarlatina, in the usually accepted sense of the word, but a group of symptoms pointing to the toxemic condition of blood dependent not upon the entrance of a specific virus, but simply of the noxious elements resulting from putrescent animal matter—are, in fact, the *sequelæ* and manifestations of the blood-poisoning which occasions what is termed puerperal fever, and which, if not checked or corrected, would immediately end fatally. In a scarlet fever epidemic, such happening to a lying-in patient would be set down to the scarlet fever poison; but I have seen them where no scarlet fever at all existed, and where no exposure to it had taken place. Those gentlemen who have seen scarlet fever attacking the lying-in, and running its usual course, well-defined, without inducing any form of puerperal fever, would do good by relating whether the lochia were offensive before the scarlatina set in, or at any time during its progress. If not before the invasion of the fever, then I say the case is one of true scarlet fever; but if the discharge become offensive only in the course of the scarlet fever, I would submit that the fever poison, perverting, as we all know it does, the entire blood, has vitiated, and it may be even rendered putrescent, the outcome of the blood—I mean the lochia; the relation of the scarlet fever being to the offensive discharge as cause to effect, the reverse of that which obtained when the symptoms exist which I have demonstrated as being those of so-called puerperal fever, and to which we might apply the term bastard, or simulating scarlet fever. Those gentlemen who hold that scarlatina is a great agent in producing puerperal fever, would do well to tell us whether the scarlet fever symptoms came on before labour. If not, on what day after delivery? They should also state whether the lochia were fetid. If they were, how many days previous to the scarlet fever symptoms had they been so? I am led to the conclusions I offer by a case of autogenetic poisoning at this moment under my care, in which most, if not, indeed, all the symptoms of scarlatina obtained, but which, recognising to be wholly the

consequence of a morbid condition of the uterine system and its excretions, the lochia, I attacked by imitating the eliminative action of involution, grasping the soft, spongy, uncontracted uterus with my hands, compressing it and expelling some clots, washing out the putrid or putrescent discharges of the womb and vagina by means of Higginson's syringe, Condy's fluid and warm water being used every two hours till the lochia became quite inodorous. Had I, however, seen the patient for the first time when those apparently scarlatinal symptoms were well developed, I might have erred in supposing I had a case of scarlet fever to deal with, have treated it as such, and lost my patient; but being alive to the puerperal facts to which I have drawn attention, I at once proceeded as I have described, and the correctness of the diagnosis was evinced by the immediate improvement in my patient, commencing as it did from the very time I got rid of the offensive clots and washed away the foul lochia. All the group of evils in this case might be termed scarlet fever, yet all at once disappearing under the defouling treatment named showed incontestably that the diagnosis was correct. Moreover, since her recovery I have made inquiries, and find no exposure to scarlet fever, nor any presence of it in the town. One difference I noticed: there was no desquamation of the skin, although I should not have been surprised had such taken place, so high did the temperature run. I admit—as Dr. Savage, I think, said—there may be cases of septicemia running to a rapidly fatal issue without any of the pyrexial disturbances I have named, or even any elevation of temperature from first to last; but that is where the patient is stricken down and overwhelmed by the dose of poison, or where the febrile symptoms occur in our absence, and hence may escape the observation of even the astutest. The same remarks will hold good of the rash as adduced by Dr. B. Hicks. I am led up to these conclusions because lately my mind has been exercised upon what I consider to be a fact—namely, that scarlet fever and typhoid may originate in the same way, from the same infecting source of poison developed in different persons or in the same persons at different times and under different circumstances; the poison at one time, determining to the lymphatics and glandular systems, producing what we know as scarlet fever, at another time determining with greater virulence to the intestines, particularly Peyer's patches, as in typhoid fever. In this—although I had arrived at my goal without knowing the way to it had been trodden before me—I find I am borne out by the previous researches of Dr. John Harley, which he published in a pamphlet he has had the kindness to send me since I commenced my investigations, also by the researches of Dr. Tilt, made known to us in his paper on Lymphangitis in Pelvic Pathology, by Dr. Tilt's speech, by the speeches of those who have preceded him, and by my own at present somewhat crude experience, which I value very slightly indeed till I verify and check it by further careful inquiry. Bearing in mind, therefore, the life symptoms which exist in scarlet fever, puerperal fever, and



typhoid, also the pathological conditions found after death, and how closely allied they are—nay, more, how much alike in a number of instances—we can see how readily the first—that is, scarlet fever—can run into the second, puerperal fever; the second into the third; how typhoid came to be supposed to originate puerperal fever (Sir Henry Marsh's opinion, as quoted by Dr. Squire); and how true so-called puerperal fever may clothe itself in the garments of scarlet fever, so that only with difficulty it can be distinguished.

MR. SPENCER WELLS: The great difficulty I have in replying to-night in the few minutes that remain before the end of the discussion, consists principally in the great number of speeches that have been made, and the extremely numerous and important topics which have been suggested by different speakers. But I think I may congratulate the Society upon the very great importance of the discussion that has been carried on, and upon the very great ability which has been shown by a great number of gentlemen who have addressed us, and by none more so than by our friend from the other side of the Atlantic, to whom we have all listened with so much pleasure and instruction to-night. I will, with your permission, while what our friend has said is fresh in our minds, refer to two or three topics which he has raised, inasmuch as that will perhaps prevent the necessity of a good deal of repetition in discussing the first three questions which were placed before the Society in the little programme by which I endeavoured to give a direction to the thoughts of gentlemen who were going to speak. There was really nothing in these questions intended in the way that Dr. Farre supposed to lead to any very definite conclusion, or to any kind of suggestion that I myself had formed any decided or positive opinion upon them; it was more with the hope that different gentlemen would take up the different questions according as their opportunities gave them means of information, and would bring before us the results of their own individual experience upon these different questions. Dr. Barker has at once started the question by his statement that we must, as he says, from the identity of cause presume identity of result, or at any rate that if we have a specific morbid poison to deal with, that specific morbid poison always produces the same results; or, as I think I understood him to say, that I might, from the same specific morbid poison, get very different results in different women under different conditions. For instance, given a morbid poison, and that morbid poison received by a healthy woman, it would produce in her a very different condition from what it would produce if received by a puerperal woman, and by the puerperal woman if she is in an unhealthy condition at the time. I do not know that I exactly interpreted his meaning, but if I did, I should like to say that I would go somewhat beyond that, and assert that if you have a specific morbid poison it must, under all conditions, produce identity of results. I do not believe if you inoculate small-pox in a patient that you can produce a chancre; I do not believe if you inoculate a chancre in a patient that you can produce

hydrophobia ; I believe you can certainly produce identity of result from identity of cause in the animal body as you do in the vegetable. If you sow a grain at the proper season in a fruitful soil the seed will germinate ; if you sow a grain of wheat you do not get barley or oats. The seed germinates, and you get the same variety of vegetable produce. It is the same, I believe, with animal poisons. If you sow a seed of hydrophobia, or a seed of syphilis, or a seed of small-pox, in a patient, you get a crop of small-pox, a crop of syphilis, a crop of vaccine, or a crop of erysipelas, according to the poison you have sown. It is the same with medicine : if you give a dose of castor oil, you do not send the patient to sleep ; if you give a dose of opium, you do not produce purging. All these poisons produce the same result. So with puerperal fever : if you suppose that in puerperal fever you have a specific disease different from any other fever, or from any other condition which we meet in surgical or medical practice, you must show us some cause which will, under all conditions, produce the disease just as certainly as you produce small-pox by inoculating small-pox, or scarlatina by exposing a patient to poison of scarlatina. That carries me to the question which Dr. West started, as to what share bacteria or some other form of vegetable or animal life in its lowest form may have in producing these diseases, or carrying the poisons to produce them. I agree with Dr. West that we are only on the threshold of this inquiry, and the question which I put before the Society was simply what relation have bacteria and allied organic forms to the pyemic process in the puerperal state, in other words, to puerperal fever ? I put that question in hopes that Dr. Burdon-Sanderson, or some other gentlemen who have lately made such important investigations on this subject, who have investigated the subject of splenic fever, sheep-pox, relapsing fever, diphtheria, and erysipelas, would have come here, and given us the benefit of their experience. The great use of this discussion has been that it has brought here, not only the ordinary Fellows of the Society, but others from distant parts—a professor from Glasgow (whose candour and ability excited universal admiration amongst us, and a hope that some day he may occupy a still more important post than that which he now fills so creditably), some of our past presidents, several of our honorary fellows, gentlemen from the provinces practising obstetrics very largely. It has also brought a distinguished physiologist, who is among us to-night, and who I was in hopes would have spoken, who has done more, perhaps, to prevent disease in the country than any other man—Dr. William Farr ; who, by making known the incidence of diseases and showing their causes, has done more, perhaps, to check preventable disease than any one else. I hope that at some future time he will favour the Society with some statistical account of the prevalence of this fever in different localities. We have been favoured by a promise from Dr. Farr of another paper on the subject, so that, at any rate, the discussion has not been fruitless in good to the Society, and I trust that it will be beneficial to the profession at

large. We have also been favoured by Dr. Farr with an account of a recent epidemic of puerperal fever in a district in the south of London. It is only for the week ending the 19th of June. In the printed returns of deaths it was stated that the cause of death was given as peritonitis and fever, which were presumably cases of child-bed fever. There were three cases in the week and one in the preceding week, which, it was found on investigation, were all attended by one medical man. Between the first case and the other three he attended two cases without a fatal result, so that the first case was probably the cause. It has not yet been quite ascertained how the first case began—whether it was a case of ordinary peritonitis or not. The investigation is still being carried on. Scarlatina is still prevalent in the district. In the same week there were nine or ten deaths registered. An interesting question arises, and is being investigated, whether the two women who escaped from childbed fever between the first and third cases by this gentleman were protected by previous attacks of scarlatina. That is a most important matter. It is still being investigated, and I hope Dr. Farr will let us know the results. I have one or two letters here showing the very close connexion of scarlatina with puerperal fever, which it will, perhaps, be interesting to the Society to read. One is from Dr. Jackson, of Notting Hill. He gives the particulars of a case that occurred in September, 1864. The husband, about thirty, had a severe attack of scarlet fever, from which he recovered. The wife, who had some years previously had the fever, and who was in the end of her eighth month of pregnancy, waited upon and attended her husband day and night. In about fourteen days from the date of her husband's illness she began to feel ill, shivering and feverish, but had neither sore throat nor eruption. Labour suddenly and sharply set in. The child was born alive, well developed and strong, but covered with the distinct and characteristic scarlatina rash. From birth it was restless and uneasy, and died on the third day. The mother progressed most satisfactorily, and the feverish symptoms abated. Milk and lochia were well established. Some slight apparently dyspeptic symptoms were complained of at the end of a fortnight, but these gradually disappeared. The cuticle on both hands desquamated, and at the end of the fifth week she was quite well, and left town for the country. I have several other letters giving instances of the close connexion which Dr. Barker has alluded to between erysipelas and puerperal fever. One is a letter from Mr. Freer, of Stourbridge:—"Some years since I left home for a few days' holiday, leaving two patients expecting their confinements. . The evening before I started my father was thrown from his horse upon the front and right olecranon. The next morning the arm was red and swollen. Two days afterwards he attended both my patients, who had easy, natural labours, not requiring any assistance, and my father was not half an hour with either of them. Upon my return on the fifth day I found both my patients dying, and my father's arm so much inflamed that I had to make incisions from the elbow to the wrist. Each case had



rigors in twenty-four hours. Ten years since I was engaged to attend the wife of a clergyman in her first confinement. She was a very fine, healthy lady, aged twenty-six. Upon entering the bedroom, I found a nurse in attendance with an erysipelatous blush and swelling upon the side of the face. Upon inquiry she told me that two days before she had been in a Liverpool hospital to have the nasal duct opened. My patient was seized with rigors at the end of thirty hours, and died of puerperal fever on the eighth day. The nurse died of rapid erysipelas of the head and neck on the twelfth day." I have also a letter from Mr. Pemberton, of Banbury, which I need not read through. It goes to show that in a house where there is typhoid fever a woman being confined may get puerperal fever, but then I do not think at all that it follows from that that the cause was the same; for you may from drains get a variety of poisons, a variety of chemical and organic products thrown out, and one patient may get one and another another. You may get sulphuretted hydrogen, phosphoretted hydrogen, and a variety of chemical substances, and vegetable or animal organisms of a low form, and one may affect one patient and another another. Showing how this bears upon surgical practice, and somewhat in answer to the question which Dr. Playfair started, I will read a letter from Dr. Keith, of Edinburgh, giving me an account of a most remarkable series of cases, in which erysipelas, puerperal fever, and septicemia after ovariotomy seem to have been in close association. "On the 19th of May my friend who assists me at my operations became uneasy for the first time about a puerperal case. The patient died on the 26th. On the 24th he helped me at an ovariotomy, but did not see his puerperal patient till after the operation. Next morning my patient was in a typhoid state, and had bad bedsores with all sorts of eruptions and superficial abscesses, and recovered after this pyemic condition had lasted a month. There was no suppuration in the pelvis. On the 30th, he assisted me at a tedious ovariotomy. The patient was quite well for six days, the pulse and temperature natural. She then suddenly shivered, the pulse rose to 170 and the temperature to 106° within a few hours, and she died five days afterwards. On the same day he opened a small superficial abscess in a lady's neck. She went after this to the country, was quite well for five days, then shivered, and died five or six days afterwards of erysipelas of the head. On the 6th of June he attended a simple case of natural labour in a healthy woman. She shivered next day, and died on the 10th of septicemia. As he went home from this confinement he himself shivered, and was laid up with erysipelas of the face, from which he recovered. The medical man who attended the last puerperal case during the few days she lived examined for an instant a patient in the first stage of labour, four days after the death of the last case, but declined to attend her. This patient had metritis, and is now recovering, the veins of the left limb being all plugged up." I have seen this kind of thing myself in surgical practice, and this led me to be extremely careful in getting the gentleman who attends the operations at the Samaritan Hospital



to sign a paper before going into the room that he has not attended an infectious case for seven days, and has not attended a post-mortem examination during that time. I believe that a precaution of this sort would really very much protect puerperal women from many of the infectious diseases to which they are subject, and which, I believe, are as scarlet fever, or as measles, or as small-pox, or as erysipelas, confounded together under the name of puerperal fever. I hope the end of this discussion will be to give more accurate names to the different conditions which have been confounded together under that one name. I should like to say a single word as to the great question of what is called homicide by infection, and the attempt that has been made, I think unfortunately, to enlist the sympathies of this Society in the protection of careless and ignorant women who deserve the punishment they have got. I think if a woman is cautioned by her medical man, who tells her that she has attended a woman in her confinement who has died of puerperal fever, and that if she attends other women she is liable to poison them, and if she goes on in defiance of this caution to attend other women, and says, "Well, if this one dies, I will give up," such a woman is not deserving of the sympathy or help of the Society. I think we should do good service if we rather stimulated the fellows of the Society and the midwives that they employ, not to carelessness, but to additional caution, teaching them not only to wash their hands, but to use disinfectants, and above all things to avoid any possibility of doing damage to patients who employ them. Only this day I heard of a case which shows that a practitioner would do well to err on the side of over-caution. I was asking about a gentleman whom I knew some twenty years ago, who had settled down in practice in a large town. He was an able man, a gentleman in every sense of the word, a person of good character and great ability. I was told that he did not get on. I asked why, and it appeared that in early life in that town he was introduced to two families. He attended a lady in one of those families, who died of puerperal fever through no fault of his; he attended another lady shortly afterwards, and people blamed him and said that, having attended one woman with puerperal fever he ought not to have attended a second, and he never got over it. I will not say that he deserved his fate, but certainly if he had shown a little more caution, he would have proved the truth of the old maxim that "honesty is the best policy." I believe the Fellows of the Society will do great good not only to themselves, but to all whom they influence if they inculcate, not carelessness, not the protection of ignorant midwives, but instruction to those midwives that they must be over-cautious, and that if they find themselves in danger of propagating or carrying about disease, they must err on the safe side and say, "For a time I will give up practice; I will not run the risk of damaging the patient who has entrusted her life to my care."

## OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, March 24th, 1875.*Dr. J. MATTHEWS DUNCAN, *President, in the Chair.*

Dr. RITCHIE read the following communication by Mr. William Donovan, Carrignavar, Cork, who had requested him to lay it before the Society :—

*Indian Hemp in Post-partum Hemorrhage.*

Dr. Connel's paper on post-partum hemorrhage has suggested to me the propriety of laying a few facts connected with the treatment of uterine hemorrhage by Indian hemp before the Society. I would not do so but for the strange fact that (as far as I am aware) there is no mention made in any work on either midwifery or therapeutics of the action of Indian hemp on hemorrhage from the uterine cavity.

In cases where flooding set in after delivery, a full dose of the tincture of Indian hemp (℥xx) has, in every instance, acted rapidly, checking the loss in a few minutes, even when ergot has failed. I have also found that it possesses the power of controlling and relieving metrorrhagia and profuse menstruation in a marked degree. What the rationale of its action is I do not know.

Dr. KEILLER mentioned that, in the first case referred to in Dr. Bell's paper, a post-mortem examination had been held, and the uterus had been sent to Professor Turner, while the fetus had come into his own possession. It presented a very curious malformation, as it wanted the lower jaw, and was in some other respects imperfect. The second case related by Dr. Bell he remembered but imperfectly; but knew that the patient made a slow recovery, having suffered, as is not unusual in such cases, from an inflammatory attack. She had had a child since, and all had gone well. He quite approved of the principle of managing those cases propounded by Dr. Bell. Three nights ago he had seen a lady, a hemorrhagic subject, in labour with her fourth child, who gave him much anxiety. From his former experience of this patient, he went prepared with ergot, and gave a dose as soon as the child was born. The uterus kept contracting and again expanding, and with great difficulty he succeeded in stopping the hemorrhage by pressure outside with one hand, and inside upon the cervix and lower segment of the uterus with the other. He could not see how the perchloride could have been of any use in this case, as its direct application to the bleeding surfaces would have been prevented by the clots from reaching the uterine wall. The patient made a good recovery. He thought Dr. Bell was scarcely justified in saying that the patient must inevitably have died had the perchloride been injected.

Dr. CONNEL was surprised to hear Dr. Keiller's last remarks, because Dr. Barnes expressly says that it is necessary to clear out the clots before injecting the perchloride against the uterine wall.

This he had done in his case, where, however, there were no clots of any size. The blood was all fluid. It was quite obvious that the clots should be taken away first.

Dr. KEILLER replied that all know that clots will form, and that it is very difficult to get them all away from the uterine wall. Many young practitioners, moreover, were afraid of removing the clots. He himself had been so when young; and, under such circumstances, he might possibly have used the injection. He believed that, in some cases, the perchloride had never reached the bleeding surface at all, and yet had got the credit of saving the patient. The very case he had just referred to, was one which, in the hands of Dr. Barnes, might have been cited as illustrative of the life-saving property of the "marvellous styptic." Had the perchloride of iron (or even ergotine) been injected in this case, our journals or Society records would, in all probability, have proclaimed the old story of the *post hoc*, &c. Notwithstanding what his friend Dr. Connel had said and urged in behalf of Dr. Barnes's rules as to the use of the perchloride, he (Dr. Keiller) from experience, affirmed that the mere squirting of any quantity of any so-called styptic against issuing blood and clots, such as we almost invariably meet with in post-partum flooding cases, is by no means required or even "equal to the occasion," and cannot be fairly entertained as an *essential alternative*; this opinion, however, did not affect Dr. Keiller's high estimate of the perchloride in many other cases.

Dr. BRUCE was surprised at the number of cases in which it was reported to have saved life. He had seen many cases of severe post-partum hemorrhage, but never a fatal one, and he had never used the perchloride.

Dr. JAMES YOUNG remarked that it was the habit of some practitioners to compress the uterus *till* the placenta was expelled, then remove the hand to look after the child, allowing some considerable elapse of time before the binder or pressure was again applied; meanwhile clots of blood collect, and prove often the cause of severe post-partum hemorrhage. His practice was to keep one hand on the abdomen almost all through the third stage, being assisted by the nurse, if necessary, in the removal of the child; and by keeping up firm steady pressure over the uterus both before and after the expulsion of the placenta, he prevented relaxation of the uterus, and consequent accumulation of clots and hemorrhage. The binder does not and cannot prevent hemorrhage, but he has no objection to its use when the perfect contraction of the uterus has been first secured.

Dr. DUNCAN said that an important point had been raised as to the presence of clots when the perchloride is used. Now, if you waited until all the clots were away you simply waited till the patient was dead, while, if there were no clots, there could be no hemorrhage; he accordingly failed to see where the perchloride was to come in. Again, to inject it while the blood is flowing

would be equally useless, for he did not believe that any quantity of perchloride would stop blood which was still flowing—at least not by its action as a styptic; those who had tried it on blood flowing from a large sinus or sinuses knew how inefficient it was. If of any use at all, it must be on other grounds. With regard to Dr. Bell's first case, without wishing to criticise the practice followed, he thought it better under all circumstances to remove the whole placenta. With regard to Mr. Donovan's paper, he would refer him to the records of this Society, where are to be found a number of experiments made with Cannabis Indica by Dr. D. Christison, at the Royal Maternity Hospital. It was found of some use. Mr. Donovan was quite in error in supposing that its use was not well-known, for it was commented on in, he would venture to say, almost every recent work on gynecology as a remedy which had been tried in various forms of menorrhagia. Dr. Churchill's book on Diseases of Women perhaps contained the fullest account of it.

Dr. BELL, in reply to Dr. Duncan, said that it was impossible to remove the small piece of placenta which was left behind without serious risk to the patient, as it was firmly adherent; all that was removable he had removed. With regard to death being the inevitable result of injecting the perchloride, he considered that in consequence of the uterus relaxing and contracting as it did in the case referred to, it would absorb the solution like a sponge, producing fatal embolism. He complained that we are left in the dark by all English authors who are favourable to the practice, as to what is the real effect of the injection upon the uterus. Does it really cause the uterus to contract? That was the all-important question. If not, it was worse than useless. He thought it might be useful in some cases of laceration of cervix and similar causes of hemorrhage, as is stated in the paper; but it was too much diluted by Barnes to produce any benefit, even in those cases.

## Obstetric Summary.

### *On Laceration of the Navel-String.*

Dr. William Pfannkuch writes on this subject (*Archiv f. Gynec.*, Bd. vii. Heft 1):—In former times it was denied that a laceration of the navel-string could occur spontaneously during labour. In course of time, however, this had to be given up, as a number of observations were collected in which it was proved that the navel-string was spontaneously torn through during the act of delivery. The question was then asked what force was required to do this. From experiments carried out with this object, it was clear that the laceration may be produced in two ways—either by general stretching or by recoil. The first can only rarely happen. Since the fundus of the uterus



follows equally the advancing the child, the length of the navel-string, in spite of its frequent coilings, remains sufficient ; then the elasticity of the tissues prevents laceration in too excessive stretching. The second more frequently occurs, and is of more practical importance. The cord has in this way been lacerated whilst the woman was lying quietly on her back, a powerful pain having expelled the child far from the genitals. This does not frequently happen, and the most common cause is the fall of the child through labour coming on when the woman is in an unusual position for labour, as when standing. As such labours more often occur in unmarried women, this subject becomes of importance forensically. Negrier, of Angers, and Späth, of Vienna, have made experiments in this direction by hanging heavy weights to the ends of fresh navel-strings. Negrier found that on an average 2000—3000 grammes was sufficient to lacerate the cord. Späth that eleven pounds were required. Casper says rightly of these experiments that they prove nothing, since the spontaneous laceration does not occur through general stretching, but through recoil. Here also the momentum of the falling child is not taken into account, nor the difference between the dead and the living navel-string. The author has now performed a series of experiments in the following way :—The placenta was wrapped up in a piece of coarse linen and hung freely, the navel-string is passed through a hole in the middle of the cloth, and to its end a bag is attached to receive the weights. In the first series of experiments 12 mm., the distance fallen through, was equal to the whole length of the cord. In two cases 500 grammes was sufficient to tear through the cord. But, as in labour, the child can never fall through the whole length of the cord, a second series of experiments were performed, where the distance fallen through was half the length of the cord ; here in six cases the cord was torn through by a weight of 700—1000 grammes ; in the other cases the injury was so severe that a slight increase of the weight would have been sufficient to lacerate the cord. The anatomical structure of the cord explains how it is it affords so little resistance to a sudden laceration. The distribution of a force acting in the long direction of the cord is very limited, and the spiral arrangements of the component parts of the cord round an ideal axis renders impossible any transverse equalization. All the parts have one after the other to sustain the full force of the lacerating weight : first the amniotic covering in the concavity is broken through, then usually the arteries follow, then the vein, and last of all the convexity of the cord. The more intensely and suddenly the force acts, the nearer the laceration usually is to the point of application of the weight.

The results of the experiments on “dead” cords, compared with the process observed in the living, show many differences ; these variations are partly such as favour the tearing of the cord by forcible labour, partly such as make it difficult or prevent it. To the first belong : 1, The considerably less capability of resistance of the living

cord. 2, The greater weight of the falling body. 3, The momentum with which the fall of the child begins. Laceration may be rendered difficult (1) by the uncoiling of the cord; (2) by rubbing against the bones or clothing; (3) by the separation of the placenta. The author concludes by saying, that in all cases of labour in which the whole weight of the expelled child can act upon the cord, there is not only the greatest probability, but the almost certainty that it will be lacerated.

---

*On the Employment of Chloral as an Anesthetic in Natural Labour.*

Dr. H. Chouppe (in *Annales de Gynécologie*, May, 1875) contributes an interesting article on this subject. He concludes from a number of observations that chloral is a powerful anesthetic, capable, when it is given in a sufficient dose, of suppressing completely the pains provoked by the uterine contractions. The suppression of the pain, so advantageous in numberless instances, is not obtained at the expense of the regularity and rapidity of labour. The uterine contractions lose nothing, either in frequency or in force by its administration. If in any case they seem to recur less often than before the administration of the drug, that which they lose in frequency they gain in force. In suppressing the pain and the excitation which it provokes, chloral generally hastens the termination of labour; because under its influence the contractions gain in power that which they lose in frequency. Chloral may be employed during labour without any inconvenience to the mother or the infant.

It is especially among excitable women, who have wasted their powers in the first part of labour, and among hysterical patients, that chloral is indicated.

Whatever it may be, when it is decided to employ chloral, it is necessary to give it in a sufficient dose to produce useful effects, and not to lose by too much circumspection the advantages of a valuable remedy.

---

*Extra-uterine Gestation terminating by the Ovum becoming Encysted.*

Mons. Polaillon (in *Annales de Gynécologie*, June, 1875) relates an instance where a multipara, aged thirty-seven, had peritonitis following her second lying-in. The menses ceased on April 20th, 1874. Incessant vomiting began at the end of May. Emaciation ensued. At the beginning of October her state was alarming, there being severe pains in abdomen, insomnia, fever, and emaciation. On the 17th October the patient was seized with nervous spasms and rigors, and severe abdominal pain. The fetus died towards the end of the month, when about six months advanced. The breasts became swollen and painful. Phlegmasia dolens of both lower extremities supervened later on. On March 22nd, when the patient left the hos-

pital, a tumour the size of an orange, and hard, was situated anterior to the uterus, the two being intimately adherent. The menses had not recurred.

---

*The Cephalotribe: its Inconveniences and its Dangers.*

Dr. Boissarie (in *Annales de Gynécologie*, June, 1875) directs attention to the frequent abuse in the employment of this instrument, contending that in a large number of the cases where it is resorted to the long forceps, properly applied, would accomplish delivery; and that in those cases where there is considerable contraction of the brim, the alternative of Cæsarian section would be attended by little increase of risk to the mother, and the child's life generally saved.

---

*A Case of Transfusion.*

Mons. Féréol (in *Bulletin Générale de Thérapeutique*, June, 1875) records a case where the patient, aged forty, was delivered at full time, but lost a considerable quantity of blood. Transfusion by means of Matthieu's instrument was employed, the blood being furnished by a nurse, and injected slowly, without being defibrinated. The patient recovered perfectly.

---

Gynecic Summary.

*Tetanus following Menorrhagia, with Purpura Hemorrhagica and Vaginal Diphtheritis—Hypodermic Injection of Chloral—Cure.*

Dr. Ribell, in a communication to the Chirurgical Society of Paris (*Annales de Gynécologie*, June, 1875), relates an instance of this nature. The patient, aged thirty-six, had suffered from purpura hemorrhagica after each of her four confinements. Nine weeks after her last one severe rigors set in, followed by contraction of the muscles of the neck, stiffness and difficulty in deglutition, with slight trismus. The symptoms increased rapidly, and left no doubt as to their nature. Fifteen-grain doses of chloral every half hour were given for three hours, when sleep supervened and lasted five hours. The symptoms returned when the patient awoke, and gradually increased in severity, pains in the back and suffocation being complained of. Thirty grains of chloral in solution were injected into the side of the neck, and repeated every hour for six times. Sleep then occurred, and lasted nine hours, the patient awakening free from all symptoms.

Fourteen grammes (210 grains) in all were injected.

The patient convalesced slowly. The diphtheritic condition of the vagina was treated with carbolic acid and liq. ferri perchlor.

*Cyst of the Ovary of Thirty-four Years' Standing—Tapping on Twenty-four Occasions—Ovariectomy practised with Success.*

Dr. Bourger (in *Gazette Obstétricale*, June, 1875) relates the particulars of a case of this nature. The cyst commenced growing when the patient was only fourteen, but it was not until twenty-five that the first tapping took place. The operation was repeated from time to time, until, when the patient had reached the age of forty-nine, the re-accumulation became so rapid that ovariectomy was resorted to. The patient recovered perfectly from the operation, but succumbed three weeks later to an attack of acute pneumonia, brought on by her own indiscretion, by exposure to cold.

---

*Uterine Hemorrhage occasioned by a Fibrous Polypus—Transfusion of Blood—Death during the Operation.*

Dr. A. Fillette (in *Archives de Tocologie*, July, 1875) gives an instructive case:—

The patient, aged forty-nine, for over three years had been subject to constant and serious loss of blood *per vaginam*, but had refused any examination being made, and dreaded the idea of an operation.

At the time of the operation she was in a very prostrate condition—blanched, fainting on the least movement, the extremities oedematous. A polypus the size of an apple was detected, and was removed by slight traction and a blunt-pointed bistoury, not a drop of blood being lost. Restoratives were administered, including soup, wine, brandy, and iron, but the faintings continued; added to which there was a very distressing feeling of suffocation, and the patient was evidently sinking fast.

Transfusion was performed the following day, the husband supplying the blood, which was received into the chamber of M. Matthieu's apparatus and injected direct into the vein of the patient, who rallied from her previous insensibility, opened her eyes, looked at the operators, cried out, "Give me the basin, &c.," and then fell back dead—the respiration ceasing at once.

---

Pediatric Summary.

*Frua on a Case of Prolapse of the Urinary Bladder in an Infant.*

Dr. G. Frua writes in the *Annali Universali di Medicina* for March, 1875, that an infant six months old was brought to hospital, supposed to have been suffering for a fortnight from prolapse of the rectum. Instead, however, of rectal prolapsus, there was found in the region of the vulva a substance of the size of a small nut, having a very slightly rugose mucous surface, and a wine-red colour. On raising



the tumour, or pressing it towards the upper angle of the vulva, the vaginal opening was rendered distinct; and when the rectum contracted, urine was seen to escape from two small apertures at its lower part, corresponding to the ureters. The prolapse was considered to be irreducible. The bladder had escaped through the urethral canal, which encircled it like a paraphimosis, and if it could have been reduced, it would have been again prolapsed by the dysenteric forcing with which the child was affected. The child died fifteen days after admission, a month after the condition was first observed. A post-mortem examination confirmed the diagnosis; the ureters were much dilated, and probes introduced into them passed out through the openings observed in the prolapsed bladder.—*Med. Record.*

---

## News.

### MIDWIVES.

At the Meeting of the South-Eastern Branch of the British Medical Association, the following resolution was proposed by Dr. HOLMAN, seconded by Mr. SLOMAN, jun., and carried unanimously:—"That this Meeting desires to call the attention of the General Council of the Association to the present unsatisfactory condition of Midwives in this country; and to urge the necessity of the British Medical Association petitioning Parliament for a Bill which should insure the instruction and provide for the examination, registration, and licensing of this large class of women."

---

### BOOKS, PAMPHLETS, AND PAPERS RECEIVED.

"Position, Pneumatic Pressure, and Mechanical Appliance in Uterine Displacements." By Henry F. Campbell, A.M., M.D., Atlanta, Georgia, 1875. "Observations on the Insanity of Pregnancy and Child-birth." By George H. Savage, M.D.

Contributions, &c., have been received from Dr. Barnes, Mrs. Garrett-Anderson, M.D., C. J. Cullingworth, Esq., Dr. Heywood Smith, Dr. Swayne, Dr. Edis, Dr. J. S. Niven, and Dr. John Williams.

---

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.

THE  
OBSTETRICAL JOURNAL

OF  
GREAT BRITAIN AND IRELAND.

---

No. XXX.—SEPTEMBER, 1875.

---

AN ADDRESS  
DELIVERED AT THE OPENING OF  
THE SECTION OF OBSTETRIC MEDICINE,

*At the Annual Meeting of the British Medical Association,  
in Edinburgh, August, 1875.*

BY J. MATTHEWS DUNCAN, M.D.,  
President of the Section.

---

CURSORY NOTES ON OVARIAN PATHOLOGY.

THE application of physical methods to the investigation of the diseases of the genital organs, brought to light, first, the most superficial or most easily observed of uterine lesions—the ulceration, as it was called, of the cervix ; and it was inevitable that to this great importance should be attached, an importance far beyond what it really possesses. This importance has now, for at least a decade, been rapidly declining ; and while it is admitted that the lesion is very significant, and forms an element of the most common disease of women, it is now well known that it has been not only exaggerated in importance, but misinterpreted as to its relation to the discharge ; and that the treatment, by applying lunar caustic through the speculum to the visible ulceration, once so very widely used, is only rarely a good or successful method. Great improvements have recently been

made in the theory and treatment of diseases of the neck of the womb ; but we can only say that great and admirable attempts have been made to improve in a similar manner the theory and treatment of the analogous diseases of the body of the organ. Endometritis, even, is very inadequately described ; and the amendment of this is, or should be, our first step to knowledge of disease of the uterine body. The influence of displacements and their relation to uterine congestion are not settled matters, after all the voluminous writing which has been zealously devoted to them.

Advancing knowledge of gynecology brings more and more into view the cardinal importance of the ovaries : organs which have, from the earliest times of true anatomy and physiology, naturally attracted a great share of medical attention, and whose extensive pathological influence in the minor ailments peculiar to women Tilt and many others have long tried to demonstrate.

It has been very generally supposed that the ovaries are the *fons et origo* of all the sexual functions, reliance being placed on such evidence as is afforded by the history of their growth, and activity, and atrophy, contemporaneously with the rise, progress, and decline of menstruation and of child-bearing ; on the results of disorganizing disease of both of them ; on the result of their accidental absence, or of their removal, as in Pott's well-known case. I do not know that this view of the ovarian influence has been entertained in recent times by professed physiologists ; but, at any rate, it has extensively prevailed among practitioners, and has only lately been rudely shaken by the observations of ovariologists as to the continuance of menstruation and the occasional persistence of sexual appetite, or its increase even after complete extirpation of both ovaries. I think that of this occasional result of double ovariectomies there can be no doubt, although care must be taken that, in this human vivisectional experiment, all the conditions are truly fulfilled, the whole of both ovaries being removed, and not merely portions of them, or, instead of them, mere parovarian cysts. In antithesis to this argument from removal of the ovaries, we have evidence to

the same effect from their occasional functional activity, as observed by Grohe in the newly-born, without any evidence of sexual appetite or possibility of sexual potency.

This dethronement of the ovaries from the supreme position in all departments of sexual activity is, however, as yet, not complete ; and there is, no doubt, much truth in the view described as being held by practitioners. The rather vague results of the experimental investigations of Eckard, Obernier, Spiegelberg, and others, have been greatly added to by the further inquiries of many, including Oser and Schlesinger, Goltz, and Freusberg. The researches of the two last-named seemed to show that there is in the lumbar spine a nervous centre for the sexual function. The phenomena of the erection of the penis and seminal emission are so easily made the basis of this experimental investigation that, as a matter of course, males have been used for it, but it is almost certain that the physiological results are true of the female also. These investigations afford, in various ways, possible explanations of the persistence of sexual appetite and activity after the destruction or removal of the organs in which undoubtedly commences the production of those germs for whose growth and development the whole of the complex generative function is arranged.

These physiological questions, purely scientific or merely theoretical though they may appear, are far from really being so ; for already operations have been many times performed whose justification lies in the belief that the removal of the ovaries is the annihilation of all or some of the sexual activities. I allude to spaying, or what is called normal ovariectomy by Battey, who, as well as Thomas and Sabine, have done it with what is perhaps justly called success. The proper attitude in regard to it is to defer judgment till more is known about its theory and practice. The diseases against which its performance is recommended, such as nymphomania, menorrhagia, dysmenorrhea, are not so dangerous to life as the great ovarian cystoma for which the very dangerous ovariectomy is now an operation everywhere admitted to be at least theoretically justifiable. Spaying must be able to produce better statistics of success



than ovariectomy—indeed, a nearly uniform success—to be theoretically equally justifiable, for the diseases against which it is used are not nearly so dangerous as ovarian cystoma.

It will be observed that I introduce with care the words “theoretically” and “practically” when I speak of an operation being justifiable ; and it is well worth while to point out the difference between being theoretically justifiable and being practically justifiable ; and I am particularly interested in this important distinction, because I have been gravely misrepresented by one or two excellent professional brethren as having changed my mind as to ovariectomy. I have made no such change. It is they who have not properly appreciated the difference between theoretically justifiable and practically justifiable : a difference which explains any apparent, but not real, change in my opinion. My remarks will apply to spaying as well as to ovariectomy ; but, to save time, I shall refer to ovariectomy only, which has a history convenient for the purpose.

Ovariectomy is theoretically justified, because the possible success of the operation far outweighs the danger of it and the dangers of the disease combined. But theoretical justification is not a sufficient sanction of its performance in any particular case : is not a practical justification. For that you must have other conditions satisfied, and an operator who may be presumed to have the needful surgical skill, or who has demonstrated that he has it by the statistics of his success. When I opposed the operation I was, as I am now, living in the midst of surgical ability, and I knew of many ovariectomies ; but they were, with scarcely an exception, failures, and failures often under the most terrible circumstances ; disaster after disaster. The only justifiable practical cry was that of Syme, Miller, Spence, and myself, for mercy on the poor sufferers prematurely deprived of life. The operation was then and there practically unjustifiable. Sufficient trials had been made and there was no success, or almost none. Who could justly do anything else than oppose ovariectomy there and then ? There are at present, even after Spencer Wells has for a long time flourished, many districts,

even countries, where ovariectomy, now happily justified theoretically by the whole world, is still practically unjustifiable, and accordingly little, if at all, resorted to. In such districts, or countries, the good physician must say to his patient: "Go to some great ovariectomist. If you can't do that, stay at home and die. The operation is not practically justifiable, or not practically justified by success here." Fortunately for Edinburgh, there appeared amongst us a true surgical genius. Long before Thomas Keith had achieved his unparalleled success, he had done enough to make ovariectomy here and now not merely theoretically, but also practically justified. Indeed, since he took up the scalpel, we have never heard a word among us against ovariectomy; and as this silence is logically proper, so was the opposition not only logical, but urgently called for.

I hope I have said enough to show the difference between the theoretical and practical justification of an operation; and I may be allowed to add a word as to the rarity of real surgical talent. I do so with the conviction that there are plenty of young surgeons in whom it may be cultivated; and that, in order to its wider development, encouragement, and ripening, we need not merely invaluable theory and systematic teaching, but also invaluable practical teaching, on the basis of the deficiency of present theory and the still persisting necessary distinction between theory and practice. It is very sad to have to say that operations have in practice to be condemned which are theoretically justifiable and supremely desirable. But it may come to pass that spaying gets recognition as an operation justified both theoretically and practically. It is not so yet.

We may anticipate the time when surgical inquiries shall have been advanced so far as to reveal the conditions essential for success in operations such as ovariectomy. At present they are not known. We know that one operator has a great amount of success, while another has not; and to a very great extent we do not know the cause of the difference between the two. There is no reason to think that the cause of this difference between operators is beyond our powers of discovery; and it is rational and natural to suppose that,

when the discovery is made, it will enhance even the success of those previously most successful. Past attempts to discover this cause of difference have been limited to matters of form of clamps, or kind of ligatures, or dealing with the pedicle, or amount of sponging, or method of draining, or mode of after-treatment, and these are all truly important ; but the causes of the differences are as yet not demonstrated. When they are so, then great operations will be rendered practically justifiable over a much wider field than at present. Yet, after all, there will never, within reasonable limits of time, be such progress as to reduce surgeons to a dead level. There will always be room for genius and for zealous care.

The subject of ovaritis, acute and chronic, has recently, through the labours of Rokitansky and Slavjansky, made some progress ; the former describing follicular and parenchymatous inflammation ; the latter, parenchymatous and interstitial ; a nomenclature which would seem to indicate a greater difference in their views than really exists. But, unfortunately, we have no means as yet of using this knowledge in practice. Our signs and symptoms have to be immensely improved before such a result can be attained. Rindfleisch, indeed, from the pathologist's point of view, and Churchill from the practitioner's, have well pointed out that we cannot tell where ovarian irritation, as it is called, ends, and inflammation begins. But, although this is true, we have now the authority of most of those who have devoted themselves to gynecology in this country for saying that we can, in a very great majority of cases of ovaritis, and especially of chronic ovaritis, and still more especially when adhesions are absent, diagnose, by bimanual examination and otherwise, the existence of the disease, with a degree of assurance that at one time, at least, Scanzoni was disposed to dispute. This diagnosis is a matter of very great moment to the practitioner, not only on its own account, for that is a matter of course, but also on account of the value of its discovery as a contra-indication—to which Nélaton is said to have attached importance—of various other gynecological operations. There are few kinds of cases more frequent occasions for correction of treatment than those that occur under this category. How

often is a pessary intolerable because an ovaritis has been neglected! How often is an uterine catarrh inveterate, or a displacement supposed to be painful, or a metritis supposed to be peculiar, when there is an explanation in the neglect of an ovaritis!

Symptoms and physical examination, especially the bimanual, are our reliance for diagnostic purposes; and, in the bimanual examination, a chief point is the size and consistence of the organ. An enlarged, and consequently a heavy and probably a descended ovary, can generally be felt; while a healthy one, or an ovary merely irritated, may escape the most expert bimanual examiner. The size and consistence of the organs were chief points in Boivin and Dugès's practical classification of kinds of ovaritis, and none better has been proposed. But size and consistence, although often well made out, always escape the examiner in two very important classes of cases: those where the ovary is surrounded by adhesions, and those where it has undergone cirrhotic atrophy. After all, I have no doubt that our improved modern knowledge of ovaritis is one of the greatest steps of progress of gynecology; and it is a blessed one, for the disease, although often obstinate and liable to return, is also often distinctly amenable to treatment. Further aid in diagnosis is offered to us by the rectal examination pushed even as far as Simon has recently recommended, or the examination *per vesicam* recently recommended and practised by Noeggerath. But rectal and vesical examination in such cases are not often required.

One of the most striking advances in gynecology in my day has been in our knowledge of hematocele, which I think is generally called by the name uterine to distinguish its origin. But it is well known that some eminent pathologists, and among them the great Virchow, bring in new inflammatory productions in the pelvis as the source of the bleeding: and, as no disease leads more frequently to such developments than ovaritis, so we find Klebs connecting this great disease with ovarian inflammation. The matter is of great importance with a view to the proper appreciation of such cases, and consequently their proper management.



Upon the question I shall not further enter here, merely remarking that I cannot agree with Klebs in, as a rule, connecting retro-uterine hematocele with any kind of ovarian disease. I speak of the great majority of cases; for I have myself seen in life, and dissected after death, cases in which disease, especially cystic degeneration of the ovary, was the cause of large pelvic hematocele. I believe that the majority of hematoceles are uterine, and not directly connected with ovarian pathology.

There is, alas! far more of darkness than of light in the field of gynecology, and I take this opportunity of stating a conviction I entertain which is founded on considerable experience. We all know how far from uncommon are those simple follicular dropsies of the ovary, seeing them in autopsies as very thin-walled cysts, often less than an orange. Such cysts are, I am sure, the source of much difficulty in practice, and a retrospective diagnosis of them may be formed in this way. A distinct ovarian swelling is found larger than any ovaritis ever grows, clearly diagnosable as an enlarged ovary; but the diagnosis cannot be made more exact. The commencement of an ovarian dropsy is dreaded. After a time, the tumour disappears. Frequently its disappearance is accompanied by adhesive perimetritis. Now, what has happened in these cases? Of course it may be said that it is a case of mere ignorance, or that the cysts were parovarian; but to this latter explanation there is, for me, the great objection that the cases occur more frequently by far than to admit of their being justly so explained. We must suppose, therefore, the bursting of the not infrequent small follicular dropsies. I would further add that the bursting of simple parovarian cysts does not appear to me to be followed by perimetritis, or accompanied by it, so generally in the case of the disease of which I have been speaking.

In connexion with parovarian cysts, a great step of good progress in practice has been made, and progress also in pathology. Such cysts used generally to be described as never reaching a great size, but this is not true. They do certainly attain enormous dimensions, so as to be in competition in this respect with true ovarian cystic disease. After

death, or when they are removed by gastrotomy, they may, at least frequently, be easily distinguished by characters which Bantock has recently described ; chief among them are the non-implication of the ovary, the almost invariable unilocularity (not surgical, but pathological or absolute unilocularity), and the easy separation of the peritoneum from the internal coat of the cyst-wall. During life, they are also, at least occasionally, diagnosable by examination of the fluid withdrawn by tapping from examples of them which have never been inflamed. Then we find the fluid to be limpid or very slightly opaline, of very low specific gravity, containing little or no albumen, and having in solution only a little saline matter, chiefly chloride of potassium and of sodium. Such cysts have been long known and partially described. Even yet they are imperfectly described ; but it is interesting to note a kind of identification of them in those ovarian cysts which Boinet found to be most readily cured by the once famous iodine-injection. Only the important fact is now known that many at least of such cases are far better cured by mere tapping than by that combined with iodine-injection ; and the practitioner should always keep in mind these propitious circumstances when he meets with cases of this kind. The subject of parovarian cysts, when fully made out, will be still further interesting and fruitful in gynecology ; for already we may presume that the numerous kinds and examples of cures of ovarian dropsy, without resort to ovariectomy, which have been described by many, including the highest authorities, were merely examples of delusion arising from imperfect knowledge. We know no one example of the cure, otherwise than by the operation of Ephraim McDowell, of an ovarian dropsy properly so called ; not one, however many may be found described, or whoever may be the describer. Cures by one or more tapplings, cures by medicines, cures by spontaneous rupture, cures by advancing pregnancy, have been, if not mere egregious mistakes, almost certainly cures of parovarian cysts whose history, as already known, quite accords with and explains such erroneous allegations. These remarks are based chiefly on observations of parovarian cysts that have never been inflamed. The study of inflamed cysts

is still in a very backward state. Their pathology and treatment are matters of great difficulty. It must also be kept in mind that simple inflammatory serous collections take place in the peritoneum, and may be mistaken for parovarian cysts.

Ovarian cystoma is the great disease of the ovaries, and improved ovariectomy is the greatest recent step in therapeutics. It is probably to the inspiring influence of the beneficence of this operation that we owe the great amount of scientific exertion that is being made in the field of ovarian anatomy, physiology, and pathology; and already the student may catch glimpses of justly expected practical fruits of this scientific zeal which will enormously extend the utility of the practitioner.

The anatomy and physiology of the ovary, closely connected as they are, have recently received many additions, and more may still be expected from the application to them of the newest methods of investigation. First among these researches comes the histological anatomy of the organ, and here we find the most recent views of Waldeyer receiving, besides verification, addition also and correction by the excellent work of Foulis. Like Waldeyer, this author, beginning with the normal ovary, pushes his studies into the more difficult and more vexed inquiries as to the origin and growth of the multiple ovarian cyst. These points have by many, and frequently, been supposed to be settled; but it is a vain thought, as any one may find who tries to unravel the divisions and subdivisions of recent pathological writers, and then peruses the most recent inquiries and setting forth of views by such authors as Malassez and Foulis.

To notice the whole of the work that is being carried on in connexion with this department, is far beyond my present intention. I shall merely, in conclusion, make brief reference to the difficult questions in connexion with the malignancy of ovarian cystoma. The uterus, although far from being an isolated organ, is generally regarded, among others by Walshe, as remarkable for the slowness or rarity of the spreading of cancerous disease from it as a centre, the slowness or rarity of what is now called the infection of neighbouring or distant parts.

There is, on the other hand, no organ in the body which is actually more isolated than the ovary, whether in health or disease, at least so long as it has no adhesions ; yet many pathologists, especially Klebs, who cites various authors, have attracted attention to cases of apparent infection, especially of the peritoneum, having malignant ovarian cystoma for their source. The diagnosis, especially in their earlier stages of growth, of even large malignant ovarian cystomata, is often a matter of great difficulty, and the examination of the fluids drawn off by tapping is anxiously made with this view but, as yet, without completely satisfactory results. In fact, we have, in all departments of ovarian diagnosis, more to admire in the zeal and diligence of histologists, in regard to the fluids, than in the exactness and reliability of the practical results they can show. In connexion with burst and unburst ovarian cystoma, it is well known that chronic peritonitis of great extent is often set up and long maintained ; but the examination of the fluids has led Foulis to the belief that in the bursting of malignant ovarian cysts we have a new kind of evil, an explanation of the infection of other parts, and especially of the peritoneum, with malignant disease. While the diffuse chronic peritonitis is explained, to some extent, by a rude induction of analogies, the diffusion of cancer is less understood. Foulis believes he can identify in the fluids of certain ovarian cysts a peculiar richly proliferating epithelial cell, which he regards as the seed sown broadcast, when a malignant cyst bursts over the peritoneum, finding in any part of this large surface a suitable soil for its further development into cancerous nodules, such as are seen in the ordinary malignant peritonitis, as it is sometimes called. To the discussion of this subject Thornton and Lawson Tait have contributed, and we hope that, as a final result, the truth will be elicited. We shall then have ovariologists instructed and guided as to the special urgency and claims of a class of cysts that may be equally important and urgent with those which are in a condition of suppuration.

I might still further enlarge on the present state of ovarian pathology, had I not already overpassed the proper limit of time. Many diseases and many researches I have not even



hinted at, and the silence is not to be attributed to neglect or to an estimate of inferior importance.

In opening the section with these remarks, I congratulate you on the promise of interest that is offered by the list of papers to be read, and I can assure you that everything which contributes to increase improvement or illustration of our knowledge will be received with the respectful attention which it ought to command.

---

### Original Communications.

---

#### ON THE RELATIVE FREQUENCY OF THE DIFFERENT CRANIAL POSITIONS.

By JOSEPH GRIFFITHS SWAYNE, M.D.

Consulting Physician-Accoucheur to the Bristol General Hospital.

IN 1852 I read a paper on the "Varieties of Cranial Presentation," at one of the branch meetings of the British Medical Association. This paper was the result of observations made during the early years of my obstetric practice, and contained some statistics respecting the relative frequency of the different cranial positions recognised by authors. It was published in the journal of the Association for February 18th, 1852, and was afterwards referred to by Drs. Churchill, Tyler Smith, and Leishman, in their respective works on midwifery, but more especially by the latter in his very complete and elaborate work on the "Mechanism of Parturition." The conclusions arrived at in my paper were somewhat at variance with those which were then generally received : with a view therefore to test their accuracy by a more extended series of observations, I have since then made careful notes of every case of cranial presentation which has occurred to me in practice. Nearly all of these have occurred in private practice ; but all have been under my immediate personal observation, and have been noted with especial reference to the different cranial positions. In this way I have collected notes of more than 1000 cases, but for convenience of

enumeration, as well as to avoid all sources of fallacy arising from early and immature observations, I prefer taking the last thousand, dating back from the present time. I shall thus exclude all cases which I attended as a student, and during the first five years of private practice, before my attention had been so fully directed to the diagnosis of the different cranial positions. I shall thus be in a good position to compare the results which I now arrive at with those which I obtained in 1852. At that time I published a record of 286 cranial presentations. These comprised 247 cases of the first position—viz., with the occiput towards the left acetabulum, 28 of the second with occiput towards right acetabulum, 3 of the third with forehead towards left acetabulum, and 8 of the fourth with forehead towards the right acetabulum. Thus there would be 86·36 per cent. of the first position, 9·79 of the second, 1·04 of the third, and 2·8 of the fourth. Now these proportions differed materially from those which were given by most obstetric authorities. At that time most British accoucheurs accepted the statements of Naegelé, that the third position was the most common next to the first, and that the second position was the most rare. It was true that Baudelocque and most of the French authorities held a different opinion; but Naegelé accounted for this discrepancy by supposing that nearly all the cases which these authorities had put down as instances of the second position, were in reality cases which had been originally in the third position, but had altered during labour to the second position; the alteration to that position having been overlooked. For a long time these views were generally accepted in this country; but even in 1852 my own limited experience led me to doubt their correctness, and to give my reasons for doing so in the paper I then published. Since that time the careful researches of the late Dr. Uvedale West and of Dr. Leishman, have tended to throw still further doubt upon the accuracy of Naegelé's conclusions. With respect to my own cases the latter observer remarks :\* "Dr. Swayne appears, in the first place, to

---

\* "Mechanism of Parturition." By Dr. Leishman. P. 104.

have had a larger number of first cases than any other observer, but in other respects his experience is strikingly opposed to the idea generally received. He has found, for example, that the second comes after the first, and that the fourth is actually more frequent than the third, and details cases which show that he had seldom any difficulty in detecting presentations of the second kind at an early period of labour. Dr. Swayne's views are confirmed by Dr. Miller, the Professor of Midwifery in the University of Louisville, U.S., whose statement on this point is as follows:—'I find by my note-book that the fourth position has occurred in my notes oftener than the third, but both together less frequently than the second.'” Let us see, then, how far my statistics of 1852 are corroborated by those which I now bring forward—viz., 1000 cases of cranial presentation :

First position . . . . .	792
Second . . . . .	152
Third . . . . .	19
Fourth . . . . .	37
	<hr/>
	1000

It thus appears that my present statistics show a percentage of 79·2 of the first position, instead of 86·36 as in my former observations. This more nearly accords with the percentage given by most other observers; although it is still the highest of any. Sir J. Simpson's figures present the nearest approach to it, and they give a percentage of 76·45 to the first position.

The second position, according to the figures above, is next in order of frequency to the first, just as it was in my former observations. The percentage, however, is higher; for it is 15·2 instead of 9·79, as it was in these. It does not differ much from the percentage given by Dr. Murphy, which is 16·18, and is intermediate between that of Naegelé, which is nothing, or next to nothing, and that of Dr. Uvedale West, which is as high as 31·39.

The percentage of the third position in the figures above

is nearly the same as in those I published before, being 1·9 in the former and 1·04 in the latter. This is lower than the percentage of any other authority, the nearest approach being the figures of Dr. Uvedale West, which give a percentage of 3·11 for the third position, and the highest those of M. Naegelé, jun., which give 32·88.

The percentage of the fourth position is higher than it was in my former statistics. It is in those I now publish 3·7, whereas in the former it was only 2·8. It is not so high, however, as that given by Dr. Murphy, which is 4·42. These figures support the conclusion which I arrived at in 1852—viz., that the fourth position is more frequent than the third, even more strongly than those upon which I then founded that opinion.

The opinion which I maintained in 1852, and which is confirmed by my present statistics, that the second position is most similar to, and next in order to the first, is strikingly opposed to that of Naegelé, who thought that a true instance of the second position does not occur once in a thousand cases, but that all the so-called instances of the second position were in reality originally cases of the third.

I gave my reasons then for doubting this theory, and I could give stronger ones now; but it is scarcely necessary to do so. Since Naegelé's time the careful researches of several more recent observers, especially of Dr. Uvedale West, have, I think, effectually disproved it.

To draw anything like a satisfactory conclusion as to the relative frequency of the third and fourth positions, it is necessary to found our observations upon a large number of cases. Occipito-posterior presentations occur so seldom that those founded upon a limited number are almost sure to prove fallacious. To show that this may be the case even with those which occur so often as occipito-anterior presentations, I will quote the statistics of two sets of 100 cases of cranial presentation which I have noted at different periods:—



*100 Cases of Cranial Presentation from Sept. 23rd, 1865,  
to July 12th, 1867.*

First position	. . . . .	90
Second	. . . . .	7
Third	. . . . .	2
Fourth	. . . . .	1

*100 Cases of Cranial Presentation from Nov. 30th, 1872,  
to July 10th, 1874.*

First position	. . . . .	71
Second	. . . . .	22
Third	. . . . .	3
Fourth	. . . . .	4

My statistics of 1852 had this radical defect that they were founded on notes of 286 cases, which is too limited a number to yield satisfactory results.

The conclusion, however, which they led me to, as to the relative frequency of the third and fourth positions, is borne out fully by my present statistics. The latter show a still greater preponderance of cases of the fourth position over those of the third. But as this result is different from that of any other observer, I will first give a few details which may serve to verify my observations. In the 19 cases of the third position the presentation remained unaltered in 8, the child being born with the face to the pubis. In 2 of these it was necessary to complete the labour by the forceps.

The presentation altered spontaneously in 5, the third position changing to the second. In 2 of these delivery was completed by the forceps.

In 6 of the 19 the position was changed by manipulation to the second ; in 3 of these by the hand, in 2 by the forceps, and in 1 by the vectis. In this last craniotomy had to be performed subsequently. In the 37 cases of the fourth position the presentation remained unaltered, so that the child was born with the face to the pubis in 18. In 3 of these the forceps was used, and in 2 craniotomy had to be performed.

In 4 the presentation altered spontaneously, the head changing from the fourth to the first position.

In 15 this change was effected by art—viz., in 12 by the hand, and in 3 by the forceps. Now the fourth position is considered by nearly all authors to be the most rare, and by Naegelé to occur so seldom that he considers the percentage of it to be almost nil. In my last statistics, however, there were 37 cases of this position, and in 18 of them the presentation remained unaltered. There could have been no mistake about these last, for in every one I observed the head emerging from the vulva with the face towards the pubis, and the right shoulder in front, and yet these alone give a percentage of 1·8.

I cannot, therefore, arrive at any other conclusion than I did in 1852—viz., that the second position is next in order of frequency to the first, and that the fourth occurs more often than the third. I am also led to conclude that in the third and fourth positions, if we leave out of consideration those cases in which the position was altered by manipulation, it is more common for the occipito-posterior position to remain throughout the labour than to change spontaneously to one of the occipito-anterior positions.

---

## NOTES ON PUERPERAL FEVER, WITH REMARKS ON MIDWIFERY STATISTICS.

By HENRY M. MADGE, M.D.

It appears to be the misfortune of puerperal fever that the more it is talked about the more puzzling the subject becomes. In fact, so much has been said about it lately that it has been nearly talked out of existence, for although one set of writers and speakers say that every feverish condition occurring about the puerperal period is puerperal fever, others maintain that nothing is puerperal fever, and that it is always something else. In my humble view, speaking from my own experience, it would be a great mistake to ignore the name and existence of puerperal fever.

Of course in speaking of this fever I do not mean the ephemeral fevers and other minor troubles of the lying-in-room. These appear to bear the same relation to puerperal fever that the *febriculæ*, or little fevers, do to typhoid or typhus, and as they do not even lead up to the severer type—which is perfectly distinct—they may be left out of consideration altogether. What we are now trying to understand is a disease which kills, and is conveyed by contagion or infection from one patient to another. Puerperal fever appears to me to be something like a many-headed monster: you may lop off half a dozen heads, and say, it is not this or that, and fancy you have killed the monster, but the heads will all crop up again in some form or other, and puerperal fever will remain, and remain, I fear, a long time, to baffle our diagnosis and in too many instances our treatment. It has very properly been laid down that “this is one of those questions which no man can decide upon his own experience,” and yet that “by the record of a series of cases, or even isolated cases arising under circumstances that admit of precise analysis, the study of the subject may be advanced.”\* This latter suggestion has prompted me to give the results of my own experience, extending over a period of about twenty-two years. For the sake of closer analysis I shall confine my remarks to cases of puerperal fever occurring in my own practice. The evidence thus afforded will be of two kinds—negative and positive. The positive connected with the facts and surroundings of each case; and the negative, which must be of some value in an inquiry of this kind, afforded by the fact of my having attended hundreds of cases of labour under all sorts of circumstances—often in the midst of scarlatina, measles, and almost every form of fever—without meeting with an example of the disease. The examples I have met with, I am glad to say, have been few in number; but few as they are, I will not occupy time and space by giving minute details of each, but merely a general outline. During the period I have named I have personally attended over 3000 cases of labour. In the first five years of my

---

\* *Lancet*, July 24th, p. 125.

practice all the cases did well. Shortly after the expiration of that time I assisted a friend in a case of breech presentation, where the child was dead and decomposed; a bad odour, notwithstanding the use of disinfectants, adhered to my hands for several days, and as I had read Dr. Routh's paper on puerperal fever in the Vienna Hospitals, I deemed it prudent not to attend midwifery for a time. At length, when I considered myself safe, and no odour remained, I attended three cases, which did perfectly well. The fourth case (this was nearly three weeks after the breech case) was that of an apparently healthy young woman, aged twenty-six, second child. Everything went on favourably until the fourth day, when she was seized with violent abdominal pains, and very soon became delirious. No treatment seemed to do any good, and she died on the ninth day. Mr. Newton and Mr. Bailey saw the case with me and pronounced it to be puerperal fever. Whether this case was the outcome of the breech case or not it would not be easy to say, I can only hope that it was not so. The painful circumstances of the case alarmed me so much for the safety of my other midwifery patients that I went into the country, and did not attend another case for about five weeks. The first I attended after returning to practice, strangely enough, ended in a severe form of phlegmasia dolens, but happily the patient recovered. I had about the same time two other cases of a similar nature, but in a much milder form. Now, whether these cases were mere coincidences—independent occurrences, or whether they show that there is some relationship between puerperal fever and phlegmasia dolens, and that I was still carrying about some of the poison derived from the fatal case in a changed or diluted form, I am not prepared to decide. During the next twelve years I was very busily engaged in midwifery practice—having attended in that time about 2000 cases. I met with almost every form of complication, mechanical and zymotic, but not one case of puerperal fever—that is, such as I understand the disease to be. In the whole series of cases there were very few deaths, and those I shall speak of later on. With regard to zymotic diseases, I had one case in which the mother had



the eruption of scarlatina out the day after her confinement. It appeared to run its usual course, and she did well ; but the child, when four days old, showed signs of the disease, and died on the tenth day. In June, 1861, I exhibited at a meeting of the Obstetrical Society two fetuses (twins) covered with small-pox pustules. The mother had had the complaint previous to the confinement, and did very well. I mention these cases, and I could add others, to show that the eruptive fevers may preserve their chief features unchanged during the puerperal state. I will not venture to say that they always do so—there is too much evidence of a reliable kind to the contrary. Scarlatina in particular, according to some of our best authorities, may be so modified and disguised in a form of puerperal fever as to have lost nearly all its ordinary characteristics, and still retain the power of infecting in the ordinary way as scarlatina. I have not seen such cases, but it would be unwise, and perhaps very unsafe for our patients to doubt their occurrence. That the majority of cases of so-called puerperal fever are only scarlatina in disguise is an opinion, however, that at present I do not hold. During the time (twelve years) I am speaking of, the nearest approach to puerperal fever I met with was a case of what would more properly be designated “puerperal pyemia.” This occurred in October, 1865. The patient, aged thirty, primipara, had a very fair time. There was a slight rupture of the perineum, but she went on very well until the eighth day. She then committed some indiscretion in diet, which made her very sick and ill. The sickness continued for several days. The perineum became painful, and the sore assumed an unhealthy aspect. There was weakness and prostration, but not much fever. The milk nearly disappeared, but as the appetite improved, more nourishment was taken, and the breasts became fuller. About this time she had a severe rigor, and on the following day the left leg was found inflamed on its inner surface, which inflammation soon ended in the formation of matter. She then had a succession of abscesses in different parts of the body. The general symptoms were very variable. There were severe rigors, followed by transient

fever and loss of appetite. As a rule she took nourishment well. The secretions were usually healthy. There was no peritonitis and no head symptoms, no delirium. She was never able to get up. At the end of two months the abscesses had nearly all disappeared or were healing up, and strength improving, when unfortunately, the weather being cold, she had a severe attack of broncho-pneumonia, which carried her off in a few days. Mr. Newton saw this case with me. We did not consider it infectious, like puerperal fever, indeed not more so than cases of ordinary abscess—all of which, of course, require certain precautions on the part of the accoucheur. I have drawn the more fully from my notes of this case, as I believe it is possible to draw a distinction—with a considerable difference—between puerperal fever and puerperal pyemia.

In January, 1870, I attended a lady in Holloway, aged thirty, fourth child; she had enjoyed good health and had always had a good “getting up” after each confinement. The house was a semi-detached villa in an open situation, and all the sanitary arrangements appeared to be perfect. She went on well at first, but on the fifth day she felt unwell. There were no very definite symptoms, no rigors, no abdominal tenderness, no fever, but a general feeling of malaise and loss of appetite. The next day there was slight fever, headache, nausea, distaste for food, and abdominal pain on pressure. On the following day all these symptoms were aggravated and accompanied with delirium, tympanitis, prostration, arrested secretion of milk, and scanty lochial discharge. Dr. West and Mr. Newton attended with me, but in spite of all treatment she went on from bad to worse, and died on the tenth day. We treated it as a case of puerperal fever, but no cause for it in any shape, although long and earnest inquiries were made, could be discovered. Whilst I was attending this patient, and before the fever had declared itself, I attended two other confinements, wives of well-to-do tradesmen. The first, aged thirty-two, good general health, fifth child, had a favourable time and seemed to go on well for a few days, but the milk did not as usual show signs of coming, and the lochial discharge became scanty and offensive. Fever

supervened, although in a very gradual manner, and the patient passed through several weeks of the most serious illness ; she had almost every bad complication : peritonitis, parietal and visceral—the intestines could be seen matted together, and mapped out against the abdominal wall ; pneumonia, and of the worst form—“ septic ;” one lung completely consolidated, frequent sickness, constant delirium, involuntary passage of feces and urine, and bed-sores. For nearly a fortnight she appeared to be just hovering between life and death ; all she could take during that time was a little iced champagne, which I have no doubt was the means of saving her life. One morning, scarcely expecting to see her alive, instead of finding a parched brown tongue, hot, dry skin, and a state of semi-stupor or muttering delirium, I found her in a profuse perspiration, the tongue soft and moist, and the head clearer. From that moment she began to improve, and made a slow but perfect recovery. She has since passed through two confinements without anything untoward occurring, showing that peritonitis in its severest form does not always lead to sterility. The case also shows that patients will sometimes rally from apparently the most hopeless condition. The second case occurred in a patient, aged twenty-eight, good general health, second child. The case was over, that is the child was born and the placenta removed before my arrival, and all that I did was to assist in putting on the binder. In commenting on these cases it will be well to bear this in mind. On the fourth day after the confinement the patient had a severe rigor, and nearly the same train of symptoms followed as in the previous case ; some, however, were wanting : there was no pneumonia, involuntary stools, nor bed-sores, but severe peritonitis, intense fever, sickness, delirium, and prostration. Like the other patient she was kept alive for upwards of a week by means of a little iced champagne, the only thing the stomach could bear. All along she complained of severe pains in the left groin and thigh, as if the vessels and tissues of that region were inflamed, but there was no obstruction of the veins, no swelling of the leg. During convalescence there was a good deal of contraction, the thigh being flexed on the abdomen, and

it was several months before she was able to walk without lameness. I need not say that these cases gave me the greatest anxiety, and I am greatly indebted to Dr. Rogers, of Berners Street, for his valuable assistance in helping me to guide them to a favourable issue. As to treatment it was of a varied kind to meet the ever-varying phases of the illnesses. I am glad to say that since these unfortunate experiences I have seen no more examples of puerperal fever.

*General Remarks.*—In commenting on these five cases of puerperal disease it will be convenient to speak of them as Case I., II., and so on in the order of their occurrence ; and as I have no particular theory to advocate, the facts will be allowed to speak for themselves.

In a series of cases it is often difficult to say how the first case originated, and this is true of many other infective fevers besides the puerperal. The same may be said of the poison of hydrophobia. Why and how it originates in the dog no one can tell, but once the poison is manufactured as it were in the animal's system it can be communicated by inoculation to an unlimited extent. There is the same mystery about the origin of many cases of puerperal fever. I say many, because some appear to have received a ready explanation ; indeed, there are said to be several factors in its production. But the rather curious part of the matter is, that whether the cause is known or unknown, whether it is septic poisoning, zymotic disease, mental shock, or direct infection from another case, the later stages of the disease present nearly the same features in all—features which under and only under the influence of the puerperal state stamp it as a peculiar fever with its own peculiar poison, capable of being propagated to an unlimited extent amongst lying-in-women in the same way probably that the poisons of the other acute specific diseases are propagated. As to the general nature of these poisons some light seems to be dawning upon us through the researches of Chevreau, Klein, Sanderson, and others. These observers have found that in sheep-pox and small-pox the infective property resides in the solid particles or granular matter of the pustules, and that these particles can be separated from the fluid portion



and employed successfully to reproduce the disease. These particles are only discovered by lenses of high powers, and by practised microscopists; and therefore, although so potent, must be exceedingly minute—so minute that when free from the fluid in which they are found they must be capable of floating in the atmosphere, and thus a not improbable explanation of how small-pox spreads is arrived at. May not puerperal fever also have its granular matter or solid particles and be propagated in a similar way? The odour of the hands has been spoken of as a source of infection. Now an odour implies the existence of particles of matter in the air. If vapour which is visible consists of matter finely divided, an odour must be made up of a still more infinitesimal division, and there may possibly be some analogy between the atmospheric physical condition of the particles of matter which constitute an odour and those which propagate disease. With regard to the microscope, the probability is that if specimens of the poisons of all the different specific diseases could be placed under that instrument they would appear to be very much alike, and that we should still be able to recognise them only by their effects, just in the same way that the germinal vesicles of nearly all animals present the same appearance under the microscope, but in development take very different directions. After disavowing any intention of theorizing, I fear the foregoing remarks may appear to savour too much of the germ theory of disease. I will therefore not pursue the subject any further, but leave it to those better able to handle it. Having been led from a consideration of my own cases and of the numerous other cases that have been recorded to believe that a specific poison capable of producing puerperal fever is frequently conveyed by the attendant, I have often speculated as to its possible nature and mode of spreading itself. Bacteria, unless there are different species, some poisonous and some innocent, do not appear to me to play an important part in puerperal disease. On three occasions I have found different forms of bacteria in the lochial discharge. There was nothing the matter with the patients. If a slide is applied to a napkin just removed from a patient a few days after confinement

and a covering glass placed over the discharge the microscope will, at least in some cases, reveal bacteria. There are fallacies to guard against, such as the débris of the uterine tissues, changed blood and other corpuscles, stringing together and looking like rod-like bodies ; but with care, and following the latest authorities as to what are and what are not bacteria, they will usually be found. Perhaps their presence here simply signifies a fluid in a state of decomposition.

A good deal of the recent discussion on puerperal fever at the Obstetrical Society seemed to turn on the question of names. If I were able to give the foregoing five cases of puerperal disease their proper names I should be doing some service. As, however, our highest and most respected authorities have failed to give a satisfactory definition of child-bed fever—a definition accepted by all, a definition that enables us to recognise the disease at once, it would be presumptuous in me or any one else in the present state of our knowledge to attempt to do so. I can only state my views as they flow from the facts of the cases, and leave others to judge for themselves.

About CASE I., I am doubtful as to what name it ought to have. It was evidently a case of rapid blood-poisoning. I have mentioned it in connexion with an unsavoury breech-case as affording a possible clue to its cause, but I think, on the whole, it is more probable that it was a sporadic, autogenetic case. The peritonitis was severe, but the evidence of blood-poisoning, and of its being something more than peritonitis, was the early advent of delirium. I have seen cases of simple "puerperal peritonitis," but in those cases the delirium—if any—did not show itself until towards the end. In this case the patient rapidly passed into a peculiar typhoid automatic condition. She could take nourishment, show her tongue when asked to do so, answer "yes" and "no" to inquiries, and then immediately lapsed into incoherence, said she was getting well, babbled about green fields and going into the country, when every one around her could see that she was dying. Whether this case should be called septicemia or puerperal fever I am unable to decide. We know that in known cases of septic poisoning

and also when septic matter has been introduced into the veins, or into the peritoneum of an animal, somewhat similar symptoms have shown themselves. Some would probably call it a case of acute pyemia, but there was no evidence of pus, healthy or degenerated, and as to possible embolisms with secondary abscesses that would be a mere speculation.

CASE II.—I have named this case “puerperal pyemia” because it had all the characters of what used to be considered pyemia, and answers to the old description—pus in the blood—multiple abscess. There seems to be very little analogy between this and the previous case, or to any of the other cases, none of which, as far as I can see, should properly be called “pyemia,” although this term is now applied to almost every form of puerperal disease.

CASE III.—This was the Holloway case—a case which puzzled us a good deal as to its origin. It may have been, like Case I., a sporadic, autogenetic case. If the poison was generated in the patient’s system, it was certainly not from any portion of degenerated placenta left behind. I will not be so certain about decomposed clots of blood, although the lochial discharge was not for some days very offensive. This case, then, might have been septicemia, generated in the patient’s system, as Mr. Hutchinson says it always is; or it might have been puerperal fever brought about by some occult cause or causes. It must be admitted that the differential diagnosis between puerperal septicemia and puerperal fever in sporadic cases is often a difficult matter. Cases IV. and V. followed so closely on Case III. that, in my own mind, I have always associated them together as cause and effect, and I have further been under the impression that if I had gone on with my midwifery attendance, I should or could have extended the disease to an unlimited extent. It will be remembered that in Case V. I did not touch the patient with my hands. There was here at least no direct inoculation with septic matter, so that the poison, whatever it was, must have been conveyed in the same invisible subtle way as the poisons of the other specific fevers are conveyed. The same explanation—if explanation it can be called—probably applies to Case IV.

Now what name are we to give these two cases? They were not erysipelas, nor scarlatina, nor septicemia, nor pyemia, nor anything that we can give any other name to than puerperal fever.\* This fever is no doubt of a protean character, and so is typhoid; but the puerperal has two features which are almost characteristic and constant, and those are peritonitis and delirium. After all, it may be said that our knowledge of the poison of puerperal fever is almost on a par with our knowledge of the poisons of the other fevers—we are able to recognise them chiefly by their effects. I venture to think, therefore, that an unbiassed consideration of the cases I have related will favour the opinion, that the terms “puerperal pyemia,” “puerperal septicemia,” and “puerperal fever,” are all necessary to describe the different forms of puerperal disease that are met with. They may run into each other, but they as often form distinct groups—different classes, so to speak, of the same order.

With regard to the predisposition to puerperal fever, and to other troubles said to accompany the puerperal state, whilst fully admitting and deploring the existence of such dangers, I believe that they have been by some writers a good deal exaggerated. We have heard so much about the morbid changes in the blood of the pregnant woman—dangers before, during, and after labour—dangers of blood-poisoning from uterine absorption, from phlebitis and lymphangitis—dangers in fact “piled up” in all directions, that it seems wonderful, if they were all true, that any of our lying-in patients escape with their lives. What, however, are the facts? Why, that in 99 cases out of 100, and, in the experience of some, in 999 cases out of 1000 everything goes well. Changes in the blood there certainly are, but if we knew more about those changes we should probably find that their tendency is more towards securing the safety of the patient than to initiate or promote disease. There is also something not very clear about the absorbing

---

\* It ought perhaps to be mentioned that a child of the patient (Case IV.) was sent to a friend's house before the confinement to be out of the way, and whilst there took scarlatina. There was nothing, however, about the mother at all resembling scarlatinal symptoms.



powers of the uterus in causing blood-poisoning. The rival claims of veins and lymphatics are now advanced, and have been long advanced as the principal agents in causing the mischief. Now, although it is well to avoid speaking in a flippant and disrespectful manner of the laborious researches of eminent men, I believe there is an axiom that, "when theory and experience do not correspond, there must be something wrong with the theory." Experience teaches us that, notwithstanding the supposed evil propensities of uterine veins and lymphatics, a practitioner may attend thousands of cases of labour without meeting with a case of blood-poisoning. Anatomically, too, whatever activity the uterine veins and lymphatics may possess before labour, immediately after that process—from the great changes which at once take place in the uterine walls—their active existence must be greatly interfered with, if not obliterated. I will also add that those gentlemen who freely use intra-uterine injections of iron, iodine, and other agents, can have but a small belief in uterine absorption. Notwithstanding what I have said, wash out the uterus by all means, leave nothing undone said to be likely to benefit the patient, although I confess I have not witnessed the magical effects attributed to that proceeding by some writers. The aim of my remarks is to show that too much has been made of the dangers of midwifery practice—that, in the vast majority of cases, it is not attended by anything deserving the name of an illness—that it is a natural process—nature's wonderful adaptation of means to an end, and that in criticising those means we are treading on rather dangerous ground, and are very likely to fall into error. Another way in which the dangers of midwifery practice are kept prominently before the profession and the public, and thus made to excite unnecessary alarm, is by means of statistics. According to some calculations, the deaths are said to be about 1 in 100 cases. If those calculations are based on official returns, there surely must be something very faulty either in the attendance or in registering the cases. I should hope and think that in such a calculation deaths from other causes must be included. I have attended upwards of 3000 cases, and have met with

eight deaths : one from flooding, the patient already reduced by phthisis ; one from peritonitis, case of fibroid tumour impeding delivery ; one from peritonitis, after *placenta prævia*, injury to cervix ; one from peritonitis, from exposure to cold, and taking stimulants ; one ruptured uterus ; two puerperal fever, and one puerperal pyemia. The pyemia case lived two months after confinement, and then died from broncho-pneumonia ; so that excluding that case the deaths were just a fraction over 1 in 500. Many of my medical friends have been more fortunate even than this, and have attended 2000 and 3000 cases without a single death. If all statistics are said to be more or less fallacious, and that very few believe in averages as applied to the future, such remarks apply with peculiar force to midwifery statistics. The meeting with bad cases seems to be a mere lottery—a mere chapter of accidents following no law whatever, not even the law of probability. It is a most unlikely thing that one series of say 2000 cases, would be anything like another series of the same number. Such considerations must lessen the importance of midwifery statistics, at the same time they are not without their value if carefully made. Indeed, it appears to me to be a subject well worthy the attention of the Obstetrical Society, to find out what the real statistics of fatal cases of midwifery throughout the country are. I do not forget that the death-rate may be increased by careless practitioners and ignorant midwives. A practitioner, for example, in attendance on a case of puerperal fever, might be the means of causing several deaths. This occurs sometimes under circumstances over which the practitioner has no control—that is, when the nature of the fever has not revealed itself. When, however, the nature of the case is known, none but the reckless would go on attending new cases. Ablutions are highly useful and necessary, also changes of dress and the use of disinfectants ; but under such circumstances nothing short of ceasing to attend for a few weeks will secure the safety of our patients.

I am not sure that an open discussion at the meetings of the Obstetrical Society was the best means for bringing out the experience of the Fellows on such a subject as puerperal fever. In the first place, comparatively few get an opportunity

of speaking ; some, again, are not in the habit of conveying their thoughts in this way ; others, perhaps, with many painful experiences would be rather chary of giving them full publicity. It requires a little courage to make what would to many appear to be damaging admissions and confessions. Yet this is the kind of information most required. There is no doubt therefore that for these and various other reasons there is in the Obstetrical Society and the profession at large a vast amount of valuable material lying latent, or rather running to waste. The best machinery for bringing all this material to the surface would be by means of a committee. It is true we cannot boast much of the work done by the committees already formed by the Society, but for the reasons just stated, puerperal fever especially calls for this mode of inquiry. Every Fellow of the Society, and every member of the profession would thus be able to give—anonynously or otherwise—the results of his experience, and most probably such a mass of information would be obtained as would go a great way towards settling many of the points in dispute. A scheme could easily be drawn up, having columns with various headings indicating the information required. The work of such a committee would be considerable ; but there are many Fellows of the Obstetrical Society who would be glad of the opportunity of assisting to solve one of the most important and puzzling questions of Obstetric Medicine.

---

## *Reports of Hospital Practice.*

---

### MIDDLESEX HOSPITAL.

#### CASES EXEMPLIFYING THE NECESSITY OF MAKING A VAGINAL EXAMINATION.

By ARTHUR W. EDIS, M.D.,  
Assistant Obstetric Physician to the Hospital.

#### *Leucorrhœa—Acute Vaginitis—Recovery.*

B. M., aged thirty-nine, married, mother of six children. Since last confinement (over ten months) has suffered from a

copious yellowish-white discharge, milky in character, much burning pain and smarting on micturition, bearing-down pain, and aching across the loins, "soreness in the parts," much distress on walking. Pain so severe as to render coitus impossible. Catamenia too frequent and too profuse.

The patient had rheumatic fever, and was laid up for two months during March and April, since when she has been in a very anemic condition, and had an attack of iritis as well as conjunctivitis. There was no history or trace of syphilis. On examination the vagina was found to be bathed with a very copious milky discharge, the vagina itself being intensely red and injected, the vulval aperture being also very inflamed. The surface was exquisitely sensitive to the touch, and it was with difficulty that a complete examination could be made. The uterus was apparently normal in position and bulk. The vagina was carefully mopped out with a solution of nitrate of silver (ʒj ad ʒj). A lotion of borax and lead with quinine-and-iron mixture and cod liver oil thrice daily were ordered. A strong solution of the nitrate of silver (ʒj ad ʒj) was subsequently applied on three separate occasions at intervals of a week, to the whole of the vaginal surface, the vulval outlet being also included. At the end of six weeks from the commencement of treatment the discharge, which had lasted nearly twelve months, had entirely disappeared.

The patient's general health had very materially improved, and she was discharged as convalescent. She stated that she had been doctoring the whole nine months preceding her attendance at Middlesex, for the discharge and inconvenience. She had consulted five separate practitioners, but had never been examined.

---

*Leucorrhœa—Menorrhagia—Polypus Uteri.*

H. J., aged forty-nine, mother of ten children, and has had five miscarriages. General health good until lately. Six months ago she had some flooding, which lasted a week, and a watery kind of discharge which had been present more or



less for some months past—gradually increasing in quantity until, as she expressed it, “she was drenched with whites.”

The catamenia were profuse and recurred every three weeks during the last six months. When first seen the patient was looking extremely anemic, the face pallid, edema of the lower eyelid, rapid feeble pulse, much languor, no appetite, and other well-marked constitutional symptoms. The necessity of making a vaginal examination was explained to her, though “none of the other doctors” that had attended her during the last six months “had ever suggested such a thing.” On examination the vagina was found to be filled with a soft fleshy growth, which at first seemed to be epitheliomatous in its nature, and was in fact diagnosed by several as such. On passing the finger carefully by the side of the growth, between it and the vaginal wall, a distinct pedicle, surrounded on all sides by the os uteri, was detected, thus clearing up the diagnosis, and plainly indicating the source of “the whites.” As her general health was in such a feeble state she was recommended to come into hospital, where Dr. Hall Davis removed a polypus as large as a moderate-sized apple, by means of the single wire *écraseur*. She remained in ten days, and left much improved in health. At the end of a month it was difficult to recognise her as the same individual.

---

## THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

SEPTEMBER, 1875.

---

### THE DISCUSSION ON PUERPERAL FEVER.

AT the last meeting of the Obstetrical Society of London its Fellows terminated an important discussion, which has absorbed nearly the whole of the time of four meetings. It was quite evident, however, that notwithstanding the lengthened period which had been devoted to the subject, the last word

had not been said, and that many speakers would have been glad to have added their experience had they found an opportunity. Several times the practice of reading speeches was adopted, and although the plan may have some advantages, it is doubtful whether on the whole it is not outweighed by other adverse considerations. The zest of discussion is in its pithiness and spontaneity. If a man has anything to say, surely, by the aid of a few notes, he can make himself understood. A few short sentences to the point uttered extemporaneously are far more effective, and obtain much more attention than the same number of sentences read, no matter how polished or rounded they may be. It is the thought, not the mode of expressing it, which is most appreciated. At one period the discussion became converted into a series of short papers read by their authors to a continuous accompaniment of impatient and disapproving noises. The debate, however, notwithstanding these drawbacks, it must be acknowledged, was conducted with dignity and ability, and most of the speeches were pointed and practical, and some unusually eloquent. Now the profession may know what are the latest and most mature thoughts of the best obstetrical authorities of England upon a disease in which few indeed are not gravely interested. Every particle of evidence relating to it has been re-sifted and re-tested. The very term puerperal fever has had a struggle for its existence, and although the time does not yet seem to have arrived for its abolition, its right to exist is strongly denied by several. On many points there still remains much obscurity and difference of opinion. On others, again, there is a happy unanimity. All agree that the puerperal condition of a woman is one which renders her liable to be affected by influences which at another time might produce no serious mischief, and that physical and mental shock during this time may turn the balance and determine disease which might not otherwise occur. A large majority of the Fellows believe that puerperal fever is caused by septicemia, autogenetic or communicated. The very soul and strength of the discussion rest in this thought. Here is the idea which cannot be too vividly impressed upon the minds of all obstetricians and midwives.

Upon its entire acceptance and proper apprehension depends the safety of many mothers. An offensive post-partum vaginal discharge must not be permitted. It must be prevented by skilful management of the third stage of labour, by insuring efficient lochial drainage, and, if necessary, by washing out the utero-vaginal canal with antiseptic fluid. No one now doubts the communicability of the poison which exists in putrid lochia, or that the most minute quantity of it conveyed to a healthy puerperal woman may produce in her a fatal complaint. A responsibility of a most serious nature therefore attaches itself to all those who have in any capacity to deal with lying-in cases. A mystery hangs over the nature of this pyrogenetic fluid, but the laws which relate to its origin and mode of propagation are sufficiently well known to enable us to do much towards checking its generation, and preventing the extension of its malignant action when begotten. How, and in what proportion, cases of puerperal fever are produced by the zymotic diseases, and what influence bacteria may have, are questions which require and we hope will receive further careful consideration.

## *Abstracts of Societies' Proceedings.*

### OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, December 9th, 1874.*

Dr. J. MATTHEWS DUNCAN, *President, in the Chair.*

#### *On Prolapsus and Retroversion as Alternating Misplacements of the Gravid Uterus.*

By FRANCIS IMLACH, M.B., Liverpool.

(Communicated by Dr. MATTHEWS DUNCAN.)

Prolapsus uteri may occur any time within the first three or four months of pregnancy, and continue during the remainder of those first months, without danger to the pregnancy or to life; so also may retroversio uteri. But while it is exceptional for the former, it is not so for the latter, to prove a cause of danger.

These two states may with advantage be compared with each other: the former as a displacement downwards in the pelvic axis, the latter as a rotation backwards about a transverse axis. The two

misplacements never coexist in the extreme state at the fourth month; but complete prolapsus is often associated with slight retroversion, and complete retroversion with some descent. Positions which result from combination of their various minor degrees are probably frequent, but of little practical importance; it is the successive occurrence of the two extreme misplacements, the substitution of one for the other in the same pregnancy, that is the chief subject of these remarks.

Both misplacements involve altered relations of the uterine attachments. Posteriorly, they involve like alterations. In prolapsus the rectum is not displaced, but Douglas's pouch descends with the uterus, and the subperitoneal fibro-cellular tissues, chiefly the utero-sacral ligaments, are drawn down to a like extent. In retroversion, the rectum also remains in its normal position, but the pouch follows the uterine cervix even to the symphysis pubis, and with it the underlying fibro-cellular tissues. And the extent of structural elongation must be nearly equal; for, suppose the uterine diameters to be 4 inches, and the retroverted uterus to have rotated through, say,  $130^{\circ}$ , then the elongation amounts to 4.5 inches, which corresponds with a very considerable degree of prolapse. Anteriorly, the vesical attachments are loosened—that is, extended—and the whole bladder displaced downwards in prolapsus, upwards and backwards so as to become extrapelvic in retroversion. Here, again, therefore, is similar structural elongation, differing only in direction. The lateral alterations are in contrast: in prolapsus there is simply downward extension of the broad ligament; in retroversion, there is twisting. In consequence of this torsion there must be shortening; hence, probably, previous extension favours subsequent torsion.

Both processes are usually reversible by natural means or artificial aid. Prolapsus consists in simple descent: ascent of the uterus is its natural course in pregnancy, and rectifies the misplacement; increased descent the natural course in abortion or labour at the third or fourth month. Retroversion consists in simple rotation about a transverse and nearly central axis; in rotation combined with more or less determinate deformation of the uterine mass, such as retroflexion and the irregular projections, described by W. Hunter; or in one of these, combined with some descent or prolapse: reverse rotation with ascent constitutes natural rectification during pregnancy; reversion with increased descent is the natural course of labour.

Each misplacement may recur after artificial rectification. As regards prolapsus, this is too well known to require any example. A case from Martin le Jeune\* illustrates recurrent retroversion of the gravid uterus. A woman pregnant at about the third month, after a

---

\* Quoted by Bernutz and Goupil, "Diseases of Women," vol. ii. p. 187.



fall suffered from retention of urine. The uterus was found retroverted, and was reduced without much difficulty; but on her getting up, the displacement was repeated, and again replaced. A recent case in my father's practice—which I did not see, but of which I had cognisance during the period of observation—is an example of alternation of the two misplacements in successive pregnancies. The lady has had three children born at intervals of sixteen and fourteen months. The first pregnancy was natural. At about the end of the third month of the second, the uterus became suddenly and completely retroverted, the os uteri being above the symphysis pubis. The retroversion was reduced without much difficulty; the patient remained in bed until the period of quickening, and after that the pregnancy was natural. At the same period of the third pregnancy, the uterus became extensively prolapsed; it was returned, and a Hodge's pessary inserted; she remained in bed for a few days, and the remainder of the pregnancy was natural. After the labours, which were all natural, there was no retroversion or other misplacement. The last case I have to mention illustrates the substitution of one misplacement for the other in the same pregnancy:—S. K., aged forty-three, nine children and three miscarriages; says the womb came down during the early pregnancy of the eighth child. It returned at the period of quickening, and did not again descend for some months after delivery—not until she had begun washing and like hard work. The uterus again became completely prolapsed, somewhat suddenly, three weeks previous to examination. She had been standing washing, but not more than usual. Had not menstruated for two months. Uterus found to be the size of a large orange, and, with the bladder, extensively prolapsed; external os patulous, and its lips averted, eroded over a large surface, and covered by a lymphy pellicle, which is easily stripped off, and is seen to be continuous with the cervical plug. Nitrate of silver applied to the eroded surface, the prolapsed mass returned, and the patient kept in bed. Next day she complained greatly of dysuria and incontinence; the uterus was found completely retroverted, the os being above the symphysis pubis, and the fundus low down in the hollow of the sacrum. After various unsuccessful attempts at replacement, a large Barnes's bag was inserted into the rectum and greatly expanded. As this was also unsuccessful, it was left in all night, and after its removal the organ was easily replaced by manipulation—possibly being then in a more favourable pelvic diameter. The patient did well for five days, but then aborted of a fetus over three months old. Six weeks after this the uterus was found retroflexed, but the misplacement could be easily rectified by the finger.

As this is not a paper on retroversion or prolapsus, but on their alternation, I have refrained from referring to authorities for the statements of anatomical facts. The best anatomical description of retroversion of the pregnant uterus is that by William Hunter. The

anatomy of prolapsus has been described, amongst others, by Dr. Savage, in his well-known work, "Surgical Anatomy of the Female Pelvic Organs."

---

*Meeting, January 13th, 1875.*

*On the Means of Ascertaining the Length of Gestation by Measurements of the Fetus and Gravid Uterus during the Second Period of Pregnancy.*

By VASSILY SUTUGIN, M.D., St. Petersburg.

(Communicated by Dr. MATTHEWS DUNCAN.)

Every objective symptom which serves to point out the period gestation has arrived at, is of great value, not only at the patient's bedside, but also from a medico-legal point of view. In spite, however, of the interest attached to such a subject, we have very few exact observations in regard to it; although in text-books on midwifery, reference is made to the size of the fetus during successive months of pregnancy, and also to the position of the fundus uteri in its relation to the symphysis pubis and the umbilicus. The size of the fetus, prior to Ahlfeld's essay,\* was determined only after birth, while the height of the fundus uteri was measured either by the fingers or by a tape measure,—a procedure which, considering the difference of height of the umbilicus in different women, furnished extremely contradictory results. The determining, by means of a tape measure, of the distance between the symphysis pubis and the fundus uteri, even in cases where the children are of the same size, is also fallacious, partly owing to the unequal development of adipose tissue under the skin in different females, partly to the varying convexity of the uterus. Our conclusions will of course be still more contradictory when the children differ in size and position. Indeed fetuses differ from each other both in length and weight during almost every period of gestation.

Elsaesser,† and after him Hermann,‡ have already drawn from the materials at their disposal the following conclusions:—"That frequently children born during the earlier periods of pregnancy surpass in length those brought into the world at a later date. Therefore it is impossible to judge at what period of gestation the fetus was born by the length of the latter. The age of the infant and its size, are far from standing in exact relation to each other; children arrived at their full time, and even those carried beyond it, may be born small in size and light in weight." "Neither the duration of pregnancy,

---

\* Ahlfeld, "Archiv für Gynaek.," Bd. ii. p. 352-373, "Bestimmungen der Grösse und des Alters der Frucht."

† Elsaesser, Henke's "Zeitschrift für Staatsarzneikunde," 1858, Bd. lxxv. (245-294).

‡ Hermann, "Monatsschrift für Geb. und Fr.-Kr.," Bd. xii. pp. 394, 395.

nor the weight of the fetus, nor its length, nor the relation between the upper and lower parts of the body, taken separately, can serve as indications of the age of the fetus; the only trustworthy signs are the degree of its development, and proofs of its body having performed vital functions." While entirely agreeing with Elsaesser that there is no *single* certain sign of the fetus having arrived at maturity, and that only all the signs taken together can demonstrate, with anything like exactitude, the age of the fetus, we cannot accept such statements as final, without also taking into consideration the results arrived at by such observers as Hecker,\* Schroeder,† and Ahlfeld, whose unanimous opinion it is that the length of the fetus serves as the surest indication of its age. The above-mentioned authors' observations were carefully made, either by themselves or by trustworthy assistants; whereas Elsaesser says nothing of the method of investigation employed by himself. Perhaps the materials he made use of were collected by midwives or by persons as little accustomed to strict observation. Now, every practising physician is well aware of the worth of observations made by such people,—although in certain exceptional cases Elsaesser's conclusions seem quite correct. Hence only accurate clinical records are valuable, and opinions founded thereon can be refuted only by a series of observations similarly made. I regret that I have been able to collect only a small number of facts in regard to infants born before the full time; but with the view of verifying, after birth, the measurements of these children taken before they were born, I consider the bringing forward of even such small data as not out of place,—data which I hope to extend as opportunity offers. In estimating the maturity of the fetus, the period of gestation as determined by the last catamenia, the corresponding size of the womb, and the general development of the new-born infant, were taken into account. As nearly all my patients were unable to define the day of conception, but only their last menses, I cannot consider myself as fortunate in my cases as Ahlfeld was in his.

---

\* Hecker und Buhl, "*Klinik de Geburtstk.*," Leipzig, 1861, 131-145; und Bd. ii. pp. 22-23.

† Schroeder, "*Gebertshülfe.*" Vierte Auflage. Bonn, 1874.

TABLE I.

Period of Pregnancy.	Weight of Child.	Length of Child.	Number of Cases.	Average Weight of Child.	Average Length.	Maximum Weight.	Maximum Length.	Number of Cases.	Minimum Weight.	Minimum Length.	Number of Cases.								
Weeks.	Grm.	Ctm.		Grm.	Ctm.	Grm.	Ctm.		Grm.	Ctm.									
44	3520	51'5	I	3438'5	49'9	4300	54	I	3145	45'75	I								
44	3200	48'5	I																
41'5	4300	54	I																
41	3145	45'75	I																
	3100	49	I																
	3575	50'75	I																
	3230	50	I																
40			108	3412	48'4	5250	55	I	2449	43	I								
39	2922	46	I	3016	47'5	4230	52	I	2200	43'5	I								
	2390	44	I																
	3020	49	I																
	5230	46	I																
	3800	49'5	I																
	4230	49	I																
	2200	46'5	I																
	2535	45'5	I																
	2725	48'5	2																
	3170	49'5	I																
	2350	43'5	I																
	3100	49	I																
	3300	52	I																
	3132	48'5	I																
	3590	48'5	I																
	3545	48	I																
	2800	46'5	I																
	2500	46	I																
38	3430	45	I	2897'3	46'6	3600	48'5	I	2350	44'25	I								
	3080	46	I																
	2950	47'5	2																
	3600	47	I																
	2350	45	I																
	2650	46	I																
	2950	48'5	I																
	2825	48'5	2																
	2559	44'25	2																
37	2650	46	I	2770	46'5	3035	48'5	I	2250	45	I								
	2720	48	I																
	3035	46	2																
	2800	48'5	I																
	2900	46'5	I																
	2250	45	I																
36	2920	47'75	2	2622	46'5	3097	49'5	I	2030	42'5	I								
	2780	45'85	3																
	2500	46'25	2																
	2750'5	47'75	2																
	2420	46'5	I																
	2301	49'5	I																
	3097	47'5	I																



TABLE I.—*Continued.*

Period of Pregnancy.	Weight of Child.	Length of Child.	Number of Cases.	Average Weight of Child.	Average Length.	Maximum Weight.	Maximum Length.	Number of Cases.	Minimum Weight.	Minimum Length.	Number of Cases.
Weeks.	Grm.	Ctm.		Grm.	Ctm.	Grm.	Ctm.		Grm.	Ctm.	
36	2030 2200 2620	42'5 44'5 47	1 1 2								
35	2500 2900 2700	46'5 46	1 1 1	2700	46'25	2900	46'5	1	2500	46	1
34	2280 2490 2550	44'5 45'5 46	1 1 1	2440	45'3	2550	46	1	2280	44'5	1
33	2270	41									
32	1850 2085 2450	41'5 42'25 48	2 2 1	2064	43'1	2450	48	1	1850	41'5	2

In regard to the earlier births, there were only a few cases, and I have therefore omitted them. For children arrived at maturity, I shall simply give the average figures, as too many details would exhaust the reader; and, further, as such details are matters of secondary importance, in so far as this essay is concerned.

For the more complete support of my measurements of the fetus, I shall cite the figures of Hecker, Ahlfeld, and Schroeder.

TABLE II.

	Period of Pregnancy.	Average Weight according to Hecker.	Average Weight according to Schroeder.	Average Weight according to Ahlfeld.	Average Length according to Hecker.	Average Length according to Schroeder.	Average Length according to Ahlfeld.
Months.	Weeks.	Grammes.	Grammes.	Grammes.	Ctm.	Ctm.	Ctm.
10	$\begin{pmatrix} 40 \\ 39 \\ 38 \\ 37 \end{pmatrix}$	2450	2528	3168 3321 3016 2878	45'47	46	50'5 50'6 49'9 48'3
9	$\begin{pmatrix} 36 \\ 35 \\ 34 \\ 33 \end{pmatrix}$	1993	2240	2806 2753 2424 2084	42'44	44'6	48'3 47'3 46'07 43'88
8	$\begin{pmatrix} 32 \\ 31 \\ 30 \\ 29 \end{pmatrix}$	1609	1700	2017 1972 1868 1576	39'41	41'3	43'4 43'7 42'0 39'6
7	$\begin{pmatrix} 28 \\ 27 \end{pmatrix}$	1343		1142	35'38		36'3

On comparing Table II. with my own measurements of new-born children, it appears that my figures approach most closely to Ahlfeld's, although infants carried to the full time are shorter in proportion to their weight with us in St. Petersburg than in Leipsic. Ahlfeld, as well as Hecker, have arrived at the conclusion that "the greater the length of the fetus, the greater the probability of the head being large." The former gives us a table of the comparative length of the child, and of the breadth of its skull. The results obtained by myself bear a close resemblance to Ahlfeld's figures. I shall therefore confine myself to a citation of my own measurements, which will be found in Table III. In constructing the following Table I have given measurements of the longer transverse diameter of the head (from the tuberosity of one parietal bone to that of the other), and of the shorter one (between the zygomatic processes). All these measurements were made by myself.

TABLE III.

*On the Relation of the Length of the Fetus to its Breadth.*

Length of Child's Body.	Number of Measurements.	Maximum Length of Biparietal Diameter.	Minimum Length of Bizygomatic Diameter.	Number of Cases.	Average Length of Biparietal Diameter.
Ctm.		Ctm.	Ctm.		Ctm.
55	1	10·8	8	1	...
54	1	10	8·75	1	...
53	3	9·5	8·3	3	...
52	9	9·2	8	9	...
51	13	9·1	7·6	...	...
50	15	9·03	7·8	...	...
49·5	10	9·0	7·9	28	8·9 to 7·7
49	18	8·8	7·6		
48·5	16	9·1	7·6		
48	21	8·9	7·6	37	9 " 7·6
47·5	5	9·1	7·8		
47	11	8·8	7·5		
46·5	12	8·5	7·1	27	8·6 " 7·1
46	15	8·7	7·2		
45·5	4	8·8	7·3		
45	4	8·5	7·4	8	8·6 " 7·3
44·5	4	8·4	7·1		
44	3	8·5	7·5		
43·5	1	8·75	7·5	7	8·4 " 7·3
43	...	...	...		
42·5	1	9	7·5		
42	1	7·5	6·5	2	8·2 " 7
41·5	1	7·5	7		
41	1	8	7		

As will be seen from the above cited figures, there is no strict mathematical relation between the length of the child and the breadth of its head ; but, in the majority of cases, the most correct conclusion is, that the longer infants have the broader heads. To ascertain

the size of the head, however, it is not sufficient to measure the short diameter, but other measurements should be taken as well.

Thus, Pfannkuch found that the circumference of the fetal head is equal to the sum total of three diameters—the transverse, the antero-posterior, and the oblique\*—and he therefore considered the circumference of the head as the expression of the degree of its development. On the basis of his measurements, he arrived at the conclusion that the weight and length of the body, and the size of the head keep pace in growth until the child arrives at its medium weight, while afterwards, during the gradual increase in weight, the increase in length first lags behind, and then the increase in the size of the head. The more rapid development of the weight goes towards enlarging the circumference of the child's body. I have myself made 167 such measurements, and 50 more my friend Dr. Stoll was so obliging as to make for me, so that my table consists of 217 measurements in all. In this table, live-born children, carried to the full time, and delivered without any extraneous aid, in cases of normally developed pelves, are taken into account. In order to strengthen the limited number of my observations, I shall take the liberty of also quoting Pfannkuch's figures.

TABLE IV.

*The Relations of the Circumference of the Head and the Length of the Body to the Weight of the latter in Newly-Born Children.*

Weight of Child in grammes.	Circumference of Head, my measurements (in centimetres).	Circumference of Head according to Pfannkuch (in centimetres).	For each kilogramme of weight a circumference (in centimetres) of	Length of Child in centimetres (my measurements).	Length of Child according to Pfannkuch (in centimetres).	For each kilogramme of weight a circumference (in centimetres) of	No. of Children measured by me (in centimetres).	No. of Children measured by Pfannkuch (in centimetres).
1500 to 2000	29·6	30·37	...	40·2	42·59	...	5	23
2000 „ 2250	31·6	30·75	14·8	43·2	44·02	20·32	8	36
2250 „ 2500	32·0	31·33	13·4	45·3	45·46	19·07	12	52
2500 „ 2750	32·3	32·28	12·3	47·2	46·81	17·98	25	90
2750 „ 3000	33·1	22·79	11·13	47·5	47·76	16·52	35	110
3000 „ 3250	33·75	33·62	10·8	47·9	48·76	15·32	48	150
3250 „ 3500	34·1	34·62	10·01	49·1	49·53	14·54	35	115
3500 „ 3750	34·5	34·27	9·51	49·3	50·33	13·00	36	79
3750 „ 4000	34·7	34·84	8·95	48·9	51·05	12·61	11	46
4000 „ 4500	35·9	35·4	...	52·1	52·46	...	11	13

\* 'Ueber die Koerperform der Neugeborenen,' "Arch. für Gyn.," Bd. iv. pp. 297-310.

My measurements coincide with Pfannkuch's—viz., that, at the commencement of each kilogram of the child's weight, there is a circumference of the head of 14·8 centimetres, and a length of the body of 20·3 centimetres. Afterwards, however, when the child reaches a weight of  $3\frac{3}{4}$  of a kilogram, there is a circumference of only 8·95 ctm., and a length of 12·61 ctm. for every kilogram. Consequently, Pfannkuch's above-cited conclusion is confirmed in so far as the average of the cases is concerned; but in many individual instances, the circumference of the head presents almost as great variations in regard to the weight as it does to the length of the body, so that for the present I am bound to agree with Hecker, Schroeder, and Ahlfeld, and regard the length of the child as the more certain indication of the degree of its development. After the infant's birth, there are within our reach a great many other signs which afford us the opportunity of judging of its development, and thus of the period of pregnancy at which labour had arrived. I have touched upon this question but slightly, as I intend passing over to the chief subject of this essay—viz., the possibility of defining the size of the fetus during pregnancy. Ahlfeld, in making a newly-born infant assume the position it occupied in utero (*i.e.*, bending its head, and crossing its arms over its breast, while its bended knees were brought up towards the abdomen, so that it presented an egg-shaped outline), found that the length between the opposite poles of the oval form was exactly one-half the length of the fetus. In proof of the correctness of his statement, Ahlfeld brought forward a whole series of measurements (250 in all) of newly-born infants. In three cases in particular, these children were covered with vernix caseosa, and it was thus easy to place them in the position they had occupied before birth. As a further proof of the correctness of his conclusions, Ahlfeld measured the frozen body of a woman in the last stage of pregnancy (case published by Braune\*), and found that the length of the oval position of the fetus equalled one-half its whole length. Ahlfeld further remarks that the bending of the child to make it assume the same position it had in utero, demands the use of considerable force. He measured a number of children in utero, and found his aphorism, that *the length of the child in the womb almost equals half its length when straightened out*, confirmed. In measuring the fetus before birth, Ahlfeld proceeded as follows:—He placed the pregnant female on an even, and somewhat hard, bed, so that the pelvis might be on a level with the upper part of the body, made her draw her knees (which were held apart) up to the abdomen; after which, he firstly exactly defined the position of the fetus, and then proceeded to measure its size. In normal presentations—*i.e.*, with the head or breech presenting, the highest point of the fundus uteri is defined by means of palpation and percussion; in very corpulent females, by percussion

---

\* W. Braune, "Die Lage des Uterus und Fetus," Leipzig, 1872.



alone. The most superior point of the fundus uteri, where the head or breech is situated, generally presents one of the poles of the oval (the outline, as already mentioned, of the position of the fetus), the other pole may be felt either outwardly through the abdominal walls, just above the inlet of the pelvis, or else directly through the vaginal pouch, which is effected much more easily, and with greater exactitude. After defining the upper pole of the oval, the point corresponding to it on the linea alba is marked off with ink. After ascertaining the position of the two poles of the fetal "oval," the points of Baudelocq's pelvimeter are placed over them respectively, and the length of the child is thus measured. The thickness of the uterine walls is insignificant, as was shown by Braune's measurements on the dead body of a woman in the last stage of pregnancy.

TABLE V.

*The Average Length of the Fetus during successive weeks of Pregnancy.*

Week of Pregnancy.	Length of Fetus.	Number of Cases.	Length of Child after Birth (my measurements).	Length of Child before Birth according to Ahlfeld's measurements.	Length of Child after Birth according to Ahlfeld's measurements.
	Centimetres.		Centimetres.	Centimetres.	Centimetres.
40	24'4	99	48'4	25'6	50'5
39	24'4	77	47'5	25'6	50'6
38	23'9	70	46'6	24'9	49'9
37	23'4	49	46'5	24'1	48'3
36	23'3	40	46'5	23'9	48'3
35	23'1	26	47'5	22'5	47'3
34	22'7	19	45'3	23'0	46'07
33	22'7	13	...	22'3	43'88
32	21'9	9	43'1	21'5	43'4
31	21'9	8	...	21'7	43'7
30	19'8	7	...	20'9	42'
29	20'0	4	38'0	20'2	39'6
28	20'0	2	38'5	19'4	40'4
27	19'0	2	...	18'9	36'3
26	19'0	1	35	17'7	...
25	...	...	...	18'3	...
24	18'0	1	...	13	...
23	18'0	1	...	...	...
22	17'0	1	...	...	...

In oblique presentations, Ahlfeld advises the straightening out of the fetus's position, before proceeding to measurement. In cases where this would be difficult of accomplishment, or in transverse presentations, he counsels the direct definition of the most prominent parts of the oval—viz., the head and breech, and, placing the two extremities of the pelvimeter thereon, measures their distance from each other. In carrying out these measurements, Ahlfeld very wisely cautions us to endeavour to avoid exciting uterine con-

traction, which is very easily produced by the manipulation necessary for a correct examination. His advice is to make our measurements only when the uterus is at rest. In measuring the fetus in utero, I followed Ahlfeld's above-mentioned rules, with this difference only, that I did not mark the situation of the upper part of the fundus uteri with China ink, but at once placed one extremity of the pelvimeter over its most superior point detectable by palpation, as I discovered that the spot marked on the skin used, during the movements of the fetus, not unfrequently to change its position in regard to the lower portion of the oval. I have myself made 409 measurements, and these were taken during various periods of pregnancy, and from once to twice weekly. In the foregoing table (No. V.) only those measurements are taken into account, where there was either a natural head presentation, or an oblique one, in which the fetus could be straightened out.

From an average of the figures brought forward, it will be observed that the measurements of the fetus before birth, very closely approach the measurements taken after birth. To elucidate the matter still further, I have cited Ahlfeld's figures, from which it will be perceived that he had to deal with children greater in length than those that came under my notice, which entirely coincides with the measurements made of children in St. Petersburg immediately after their birth. During the earlier months of pregnancy, Ahlfeld's measurements bear a close resemblance to mine. The measurement of the fetus in utero can only be undertaken when the projecting parts of the child are easily felt, and when the womb keeps its solid contents fixed—*i.e.*, from the seventh month of pregnancy. The mistakes amounted to about 18 per cent. of the measurements hitherto made; the errors arising chiefly from an excess of liquor amnii, and a puny fetus which floats freely in the fluid, and is not doubled up in the manner it would have been, with the ordinary amount of waters, in the womb.

Moreover, strict attention should be paid to the state of the uterus—*i.e.*, whether it is contracted, as the contractions of the womb produced by examination, lengthen the fetus, according to my observations, some 4 centimetres during the last three weeks of pregnancy, and from  $\frac{1}{2}$  to 2 centimetres from the thirty-fifth to the thirty-seventh week of pregnancy. At the commencement of labour, the lengthening of the fetus is still more marked. Ahlfeld found the difference as great as 5 centimetres sometimes. The contractions of the uterus which take place before actual labour commences, produce a change in the length of the fetus, so that most of the errors in measurements arise from these measurements having been made from one to five days prior to parturition. In proof of this, I shall bring forward measurements made during labour pains, at the height of uterine contraction, and also in the intervals thereof.

TABLE VI.

*Measurements of the Fetus during Parturition.*

Order of Measurements.	Length of Fetus without Uterine Contractions.	Length of Fetus during Uterine Contractions.	Length of Child after Birth.	Order of Measurements.	Length of Fetus without Uterine Contractions.	Length of Fetus during Uterine Contractions.	Length of Child after Birth.
1	Ctm.	Ctm.	Ctm.	11	Ctm.	Ctm.	Ctm.
2	26	29	26·5	12	26	28	24·5
3	26	29	24·25	13-14	25·5	28	25
4	24	26	22	15	24·5	27	24·5
5	25·5	28	24	16	25	26·5	23·25
6	25·5	27·5	23·5	17	26	28	23·25
7	25·5	27	23	18	26	28	25
8	25	27·5	25·5	19	24	25	22
9	25·5	29	25	20	26·5	28	25
10	26·5	29·5	24		27	29	24·0
	26	28	25				

Out of thirty similar observations, I found the greatest length of the fetus to be 35 centimetres—*i.e.*, an increase of  $1\frac{1}{2}$  centimetres. Generally those measurements made during labour can alone be taken into account in which the womb remains but very slightly contracted during the intervals of pain. All the measurements above referred to were made when the cervix uteri was smooth, and the os dilated from 1 to 2 finger-breadths, and before the escape of the waters. The results of these measurements agree with Ahlfeld's. The length of the fetus in oblique presentations, I have placed in Table VII.

TABLE VII.

*Measurements of the Fetus in Oblique and Transverse Presentations.*

Week of Pregnancy.	Length of Fetus (my measurements).	Number of Cases measured by myself.	Length of Fetus according to Ahlfeld.	Week of Pregnancy.	Length of Fetus (my measurements).	Number of Cases.	Length of Fetus according to Ahlfeld.
	Ctm.		Ctm.		Ctm.		Ctm.
40	24·5	9	25·5	32	21·2	2	22·2
39	24·2	9	25·37	31	19	4	22·1
38	24	10	24·6	30	19	2	20·5
37	23·6	6	23·7	29	18	2	20·3
36	22·7	8	24·4	28	...	...	20·7
35	23·25	2	24·3	27	19	1	20·0
34	23	6	23·9	26	19	1	19·0
33	22·5	2	22·8	25	...	...	17·0
...	...	...	...	24	19	1	...

In these cases I did not obtain, as Ahlfeld seems to have done, invariably high figures (*i.e.*, high when compared to the measurements made in natural presentations), but, on the contrary, observed very considerable differences in the length of the fetus. Ahlfeld very justly remarks that in consequence of the excess of liquor amnii, the child is not so much bent as where the waters are in moderate quantity, and that these children are often ill-developed. My own observations have brought me to the conclusion—

1stly, That the definition of the length of the fetus before birth is possible in the majority of cases.

2ndly, That the size of the fetus serves as a test for determining the period of pregnancy, combined, of course, with other objective symptoms.

3rdly, That by the length of the child, an approximate opinion may be formed of the size of its head.

4thly, That, in measuring the length of the fetus in utero, Pfannkuch's observation is correct—viz., that the length of the child at first keeps pace with its weight, but towards the latter part of pregnancy the progress in length is slower, so that up to the thirty-fourth week the length increases by nearly 1 centimetre weekly, whereas after the thirty-fourth week it barely reaches  $\frac{1}{2}$  a centimetre. In some cases this law is even more strikingly illustrated, but I shall abstain from citing figures, not to prove tedious.

Ahlfeld's belief, founded upon his measurements of the fetus, is, that the position of the child in utero does not depend upon the form of the womb, but that, on the contrary, the shape of the uterus depends upon the position of the fetus. This is by no means a maxim; indeed it is impossible to agree with Ahlfeld, or to understand how such a powerful muscular organ as the gravid uterus could, with the usual amount of liquor amnii within it, have no influence on the position of the fetus within. How otherwise can we explain those breech presentations which occur in consecutive pregnancies commencing with the first? This brings us to a very important question, the consideration of which would prove out of place in this article. I shall therefore simply remark that three causes—the womb, the fetus, and the waters—have a mutual influence on one another.

Ahlfeld was the first to apply the pelvimeter for measuring the height of the fundus uteri, with the view of obtaining more correct results than could be arrived at by the use of the tape measure—a measure which, owing, as has already been pointed out, to the varying convexity of the anterior wall of the womb, produced by the position of the fetus, and the unequal amount of subcutaneous adipose tissue, supplies very contradictory data when applied to different gravid women at the same period of pregnancy. Ahlfeld in his measurements places the patient on a flat bed, for the purpose of determining correctly the position of the soft parts above the symphysis pubis in their relation to the latter,—puts his index finger over the horizontal ramus of the pubes, so that the radial edge of the



finger should be on a level with the upper border of the ramus, while the point of the finger just touched the symphysis pubis. Along the middle of the finger-nail a line is drawn with Indian ink, opposite which one extremity of the pelvimeter is placed, so that its point is applied  $\frac{1}{2}$  a centimetre below the upper border of the symphysis pubis.

TABLE VIII.

*Measurements of the Height of the Fundus Uteri, and of its Breadth.*

Week of Pregnancy.	Height of Fundus Uteri among Primiparae.	Number of Cases.	Height of Fundus Uteri among Multiparae.	Number of Cases.	Average Height.	Height of Fundus Uteri according to Ahlfeld.	Maximum Breadth of Uterus among Primiparae.	Number of Cases.	Maximum Breadth of Uterus among Multiparae.	Number of Cases.	Average Breadth.
Week.	Ctm.		Ctm.		Ctm.	Ctm.	Ctm.		Ctm.		Ctm.
40	25·2	53	25·6	48	25·4	26·1	20·1	47	20·8	46	20·4
39	25	37	24·8	32	24·9	25·7	19·9	32	20·9	31	20·4
38	24·4	41	24·6	32	24·5	25·0	19·4	30	20·1	33	19·8
37	24·0	32	24·3	21	24·15	24·48	19·7	25	20·1	17	19·8
36	24·0	24	23·9	19	24·0	24·0	19·5	21	19·8	16	19·6
35	23·6	17	24·0	13	23·8	23·5	18·6	15	19·3	11	18·9
34	23·5	12	22·0	4	23·2	23·4	19·1	12	19·2	5	19·1
33	22·3	9	21·5	2	22·1	22·3	18·5	8	18	2	18·3
32	22·2	9	21·7	4	22·0	21·7	18·2	7	17·2	4	17·9
31	22·1	6	21·5	4	21·9	21·6	18·2	5	16	3	17·4
30	22·2	5	19	2	22·4	21·5	18·5	4	15	2	17·1
29	23	2	20	2	21·5	20·2	18	3	15	2	16·8
28	21·3	3	19·2	4	20·1	20·3	18	2	15·7	4	16·0
27	21·0	2	17·3	3	19·0	19·1	16·5	2	15	4	15·5
26	21·0	2	21·0	1	21·0	19·0	16·5	2	...	...	16·5
25	21·0	1	...	...	21·0	16·9	...	...	...	...	...
24	19·0	1	...	...	19·0	15·0	15·5	2	...	...	15·5
23	19·0	2	...	...	19·0	...	15·5	2	...	...	15·5
22	19·0	2	...	...	19·0	...	15	1	...	...	15
21	19·0	1	...	...	19·0	...	14	1	...	...	14
20	...	...	...	...	...	...	...	...	...	...	...
19	...	...	17	1	17	...	...	...	13	1	13
18	...	...	...	...	...	...	...	...	...	...	...
17	...	...	...	...	...	...	...	...	...	...	...
16	...	...	13	13	13	...	...	...	13	1	13

Should the highest part of the fundus uteri happen to be at the side of the linea alba, the point on a level with it in the mesial line is determined, and the measure taken by means of the pelvimeter from that point to the symphysis pubis. In my measurements, I followed Ahlfeld's method, with this difference only, that instead of marking any line on my finger-nail with China ink, I simply fix my right thumb horizontally over the horizontal rami pubis, while with the right index finger (I stand to the patient's right) I make one point

of the pelvimeter hook the fixed thumb, and press against its lower border. The thumb thus serves as an immovable point. With the left hand, the other extremity of the instrument is placed over the base of the xiphoid cartilage. The space between these two points being measured, I next place that extremity of the pelvimeter which had first been upon the xiphoid cartilage over the fundus uteri, and note the distance between it and the right thumb. For further proof that my thumb has been a steady and fixed point during the latter measurement, I again place one extremity of the pelvimeter over the xiphoid cartilage, and if I find my first and third measurements agree, it is proof of the correctness of my second. For the purpose, however, of observing to what extent the womb is enlarged, I have found it necessary to measure its greatest breadth. This was accomplished by means of the pelvimeter, the two extremities of which were placed at the sides of the uterus, and its greatest breadth noted. In head presentations, the maximum breadth of the uterus is at its fundus; in breech presentations, a little below the fundus.

It will be observed that the results of my measurements closely resemble Ahlfeld's, with this difference only, that towards the end of pregnancy the height of the fundus uteri was somewhat less in my cases than in Ahlfeld's, which exactly corresponds with my measurements of the child's length, and which may also be accounted for by the fact that I excluded from my tables all cases of narrowed pelvis, as well as breech presentations, in both of which the fundus uteri is raised higher than usual. It is evident, from the figures brought forward in Table VIII., as well as from Richelot's\* and Ahlfeld's observations, that the fundus uteri keeps ascending to the very end of pregnancy, and that there is no descent of the fundus uteri during the tenth month. This rule is applicable, not only in regard to the majority of cases, but in regard to every case individually, whether in *primiparæ* or in *multiparæ*, always provided the patient is placed, during examination, in a horizontal posture. In determining the height of the fundus uteri in the erect posture, however, the descent of the womb is clearly noticeable. I have made several measurements, which gave the following figures:—During the 40th week of pregnancy, the average height of the fundus uteri, on the patient assuming the vertical posture, was 22·5 centimetres; during the 39th week, 23·3 cent.; during the 38th week, 23·4 cent.; during the 37th week, 23·3 cent.; during the 36th week, 22·5 cent.; during the 35th week, 22 cent. As I do not attach much value to this question, my measurements were somewhat limited in number; but the general results arrived at show that on the patient assuming the erect posture, during the last month of pregnancy, the fundus uteri generally stands two finger-breadths lower than in the preceding month of pregnancy. On comparing *primi-*

\* Richelot, "Zur Diagnostik der Schwangerschaft." Königsberg, 1868. Inaugural dissertation.

*para* with *multipara*, we perceive that the difference in the height of the fundus uteri is almost inappreciable when the patient is placed in a horizontal position. In regard to the breadth of the womb, however, the difference is palpable,—viz., in *multipara* the womb is somewhat broader, commencing with the 34th week of pregnancy. This coincides with Gassner's\* observations, who discovered that a larger quantity of liquor amnii existed in *multipara* than in *primipara*. No conclusions can be arrived at in regard to the earlier periods of pregnancy, owing to the paucity of observations hitherto made. In constructing the following tables, only those cases were taken into account where there was no uterine contraction, and where the bladder was emptied. When the uterus contracts, the fundus ascends from 0·5 to 1 centimetre during pregnancy, and from 0·5 to 4 centimetres during labour.

TABLE IX.

*The Height of the Fundus Uteri in Breech Presentations.*

Period of Pregnancy.	Height of Fundus Uteri.	Number of Cases.	Maximum Breadth of Womb.
Week.	Centimetres.		Centimetres.
40	28	1	21·6
39	26·04	5	23·6
38	26·16	3	21
37	25·6	6	20·7
36	24·8	7	21
35	23·6	3	19·6
34	24·5	1	20·8
33	24·0	2	21·5
32	24	1	19
26	20	3	...

In comparing the above-cited figures to those representing the height of the fundus uteri in head presentations (Table VIII.), it will be perceived that the highest ascent of the base of the womb in breech cases is 3 ctm. at the 40th week, and from 0·5 ctm. to 2 ctm. for earlier periods of pregnancy, although only normally developed pelves were included in our measurements. Consequently, in determining the height of the fundus uteri, it is necessary to pay strict attention to the presenting part, while it will only be after a much more extended number of observations in breech cases that strictly correct results can be arrived at. Moreover, one cannot ignore the considerable expansion in breadth of the womb—an expansion observed in *primipara* as well as *multipara*. In regard

---

\* Gassner, 'Über die Veränderungen des Körpergewichts der Schwangeren,' etc. "Monatsschr. für Geb. und Fr.-Kr.," xix. pp. 31, 32.

to slightly narrowed pelves, we have too few observations; these, however, going to prove that the fundus uteri is very little higher in position than in head presentations with normal pelves. For instance, at the 40th week, the height is 25·2 ctm.; at the 39th, 25 ctm.; at the 38th, 24·6 ctm.; 37th, 24·5 ctm.; 36th, 24·1 ctm.; 35th, 24·5 ctm.; 34th, 24·5 ctm.; 33rd, 24·1 ctm.; 32nd, 28·8 ctm.; 31st, 23·7 ctm.; 30th, 23·5 ctm.

In the earlier periods of pregnancy, the measurements do not differ from those of normal pelves.

With regard to oblique and transverse presentations, my observations are too limited to tabulate.

If the size of the unborn fetus be compared with the height of the fundus uteri, and with the measurement of the child after birth, it will be seen, as already pointed out by Ahlfeld, what a remarkably constant relation they bear to each other, if head presentations are alone taken into account.

TABLE X.

*On the Height of the Fundus Uteri, as compared with the Length of the Fetus, before and after Birth.*

Period of Preg- nancy.	Height of Fundus Uteri.	Length of Fetus.	Length of Child after Birth.	Period of Preg- nancy.	Height of Fundus Uteri.	Length of Fetus.	Length of Child after Birth.
Week.	Ctm.	Ctm.	Ctm.	Week.	Ctm.	Ctm.	Ctm.
40	25·2	24·4	48·4	31	22·1	21·9	...
39	25	24·4	47·5	30	23·2	19·8	..
38	24·4	23·9	46·6	29	23·	20·0	38·0
37	24·0	23·4	46·5	28	21·3	20·0	...
36	24·0	23·3	46·5	27	21·0	19·0	38·5
35	24·6	23·1	47·5	26	21·0	19·0	...
34	23·5	22·7	45·3	25	21·0	...	35
33	22·5	22·7	...	24	19·0	18·0	...
32	22·2	21·9	43·1	...	...	...	...

In exceptional cases the height of the fundus uteri equals the length of the fetus; in the great majority of instances, however, if we exclude all cases of an excess of liquor amnii, which, in proportion to its quantity, lifts up the fundus uteri, it will be found that the height of the base of the womb is equal to one-half the length of the fetus. In determining, with a tape measure, the distance between the fundus uteri, and the symphysis pubis, the average figures point to the gradual increase in size of the womb (Richelot, p. 16, op. cit.); but for that the differences in individual cases are too great, when compared with the slight variations in the measurements taken with the pelvimeter. Thus, according to my own observations, in 281



cases, measurements were made with the tape measure, and also with the pelvimeter, and the variations obtained with the tape measure extended from 2 to 13.5 centimetres,—*i.e.*, in the seventh and eighth months there were differences from 2 to 11 centimetres; during the ninth month, from 3 to 11 centimetres; while during the tenth month, from 8 to 13.5 centimetres; whereas with the pelvimeter the variations were only from 2 to 5 centimetres, and that in a decided minority of cases; consequently three times less than in measurements taken with tape. It is impossible to take the navel as a fixed point from which to measure the fundus uteri, because the length between it and the symphysis pubis varies greatly in different individuals. According to measurements which I made, the distance between the umbilicus and the symphysis pubis in pregnant women varies from 15 to 22 centimetres, and it has therefore appeared strange to me how so cautious a practitioner as Hildebrandt of Königsberg could have advised his pupil Richelot in 1868 to resort to such measurements, while Schroeder, even in the last edition of his "Text-book of Midwifery," published in 1874, measures the position of the fundus uteri by determining how many finger-breadths there are between it and the navel.\* From measurements of the uterus made by myself, I have arrived at the following conclusions:—

1. The employment of the pelvimeter for measuring the height of the gravid uterus gives more trustworthy results than the use of the tape measure.

2. The upper border of the symphysis pubis is the most correct and unchangeable fixed point from which to measure the height of the fundus uteri, beginning with that period of pregnancy when the womb can be distinctly felt above the pubes.

3. The height of the fundus uteri above the pubes is a trustworthy objective symptom of various periods of pregnancy in normal and in reducible oblique presentations, when the womb contains only one fetus. In these cases it is necessary to note the presenting part of the fetus, and the size of the pelvis.

4. For plural pregnancies, a different scale of measurements should be adopted.

5. In non-reducible, oblique, and transverse presentations, the height of the womb cannot serve to indicate the period of pregnancy. In these cases the length of the fetus, determined according to Ahlfeld's method, will serve to point out the corresponding height of the fundus uteri.

6. In slightly narrowed pelves, the distance between the fundus uteri and symphysis pubis is almost equal to the height in breech presentations. In cases of greater pelvic contraction, we have nothing to rely upon but the state of development of the fetus as determined by Ahlfeld's method.

---

\* Schroeder, "Geburtshülfe," 1874, pp. 100, 101.

*Meeting, March 24th, 1875.*

*Cases of Fatal Hemorrhage after Childbirth.*

By CHARLES BELL, M.D., F.R.C.P.

I was requested, on the 19th May, 1872, to see Mrs Kerr, aged forty-two, a district patient of the Royal Maternity Hospital, who had been delivered of her thirteenth child at 2 P.M. On my arrival, I received the following history of her case from Mr. Jeffrey, the House Surgeon. She was married in 1853, and had had twelve children at the full time, and one miscarriage at the third month. In consequence of a congenital malformation she had always refrained from employing a midwife or accoucheur in her confinements, and had always been able to deliver herself safely in all her previous labours, until the present occasion, when, after delivering the child, which was stillborn, she failed to remove the placenta, and, in endeavouring to do so, she separated the cord at its insertion. She did not apprehend any danger, however, and proceeded to wash the child; but she was soon seized with hemorrhage, followed with syncope. A doctor was then sent for, but unfortunately he was from home. A midwife who lived in the neighbourhood was then called in, but on ascertaining the nature of the case, she, according to the custom of such persons in cases of danger, declined to interfere with it, suggesting that Medical assistance should be obtained from the Maternity Hospital. This suggestion was acted upon, but the nature and urgency of the case not being stated, a pupil was sent, who, on discovering the condition of the woman, sent for the House Surgeon, who immediately sent for me, and, in the meantime, administered a large dose of turpentine, which seemed to check the hemorrhage.

On my arrival between eleven and twelve at night, I found the poor woman in an extreme state of exhaustion, pulseless at the wrist, and quite anemic. On examination, I found the entrance to the vagina in a great measure blocked up by two condylomatous tumours about the size of the fist, occupying the labia. I met with considerable difficulty in passing my hand between them, and on reaching the uterus, which was quite relaxed, and the placenta partially but firmly adhering, I removed the portion that was detached; but from the extreme exhaustion of the patient, I refrained from separating what was adherent, and ordered her to have stimulants and a large dose of ergot. She rapidly sank, however, and died a few minutes past twelve, about ten hours after the child was born.

This case presents several points of interest, which I shall briefly refer to. In the first place, it is, so far as I know, the only fatal case of hemorrhage on record among the patients of the Royal Maternity Hospital; and although I have seen many cases of puerperal hemorrhage, this is the only fatal case I ever met with, having always found that the established means of treatment were quite sufficient to check the discharge; and had they been had recourse to in proper

time in the present case, there is every reason to believe they might have been successful ; but from the history of the case it will appear that much valuable time was lost, and the system, being unaided, was unable to contract the uterus and entirely throw off the placenta—the only true source of safety in such a case. This case gives a striking example of what women can do in trying circumstances, especially when actuated by false shame. Had this not been the case, this unfortunate woman would have had the condylomatous tumours removed in proper time, and in all probability her life might have been preserved. This case also illustrates the custom among savage tribes, in which women not only deliver themselves, but immediately afterwards proceed to wash their children, and return to their domestic duties.

The following case fully bears out the opinion I have expressed in regard to the proper treatment not having been pursued in due time in this unfortunate case.

In the absence of Dr. Keiller, I was requested to see Mrs. J., who was in labour. On my arrival I found that she had just been delivered of a daughter, after a natural and rapid labour. The placenta was retained in the uterus, but it was easily removed ; the binder was applied, and everything seemed satisfactory ; but the womb relaxed, and profuse hemorrhage came on, producing complete cessation of the pulse at the wrist for two hours, during which the uterus became contracted and relaxed several times, each relaxation being attended by a gush of blood. Ultimately, however, the uterus remained firmly contracted. I continued for five hours at the bedside of the patient, applying pressure on the abdomen with the one hand, and occasionally removing clots with the other, and irritating the internal surface of the uterus. At the same time the tincture of the ergot was given, along with first brandy, and afterwards port wine. On the following day she was wonderfully well, and I believe she made a good recovery ; but I must refer to Dr. Keiller, who then took charge of the case, for the further history of it. I am quite satisfied that had this case been treated by injection of the perchloride of iron, it would have proved fatal ; for the inevitable consequence would have been, that on every contraction of the uterus a certain quantity of the styptic would be sucked in by the bloodvessels, forming embolism. If this remedy ever proved successful, it must have been by acting as an irritant, of which there are many more safe in their character, inducing the uterus to contract, or when the hemorrhage proceeds from tearing of the cervix, which will be ascertained by the flooding continuing after the uterus has contracted.

---

*A Case of Intra-uterine Fibrous Polypus.*

By Dr. UNDERHILL.

About the middle of January I was asked by a medical man to send a dispensary student to see Mrs. Gannen, aged forty-nine, who, he said, was suffering from prolapsus of the uterus. She was accordingly seen by Mr. J. G. Brown, one of my students, who, on examination, found a swelling of considerable size in the vagina, and far up and behind it he could detect the lips of the os. I first saw her on the 30th January, and obtained the following history:—For the past six months she has been suffering from hemorrhages, which, during the earlier part of the period, intermitted for a few days or a week, but have latterly been nearly constant. At the New-year time, a tumour appeared at the genital orifice, which was described as smooth, rounded, and dark red; this caused her much pain and annoyance, and she could not sit down or walk without great discomfort and pain; the tumour soon went back, and a fortnight later she was seen by Mr. Brown. The hemorrhage was still continuing, and the patient was anemic, and exceedingly nervous. On making an examination I found the uterus lying unusually high in the pelvis, and protruding through the os, to an extent hardly if at all beyond the lips of the cervix, a smooth rounded tumour. The uterine sound passed in by the side of the tumour between four and five inches.

On the 6th February, the patient being put under chloroform, assisted by Dr. James Cumming, I seized the tumour in the vulsella, and though I used considerable force, I could not bring it down low enough to expose or feel the pedicle. I accordingly introduced the wide ecraseur, and easily removed the polypus on the table. When fresh it measured two and a half inches in length, three inches in circumference, with a thin whipcord-like pedicle of nearly three inches more. The tumour is about the shape and size of a large hen's egg, and, with the pedicle described, looks like the old-fashioned oval wood pessary, with the string attached to assist removal. When fresh it was very soft, and felt almost as if it contained a fluid. Microscopical examination showed it to be a loose-tissued fibroid, with numbers of what appear to be smooth muscular fibres, and numerous round cells lying among the fibres; it is, moreover, exceedingly vascular. No mucous membrane or envelope of any kind could be made out, the exterior boundary appearing to be some layers of flat epithelial cells. The bleeding stopped at once, and the patient made an excellent recovery.

The only point which makes this case worth relating to the Society is its history. The evidence appears to me to be quite distinct that the polypus was pushed out of the uterus, and down as far as the vulva, and was then gradually retracted till it was, at the time I saw it, drawn almost altogether within the cervix; and indeed I thought I could detect a difference in this respect between its position on the day I first examined it and on the date of the operation,



a week later. That the whole uterus was prolapsed, as might be suggested in explanation, I think very unlikely, for the following reasons :—(1) The tumour seen at the vulva by the patient was described as a simple round smooth body ; (2) When examined by Mr. Brown a fortnight later, the body was distinctly felt to project far through the os, the edges of the lips being made out lying higher up and behind it ; (3) At the time I saw it, the uterus, instead of being in any way prolapsed, lay very high up, and the polypus was contained almost entirely within it ; (4) The whole uterus was so high up that at the time of the operation I had great difficulty in applying the vulsella without injury to the vagina ; (5) Though I pulled very hard, I did not succeed in dragging down either uterus or tumour as low as the opening of the vulva.

If we look upon this, then, as a polypus which was driven down and then retracted, what explanation can we offer of the mechanism by which this was effected ? The uterine action would easily explain the first part of the process, but the retraction of a tumour of this size within the uterine, or at all events cervical cavity, is much more difficult to understand. Elasticity of the pedicle it cannot be, for the pedicle is not elastic ; the effect of muscular fibre in the pedicle itself is not sufficient, as it is very thin, and the amount of contraction necessary to pull back the tumour would be enormous. The efficient cause must be somehow or other in the muscular wall of the uterus, though I cannot conceive any form of uterine contraction capable of drawing a body up into its cavity,—or it must be in a suction power exerted by the uterine cavity, or in some modification of the retentive power of the abdomen.

Dr. D. MURRAY wished to know what ought to be done with polypi which come down during labour and obstruct the passage of the fetus. He had had such a case, and had lost the child in consequence.

Dr. DUNCAN observed that there is more than one kind of descending polypus. He had lately removed one from the cervix, which came down occasionally, in an old woman ; some with very long pedicles hang permanently outside, in others the protrusion is only occasional ; but cases which descend and then go back within the uterine cavity are not very uncommon. Dr. Underhill's case was not a perfect example of this condition. He had under his care one case in which a married lady suffered from very severe hemorrhages at each menstrual period. She was sent to him as a case of fibrous polypus, but there was no polypus to be made out when he first saw her, and the os was closed. At the next menstrual period, however, a tumour of considerable size came down, and the usual hemorrhage took place. The tumour was removed, and the patient recovered. He had seen other such cases. The polypus is expelled by uterine contractions, but he could not conceive how any form of uterine contraction could pull it back again. This was done entirely by the positive or inspiratory action of the retentive power of the abdomen,

in the same manner that air and foreign bodies are at times drawn into the vagina and bladder.

Dr. KEILLER said the os would contract after expelling the body, and would expand again when the polypus was pressed up from any cause against it from below. Such may have happened in this case.

Dr. UNDERHILL, in reply, said that his object in reading the case was to obtain an explanation of the process of retraction, and was much obliged to Dr. Keiller and the President for their remarks.

---

*Meeting, April 14th, 1875.*

*Note on Intra-uterine Craniometry.*

By J. MATTHEWS DUNCAN, M.D.

The subject of intra-uterine craniometry, or measuring the dimensions of the fetal head before birth, is surely destined to assume great importance, on account of its affording the accoucheur information of the highest value to guide him in the treatment of labour rendered difficult by deformities of the pelvis, or other cause of contraction of the genital passage, or by enlargement of the head.

It is several years since I called the attention of the profession to the remarkable neglect of intra-uterine craniometry, while the closely correlated procedure of pelvimetry was, and had long been, the subject of much investigation and experiment, and that not without valuable results for practice.

To show the practical importance of intra-uterine craniometry is so evidently a work of supererogation that I shall not enter upon it. It is the natural and now desiderated complement of pelvimetry; each, in any individual case, adding importance and interest to the other. And it is the more desiderated on account of the recognised imperfection of our best pelvimetry, an imperfection which is truly reflected in the well-known apophthegm of Dubois and of Credé on the propriety of waiting, especially in first deliveries, to see what the natural forces of labour can effect, even in a considerably contracted pelvis; and which is illustrated by the occasional occurrence, in the hands of learned and experienced men, of spontaneous delivery when even cesarean section had been expected and prepared for.

My object in this note is merely to indicate the lines of inquiry already occupied, or that should be taken up, with a view to the advancement of this matter. These lines of inquiry either lead, or have already led, to valuable results, which have a bearing more or less direct on the great object of telling the dimensions of the head of a child before it is born. But it is important to remark that mere dimensions are not all that the accoucheur desiderates, and it is to mere dimensions that I am now directing attention.

Another important quality, for the obstetrician, of the fetal head is

its mouldability, a quality in which fetal heads differ greatly. The difference is not only in degree of mouldability, but in kind, and also under pressure on different parts or in different directions. Any one who has made experiments with a view to the study of the comparative merits of version and the long forceps, will recognise the interest attaching to these qualities of the fetal head.

In stating the subject of intra-uterine craniometry, I shall first allude to those inquiries from which only a little aid is to be expected, and then advance to those which are more or less certain of being fruitful in the desired way.

We must not despise indications of the most trifling value, such as are to be derived from observation of—

1. *The race to which the parents belong.*
2. *The stock or breed from which they spring.*
3. *The size of the individual parents, and especially of their heads.*

Much interesting knowledge is, it is well known, already accumulated as to the cranial capacity of different races of men. But, so far as I know, the observations have been confined to adults; and, while these are, no doubt, of value in reference to the matter on hand, it would be more important for us to have measurements of the fetal heads of different races. I may remark that we already possess many contributions to the healthy pelvimetry of different races.

The influence of the stock or breed from which the parents are sprung, and of the size of the individual parents, and especially of their heads, is matter of general knowledge. This kind of influence has been hitherto clearly observed only in some of the lower animals, in which there are more remarkable variations according to breed and in individuals than in the human race, and in which the observations have included both parents. Indeed, although few would express any doubt as to the influence of stock or breed or family, and of individual parents, yet the subject cannot be said to have been in any degree scientifically examined. There can, however, be no doubt that it is well worthy of careful scrutiny.

Gassner and Frankenhaeuser have indeed shown the very interesting direct relation between the weight of the mother, the size of the mother, and the weight and size of the fetus; but their observations need to be corrected or added to by taking into consideration the male as well as the female parent, and the former has hitherto been altogether omitted from the inquiry.

The next points to which attention must be paid are—

4. *The age of the parents; and,*
5. *The number of the pregnancy.*

On these matters it is well known that much labour has been bestowed by Hecker, Veit, Frankenhaeuser, Castell, Wernich, Cohnstein, Schroeder, and others; the general opinion being, that weight and length of the child and size of the head increase with the age of the mother, and with the number of the pregnancy; but I

have never formally disavowed my own opinion as to a climax and anti-climax in the measurements of the fetus, rising from immaturity to maturity of the mother, and falling from maturity to elderliness, and rising from a first pregnancy to a higher number, and then falling off with an excessive number.

We now come to a means of arguing as to cranial measurements, which is familiar to all, viz.—

6. *The period of pregnancy.*

Every one is familiar with the anatomical basis of the practice of induction of premature labour, and with the actual measurements by Figueira and many others of the sizes of fetal heads, and especially of the most important bi-parietal diameter, at different periods of pregnancy.

7. *The sex of the fetus.*

This can seldom be made out before its birth, even during labour. In the rare cases where it can be so, its significance will be easily appreciated. Every one knows the measurements and observations of Clarke, Quetelet, Riecke, Simpson, and their followers, which show the greater size of the male than of the female head, and the greater difficulty and danger of its birth.

The value of the rate of the fetal heart's pulsations, as pointed out by Steinbach, Frankenhaeuser, and Cumming, must not be altogether forgotten. So far as the rate can be held to indicate the sex, it may be held as contributing to our information under this head.

We reach now methods of more exactness, which have only recently been introduced, and which have still to be perfected. Even as they at present stand, however, in their youthful feebleness, they are far superior to the presumptuous method which we often follow, and which is truly only the rash guessing of men not so ignorant as to be insensible of the advantage of accurate knowledge.

8. *The size or length of the fetus.*

The recent works of Ahlfeld and Sutugin show that the length of the fetus can be, with considerable accuracy, made out during pregnancy. The researches of Hecker, Schroeder, and Ahlfeld show, that the length of the fetus is a better criterion of its development than its weight. Hecker, Ahlfeld, Pfannkuch, Sutugin, and lastly Fehling, have shown that the development of the head, as measured by its horizontal circumference, increases in some kind of direct ratio with that of the body. The length of the fetus is thus shown to give us a means of estimating the size and development of its head. To this deduction there is, as Fehling points out and illustrates by example, a remarkable exception in the case of twins.

9. *The size of the anterior fontanelle.*

This can also be ascertained occasionally in an uncertain way during pregnancy, through a thin lower uterine segment, and, at any rate, in the early part of labour. Now, Fehling has recently shown



in an admirable paper, that this fontanelle increases in size during the latter part of pregnancy, the part in which the operating obstetrician is most interested ; and he points out that its size may be used as an indication of the general fetal development, its smallness as an indication of the opposite. His interesting observations form a remarkable addition to those of Elsaesser, on the increase of this fontanelle for some months after birth.

10. *The length of the sagittal suture.*

The importance of this has been pointed out by Schroeder ; but, so far as I know, he has made no measurements with a view to its being utilized in practice. A great improvement is made in this measurement by Fehling, who calls it the distance between the anterior and posterior fontanelles, and takes it up from the upper angle of the posterior fontanelle to a line crossing the anterior fontanelle, and joining the two coronal sutures. Fehling's observations show a kind of direct relation between this dimension and the length of the child.

11. *Measurement of the fetal head through the uterus and abdominal wall.*

This is a plan which I have attempted, but without sufficient repetition and care to enable me to say what value it may possess. I trust, however, to be able, ere long, to lay before the Society careful observations made to determine this point. It is a matter of course that such observations will be easier carried out in advanced pregnancy in cases of contracted pelvis, when it is most desiderated, than in ordinary pregnancies, especially the first, in which the head sinks deeply into the bony cavity. In these latter, however, some combinations of internal and external measurements may be available.

In cases of hydrocephalus the value of this plan is well-known, and has no doubt been realized by practitioners who have met with these not very rare cases.

12. *Measurement by the forceps.*

It is easy to remark that the blades of the forceps will be more separated in proportion as the head is large, and that the handles are separated exactly in proportion to the distance between the blades. As the practitioner can determine what diameter of the head he seizes, and how much the head occupies the belly of the instrument, he can by their application determine the size of a diameter of the head, and by reapplications he may arrive at the determination of other measurements. I have often received good indications from this plan, and in one case was led by it to recognise hydrocephalus ; and in a report of a lecture by Carl Braun, I find he has availed himself of this expedient to measure a hydrocephalic head.

It is at once evident which of these plans are available during pregnancy, which during labour, and which in both states. It is also evident that they are all as yet very imperfect and little reliable resources, whether adopted singly or in combination. It is in the latter way, no doubt, that the practitioner should proceed.

Investigators, however, should not be discouraged, for they may reflect on the imperfect and unsatisfactory character of pelvimetry, even now after so much learning and ingenuity have been expended upon it. Practitioners, also, will not despise the small beginnings, for they will be glad to have the value and precision of pelvimetry eked out by the use of intra-uterine craniometry.

---

## Obstetric Summary.

### *Case of Cesarian Section Post-mortem.*

Dr. Pinard (in *Gazette Obstetricale*, August 5th 1875), relates an instance where a cerebral tumour of hemorrhagic origin occurred in a primipara, aged twenty-two, producing epileptiform convulsions, and subsequently death. Cesarian section was performed five minutes afterwards, the child being resuscitated with some little difficulty. It lived for three hours.

---

### *Metrorrhagia arrested by the application of Heat to the Lumbar Region.*

Dr. Noel Gueneau de Mussy (in *Annales de Gynecol.*, July, 1875) narrates the history of two cases where hemorrhage was arrested by this means after various other methods had been tried in vain.

The cases are too few to form any definite conclusions from, but he regards them as interesting, and sufficient to suggest further investigation.

---

### *Influence of Chloroform upon the Fetus in Utero.*

Dr. Zweifel (*Bull. Gén. de Thérapeutique*, July 30th, 1875) states that he has found chloroform in the urine of the newly-born infant when the mother had inhaled the vapour during parturition. He also found chloroform in the placenta where the patient had been lightly chloroformed for a quarter of an hour. It seems to be demonstrated then that chloroform inhaled by the mother passes into the blood of the fetus. This fact should influence practitioners in administering chloroform during parturition.

---

### *On the Treatment of Tubal Pregnancy.*

Dr. Thomas (in the *New York Medical Journal*, June, 1875), narrates a most instructive case, and offers some valuable clinical suggestions. A married lady, aged thirty, sterile for six years, menstruated on October 25th, 1874. Nausea and vomiting occurred within three weeks. Severe "cramp" came on during the night in the left iliac

fossa, increasing in severity until it became agonizing in the middle of January, 1875.

Hypodermic injections of morphia relieved the pain, but had to be frequently repeated to allay the severe attacks which occurred every few days. On February 4th—fourteen weeks from the last catamenia—the patient's condition was very distressing: she was emaciated and in constant dread of a recurrence of the cramp.

On vaginal examination the uterus was found to be larger and less mobile than normal, a tense, elastic cyst filling up the whole iliac fossa and dipping into the pelvis, pushing the uterus over to the right side. Ballottement was detected by Dr. Thomas. On February 7th the patient having been etherized was placed in the left lateral position, and Sims's speculum introduced. The uterus was then steadied by a tenaculum in the cervix, and with the platinum knife of the galvano-caustic battery, which was brought to a white heat, the vaginal roof was slowly cut through, the fetus was extracted, and the cord cut, no blood having been lost thus far. Gentle traction and detachment of the placenta was then attempted, but was desisted from on account of hemorrhage when a little over half had been extracted. A solution of the persulphate of iron was then injected into the sac, and the flow of blood was instantly checked.

Symptoms of septicemia came on about the fourth day, but were combated by frequent injections of carbolized water. Slight hemorrhage from the sac occurred on the seventh day. On the fifteenth day the remaining portion of the placenta came away spontaneously. Six weeks afterwards the vaginal opening had completely closed.

The patient convalesced perfectly.

---

### *Triple Birth.*

Dr. Moussons (*Arch. de Tocologie*, August, 1875), narrates the particulars of a case where three living female children were born prematurely at the eighth month. The first presented by the head, and was delivered by the aid of forceps as the pains were so feeble. The second presented by the breech, and the third by the head. The placenta formed a single mass, weighing over  $2\frac{1}{2}$  lbs. There were three cords inserted in three different ways—the one in the centre, another on the margin, the other upon the membranes about three fingers' breadth from the placental edge.

Vascular communications united one of these cords to each of the other two. There were three amniotic bags, one unique chorion apparently enveloping all three. The development of the children accorded with the insertion of their umbilical cord into the placenta, that attached to the centre being the most vigorous. The children were living a month after birth. The patient did well.

---

*The Arrest of Post-partum Hemorrhage by Compression of the Abdominal Aorta.*

Dr. Dujardin-Beaumetz, in *Bulletin Générale de Thérapeutique*, May, 1875, relates an instance of this, where the result was very satisfactory. Whilst examining the perineum of a patient, which had just been torn during delivery by the forceps, a sudden gush of blood ensued, and, but for the prompt compression of the aorta, would in all probability have proved fatal. Pressure was kept up for a quarter of an hour, when the uterus contracted, the hemorrhage being speedily arrested, and the patient making a good recovery.

---

*On Extra-Uterine Peritoneal Pregnancy.*

Professor Depaul (in *Archives de Tocologie*, July, 1875) brings his remarks on the diagnosis and treatment of this condition, which have extended over a series of eight articles, to a conclusion.

On account of the uncertainty of our diagnosis in the early stages, before the fifth month surgical interference can rarely be usefully resorted to. It is better to operate at the commencement than the end of the ninth month, where the fetus is still living.

Nine cases, collected from various sources, are mentioned in which gastrotomy was performed—seven of the infants having been extracted living, four of the mothers having recovered.

After the death of the fetus serious complications may arise, such as rupture of the cyst and fatal peritonitis, inflammation and suppuration in the cyst, with septicemia as a consequence.

Four illustrative clinical cases are given in detail. Extraction of the fetus may be made through the abdominal wall or through the vagina, depending upon the situation of the cyst.

The bistoury is better where the fetus is living, but the cyst may be opened by the successive applications of caustic where the fetus is dead, or it is feared the cyst is not adherent to the abdominal wall.

Where rupture occurs spontaneously, the opening generally needs to be enlarged and the cyst injected with disinfectants; care being taken not to extend the incision beyond the line of adhesion.

The danger of hemorrhage from cutting down on to the placenta is avoided where caustic is employed; and the placenta, being left to come away spontaneously, also lessens the risk, provided the opening be kept patulous and the sac be frequently injected.

---

Gynecic Summary.

*Rupture of Ovarian Cysts.*

Dr. G. Nepveu (in *Ann. de Gynecol.*, July, 1875) contributes a most valuable article upon this subject, giving reference to no less than 155 cases, 128 of which ruptured into the peritoneal cavity,



11 into the large intestine, the others perforating the abdominal wall, bladder, vagina, and uterus. Of 127 of these cases, 63 proved fatal. The various modes of dealing with this complication are briefly noticed, the author contenting himself with indicating the different methods without committing himself to recommending any one plan.

To any one interested in the subject the memoir cannot fail to be of much interest.

### Pædiatric Summary.

#### *Treatment of Whooping Cough by Carbolic Acid Inhalations.*

Dr. Ortille (*Bull. Gén. de Thérap.*, July 30th, 1875), on the assumption that there are parasites present, employs inhalations of this acid during the paroxysms. At the same time he employs belladonna, hyoscyamus, ipecacuanha, and other remedies. The three chief indications being:—

1. To attack directly the cause of the evil by inhalations.
2. To combat the nervous excitation of the respiratory apparatus by antispasmodics, at the head of which he places belladonna.
3. To sustain the forces of the invalid by a tonic régime appropriate to its age, and to place it in the best hygienic conditions possible.

By this means he almost invariably abridges the duration of the malady to three or four weeks, and avoids complications.

### BOOKS, PAMPHLETS, AND PAPERS RECEIVED.

“Clinical Thermoscope and Uniformity of Means of Observation.”  
Two Notes by Edward Seguin, M.D. New York. 1875.

Communications from Dr. Madge, Mr. Lawson Tait, Dr. J. R. Beck, Indiana, whose request shall be attended to, Dr. Edis, Mr. Cullingworth, Dr. James Braithwaite, and Dr. James Young.

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.

# THE OBSTETRICAL JOURNAL

OF  
GREAT BRITAIN AND IRELAND.

---

No. XXXI.—OCTOBER, 1875.

---

## Original Communications.

---

### CASE OF CONGENITAL DEFICIENCY OF THE PERITONEUM, RESULTING IN INTESTINAL OBSTRUCTION AND SIMULATING AN ABDOMINAL TUMOUR.\*

By LAWSON TAIT, F.R.C.S.

Surgeon to the Birmingham and Midland Hospital for Women ; Consulting  
Surgeon to the West Bromwich Hospital, &c. &c.

IN the *Dublin Quarterly Journal of Medical Science* for February, 1869, I published a short paper detailing some curious anomalies which I had met with in serous membranes, including a case in which the peritoneum had been very insufficiently developed.

The appearances in that case have so close a resemblance to those of the case which is more immediately the subject of this paper, and, moreover, descriptions of such anomalies are so meagre and rare, that I trust I may be allowed briefly to narrate them here.

The patient had died of a purpuric affection, which had no relation to the peritoneal peculiarities.

“On opening the abdomen by the usual incision, we were struck by the remarkable appearance of the intestines. They presented an appearance exactly similar to that seen when

---

\* Read in Abstract before the Royal Medico-Chirurgical Society, May 25th, 1875.  
No. XXXI.—VOL. III. H H

the convolutions of the brain, with the arachnoid over them, are exposed by laying open the dura mater. The peritoneum was perfectly transparent, free from the slightest appearance of inflammatory action, and instead of being in relation to the whole circumference of the walls of the small intestines and dipping down to the posterior abdominal wall to form mesenteries, it passed from one coil to another, being in relation to about a third only of the circumference, and disposed exactly as the arachnoid is to the sulci of the brain. It formed a mesentery at no point of the small intestines, nor, indeed, elsewhere. There was no appearance of the great omentum.

“Tracing the peritoneum from the umbilicus upwards I found that it left the parietes at the level of the edge of the liver, to be continued at once over the lower surface of that organ, as far as the portal fissure; and behind the vessels and duct, entering and emerging there, it turned as usual and formed Winslow's foramen. The upper surface of the liver and its posterior edge had no relation whatever to the peritoneum, but were attached immediately to the diaphragm by the ordinary loose sub-peritoneal connective tissue; while the only ligament the organ had was an unusual anterior-marginal single layer of membrane, in which, however, was embedded the usual round ligament. The lesser cavity of the sac was extremely limited in extent, and its capacity was not greater than would contain an orange. At the lower and posterior part it had relation to the head of the pancreas to the extent of about a square inch, and a still slighter connexion with the duodenum; superiorly it had a limited relation to the under surface of the liver, whilst anteriorly it covered about one-third of the posterior wall of the stomach. From the anterior wall of the stomach the peritoneum proceeded directly to the anterior wall of the transverse colon, the gastro-colic omentum being represented by about half an inch of a single layer of membrane. From the colon it passed to the small intestines, being related to them as before described, and it left them again to be related to the anterior abdominal wall, three inches above the fundus of the bladder. The diaphragm at no point was in actual

contact with the peritoneum. There was no gastro-splenic omentum, as the spleen was entirely divested of serous covering, and lay at some distance from the peritoneum, as also did the kidneys and ureters. Neither the cecum, ascending colon, descending colon, nor sigmoid flexure, had any serous covering. The bladder had no relation to the peritoneum. The testicles were both in the scrotum, and had the usual tunica vaginalis."

On January 21st of this year, I was called to see Miss M., aged twenty-five, in consultation with Dr. Hickinbotham and Mr. Pugh, of Nechells, and the following history of the case was given to me :—

Mr. Pugh saw her first on the 3rd of January, and found her suffering from constant sickness, the vomit being of a greenish colour, and of the consistency of lumpy porridge. She had been in good health up till a week or ten days previous, when she began to return her food, for which symptom she had been treated by another practitioner with a mixture of bismuth and hydrocyanic acid.

Mr. Pugh found that her bowels had not been moved for about ten days previous to his first visit, and he discovered a doughy tumour, of the size of a man's hand, in the left hypochondriac and lumbar regions. This tumour could be freely grasped, and the hand passed partially under it without the patient giving any indication of pain. She stated that once before she had had a long attack of constipation, and that one of her sisters, who had since died of consumption, had also suffered in a similar way. She had no further general symptoms of any note.

Mr. Pugh prescribed a draught of compound senna mixture, with the result of inducing a slight watery evacuation, but for the succeeding week both the vomiting and constipation resisted all his efforts.

She was then seen by Dr. Hickinbotham, in consultation with Mr. Pugh, and both gentlemen state that the abdomen was free from pain on pressure, that it was rather flat, and that an irregular and not well-defined tumour, of a doughy consistency, occupied the left side of the abdomen. This tumour was not in the least painful on pressure. O'Beirne's



tube was passed well into the rectum, and by injections of gruel a few *scybalæ* were brought away. This was repeated during the second week at intervals, but without any marked result. A small tumour was found to be bulging high up in the rectum on the 19th, and this gave rise to "some doubt" as to whether the tube had passed above it. Some slight symptoms of cerebral disturbance also showed themselves, but there was no pyrexia.

On the 21st I found her to present a somewhat dazed look, answering questions with great hesitation. She was thin and of very small size; and her general appearance, together with a very tubercular family history, made me suggest a suspicion of tubercular brain mischief. The temperature however was normal, the pulse a little over a hundred, and the only symptoms she complained of were the obstinate vomiting and slight headache. I gathered from my colleagues that they suspected that the tumour in the rectum might have some influence in the obstruction, and that they both regarded the abdominal tumour as a collection of feces.

At my suggestion we placed the patient completely under the influence of ether, and readily satisfied ourselves that the tumour in the rectum was a small ovarian cyst which could readily be passed, and which could therefore constitute no element in the intestinal obstruction. Indeed, that the lesion existed further up was proved by the fact that a large injection of water from a tube undoubtedly passed above the ovarian tumour, brought away only a very small amount of feces. The abdominal tumour I found to be, as already described, not well defined in outline, and about the size and shape of a large lemon. It seemed to have an obscure sense of fluctuation, or a feeling of doughy resistance, very suggestive of feces, but which also resembled the feeling of the tumour in a case where I found on section a cystic kidney. The tumour was dull on percussion, and could not be moved very freely. Dr. Hickinbotham and Mr. Pugh were both of opinion that it had altered a little in size since their last examination of it, having become somewhat less.

I could not advise any improvement in the treatment

which had been so well carried out by my colleagues, neither could I form any clearer notion of the nature of the abdominal tumour. The patient died on the 24th, and on the evening of the same day Mr. Pugh made an examination of the body, at which Dr. Hickinbotham and I were present.

The following notes of the conditions found have the complete concurrence of all three.

In making the usual median cut, though Mr. Pugh was very cautious, a considerable opening was made into the stomach, which was adherent to the anterior parietes. A knuckle of small intestine below the umbilicus was similarly adherent, and was accidentally divided.

On drawing aside the lips of the incision there was no appearance of an abdominal cavity, there being nowhere any appearance of peritoneum. The tissue of the anterior wall seemed to run on to the stomach and small intestines, and these lay matted together, looking exactly like the convolutions of the brain, only they were not covered by any glistening membrane. On taking up two coils they were found to be readily separable, and that their union was the result of an abundance of ordinary areolar tissue, identical in naked eye appearance to what is seen on separating fresh muscles by tearing. The separation of the coils was accomplished with ease, and there was no tendency in the walls to tear or in the coats to separate. The stomach was bounded at its lower margin by normally placed gastro-epiploic vessels, but there was no trace of omentum. The liver was adherent to all the surrounding structures by the same loose connective tissue, as was also the spleen.

Nowhere was there any trace of inflammatory action, as everywhere the loose extensile tissue prevailed. Indeed, no life could possibly have withstood inflammatory action, either acute or chronic, which would have resulted in a tithe of the extent of adhesion which prevailed.

The tumour in the left lumbar region was found to be only a number of knuckles of intestine matted together by masses of loose tissue which was singularly suggestive of the tissue which butchers blow up in a leg of veal. In what ought to have been the mesentery of this mass of convolu-

tions the tissue was somewhat gristly, and the trouble to unravel the plications was somewhat greater. But the whole length of small intestine was undone, and that without a single tear being made, though there was not a fraction of an inch which was not adherent. In the part of the tube which was included in the tumour were numerous nodules of hard feces which were doubtless the immediate cause of the obstruction. It would seem as if the vermiculation, unassisted by the free movement of the intestine, had been unable to pass the numerous masses onwards, and I may describe the condition as one of passive obstruction. At no point was there any entanglement, invagination, or narrowing of the tube.

In the pelvis it was absolutely impossible to identify any organ but the uterus, from the entire absence of the usual peritoneal limitations. Even the uterus was only identified and removed by the aid of the tumour which we had regarded as ovarian.

Thus the bladder was torn open in removing the uterus, under the impression that it was some of the loose areolar tissue, and it was only recognised by the escape of urine.

After a very great deal of trouble a mass was removed, having the tumour as its centre, and only considerable dissection showed what it really contained. Nowhere was there any indication of a serous membrane; but on the anterior surface of the fundus uteri there was a small sac which seemed to have been opened, though even this may only have been made by the separation of the layers of tissue during the forcible removable of the organs from the pelvis.

Two masses close to the uterus, one on either side, when cleared of the abundant connective tissue, and laid open, proved to be the ovaries, and in the left there was the clot of a recent Graafian follicle; the ovum of which, if it ever were extruded, must have been arrested in the surrounding tissue.

A large vessel, probably the superior ovarian (spermatic) ran down behind the ovary. Over the right ovary the Fallopian tube seemed to course in a normal direction, but it became lost in a mass of connective tissue, and I could find no appearance of the fimbriated extremity. On the left side

there was an appearance of a rudimentary tube in a fold of tissue.

Between the two ovaries and behind the uterus, in the position where Douglas's pouch should have been, was a group of eight cysts of varying size, the largest being about the size of a walnut, and filled with cheesy looking stuff. On microscopic examination, especially after litmus staining, and by the use of a 1-16th objective, this was seen to be composed chiefly of regular round epithelial cells corresponding exactly with those which line the cavity of the Graafian follicle. There were also mixed up with these cells some detritus, some oil globules, and some plates of cholesterine. By carefully teasing out this cheesy stuff I also discovered a few hairs. The inside of the cyst exhibited appearances which are characteristic of dermoid development.

Three other much smaller cysts were filled with this same pultaceous matter, the smallest being not larger than a pea. The other four were all filled with clear serum, and one of them was as large as a walnut, whilst the smallest was evidently only an ordinary follicle. All were in relation to the inner end of the left ovarian mass, but the smaller cysts were absolutely part of it. I regard it as certain, therefore, that they are merely dilatations of follicles which could not rupture.

The menstrual history of the patient, as ascertained for me by Dr. Hickinbotham from her relatives since her death, gives no more marked abnormality than that about a year ago, it was more than usually profuse on one occasion.

For at least fourteen years this patient had menstruated regularly, yet in no instance could it have been possible that an ovum should reach the uterine cavity. If the ova were matured, and that is more than probable, they must have been either destroyed by the non-rupture of their follicles, or they must have been extruded into the loose tissue which surrounded both ovaries on every side and then have perished. One at least seems, however, to have retained its vitality, and to have taken on a hypererchetic action, for it had made a partially successful attempt to become a dermoid cyst. This must, I think, have taken place at least very early in life ; if,



indeed, it did not do so during the intra-uterine existence of the patient; for as I have already pointed out (Hastings Essay, "British Medical Journal," 1874), I do not see how such an effort could be made save when the action of development is at its height, as no mere access of growth would account for such parthenogenetic attempts.

The post-mortem in this case was unfortunately imperfect, for it occurred in private practice, and even the partial investigation we made of this singular anomaly took more than three hours to accomplish. We found the brain perfectly healthy. The chest was not examined.

There can be no doubt that in both these cases an arrest of development of the peritoneum took place very early in embryonic life, earlier in the latter than in the former. It is very singular that in both the abdominal organs should be so perfect both in arrangement and structure, in spite of the absence of their proper investment, though it is quite intelligible that it should be so. Had the arrest been as complete in the male case as in that of the female patient, the testicles would not have had their serous covering, but they would have left the abdominal cavity as usual. In the second case the ovaries descended as usual, and became actually more intimately associated with the uterus than they generally are, perhaps on account of the absence of the broad ligament.

It is perfectly impossible to imagine that such a condition should be due to inflammatory action, even during infantile or intra-uterine life, for that could not obliterate every appearance of mesentery, &c.; and the appearances in the first case bear so strongly on those of the second that I am forced to explain them by supposing arrest of development.

---

## A CASE OF NATURAL TURNING.

By P. B. GILES, Junior, M.R.C.S., L.R.C.P.

Formerly Obstetric Assistant University College Hospital.

CASES of natural, or "spontaneous version," are of such rare occurrence as to be worth recording. To begin with, it will be best that a definition of the term "spontaneous version"

be given. According to Dr. Barnes, "there are two varieties of spontaneous version, one in which the head is substituted for the shoulder. The other in which the pelvis is substituted for the shoulder. These varieties of spontaneous version correspond with two similar varieties of artificial turning." This is directly opposed to Dr. Denman, who uses the phrase "spontaneous evolution" to express the effort of nature to substitute the vertex or pelvis for the originally presenting shoulder. A little thought will, I think, convince the student that, though both wish to express the same act, the definition of the former is the most happy, and that the term "spontaneous evolution" should be strictly applied to those cases of shoulder presentation where delivery is brought about by a doubling up of the fetus upon itself, and where there is no substitution of either pole for the originally presenting part, which persistently remains during the whole of delivery.

On August 27th I was called at 7.15 A.M. to attend Catherine W., who, I was informed, was in labour. I arrived there a little after eight, the distance being about seven miles from our house, and found the woman sitting on the side of the bed partially dressed, having strong pains with an interval of from two to three minutes; while having her clothes removed, one of the midwives, who had attended her in previous labours, gave me the following account:—That the patient was thirty-seven, and had been married eleven years, and that this was the fifth pregnancy. The first (ten years since) was a long time about, lasting over three days; that she was ultimately delivered with forceps by a medical man since deceased, who called three times before he could deliver her, as the child was so high. The child was born dead; there was also difficulty in extracting the placenta, and flooding. The second time labour was also tedious, the child dying in the birth, and again flooding. The third was a case of twins. The first, after a long labour, was small, a footling, and born alive. Four and a half hours elapsed before the second, a vertex, was born dead. There were two distinct *placentæ*, the first coming away before the second child was born. Little or no flooding. Fourth—this was a

good time—live child, and no flooding ; occurred four years ago. Two years since, noticed that she passed less water, that her feet swelled during daytime, and that she had palpitation of the heart. Sought advice, and was told her kidneys and liver were congested. Under treatment got much better ; but during third month of this pregnancy, the symptoms returned only in an aggravated form. Soon after quickening had a fit, becoming suddenly insensible ; was found in that state and carried home ; but as she got better towards morning, did not seek advice. Did not bite her tongue. Since then face has taken to swell, and she has had four more slight fits ; the last occurred on the 23rd, when she fell down, and has not felt the child since. Was taken with pains on the 24th, and continued to have slight pains till about 11.30 P.M. ; on 26th, when she fainted, about this time, the midwife wished to have a medical man, but the woman objected, knowing that she always had tedious labours. At 3 A.M. of the 27th the waters broke, and the midwife recognised a hand. Soon after a loop of cord came down. She then sent for me. At 8.27 A.M., the woman being then in bed, I found her with great edema of the eyelids and cheeks, tongue furred, pulse between the pains 117, and intermitting at 9 and 23. The front of the chest was slightly edematous, abdomen enormously enlarged, fluid clearly in the peritoneal cavity, legs and feet pitted on pressure, and at the commencement of each pain there was a noisy escape of gas, not very offensive, which I first thought was flatus, but which I found proceeded from the uterine cavity. Heart's action feeble, but no cardiac murmur at base or apex. Fetal heart could not be detected, and though doubtless the child was dead, if it had been alive, I expect the fluid would have masked it. A vaginal examination revealed the cord, cold, small, and pulseless, the left arm lying in the vagina, rather high up, the child apparently lying on its left side with the head above and to the right of the pubes. Continuing my examination, I detected a foot high up and a little behind the right sacro-iliac synchondrosis ; rectum nearly empty. I then drew off about one and a half ounces of high-coloured urine. I roughly boiled a portion

in a wineglass, which threw down albumen. This I verified by a careful examination at home. Before proceeding to do anything, I explained to the relations the critical state my patient was in, telling them that if she bore the shock of delivery, and even rallied from that, a very few days must end her earthly career, so urgent were the renal and cardiac symptoms. Before operating, being struck with the peculiarity of the presenting parts, I told the midwife to examine; she did so, and at once stated that the arm was much higher up than when she last "tried the pains," but she could not detect the foot. Recognising what class of case I had to deal with, it was rather a struggle to let nature complete delivery, but the fact that the severe pains which had wrought this change were getting less strong, and that the woman was so spent that she must die before the uterus rallied again sufficiently to be of use, I attempted to seize the foot (a right one), and, though I tried patiently, failed. An attempt to pass a loop of tape over the ankle was also unsuccessful, when it struck me I might effect my purpose with a wire *écraseur*. This was tried, answered, and brought down the leg. Complete version easily resulted. The great size of the child made extraction tedious, and though fairly careful in bringing down the arms, the anterior or left arm broke in the attempt; but eventually I delivered a dead female child, which was very large and immensely heavy. Afterwards, when straightened out (not extended), measured  $19\frac{3}{8}$  inches, and weighed 11 lbs.  $1\frac{1}{2}$  oz. An ergot draught was given, and a little brandy and water, pressure, which was difficult on account of the ascites, applied to the uterus for nearly three-quarters of an hour, when, as the woman felt a little better, I removed the afterbirth from the vagina. There was no real loss. Again applied external pressure for half an hour, at that time, more than an hour and a quarter having elapsed since delivery and the uterus being firmly contracted, I applied a binder and gave nourishment. At the end of another hour, the woman being fairly comfortable, with a pulse of 81, I left.

27th.—The next day at 3 P.M., when seen, found woman had slept fairly, and though thirsty had taken food; said



she was comfortable, lochia natural, not offensive, had passed water freely twice, and not much afterpain. Yet there was a restlessness I did not like. Breathing 43, but no cough, or sign of pulmonary congestion; pulse 146, and temperature  $103^{\circ}\cdot 1$ ; no rigors, neither tenderness over the abdomen on superficial or deep pressure. Yet it was with difficulty I could persuade the friends that the beginning of the end was at hand. I ordered a mixture of tinct. ferri perchlo. with ammonia, and left, telling them to send over in the morning.

28th.—As I was preparing to go to her, the husband arrived, saying that soon after I left profuse diarrhea had set in, but, as she had not taken any of the medicine when it began, he did not think it was from that; did not wish her seen, but asked for some medicine to stop the diarrhea. A mixture containing tr. cardam. co., tr. kino, aromatic confection and mist. creta was sent.

29th.—Heard nothing.

30th.—8.20 A.M. Husband came to ask that his wife may be seen, as the clergyman thought she was very ill; stated that though she was better yesterday, the diarrhea had returned in the night. On my arrival I found the woman dead. In getting up in bed she had fallen back, and expired at once. Having many obstetric engagements on hand I did not see her, but learned she had enlarged a great deal the last twelve hours.

The case, although ending unhappily, is full of interest. That spontaneous version does occur more frequently than is recorded I do not doubt; still I am certain it would at least be unwise in shoulder presentations to wait for it to occur; still more so in cases where the arm comes down. Several times I have seen an attempt at spontaneous evolution has begun, but in all these cases the children have been small and dead. In this case, although the pelvis was very roomy (the former difficult labours being caused by the huge size of the children), the child was far too massive to pass that way (*i.e.*, by spontaneous evolution). It is also interesting to note the unusual development of the fetus correlatively with a maternal system so extensively diseased. Also, the seizures,

in my opinion, which were undoubtedly of uremic origin, should not have culminated during labour in a fatal attack of so named puerperal convulsions.

---

## ON ACCIDENTAL CONCEALED HEMORRHAGE, WITH CASES.\*

By JOHN BRUNTON, M.A., M.D., L.F.P.S.G., L.M.

Medical Officer to the Great Northern Railway Company ; Surgeon-Accoucheur to the Royal Maternity Charity ; Councillor of the Medical Society of London ; Fellow of the Obstetrical Society," &c.

I FEEL it to be the duty of every Fellow of this Society to put on record every abnormal case that he meets with in his practice. These records are to his fellow labourers somewhat of the same character as a chart, pointing out sunken rocks and passages difficult of navigation, is to the mariner, as well as a means of instruction, as to how to save life in case of disaster. Many of our difficult cases require for their management as much coolness and self-possession as *he* needs who leads a forlorn hope, handles a few men in battle against great odds, or extricates a shipload of human beings from the jaws of impending death. There is nothing more trying to the nerve of a medical man than an abnormal case, difficult in the extreme, and in which the death of his patient stares him in the face, and as surely follows if he blunders.

Such a feeling pervaded me when a young beginner I met my first difficult, occult, and abnormal case in a relative of my own (the first narrated below), and as I have been fortunate in meeting but few such cases in fifteen years' practice, I am induced on the occurrence of a fourth case to relate them.

I may mention that my late worthy instructor, Professor Pagan, of Glasgow University, on getting the details of my first case, told me that he had in his extensive practice met but one ; the mother had been kicked by a brutal husband, had separation of the placenta, concealed hemorrhage, and before delivery could be completed she died. Though in

---

\* Read before the Medical Society of London, April 7, 1875.

his lectures he mentioned that cases of accidental concealed hemorrhage might occur, it was really so rare an occurrence that little was said on the subject, and his successor, Dr. Leishman, in his "System of Midwifery," while he discusses the usual forms of accidental hemorrhage, is silent on *this* subject. This just shows one how valuable it is to record cases and their treatment.

It is due to Dr. Braxton Hicks, ex-President of the Obstetrical Society, to mention that he was the first to bring this subject forward at that Society, and as far back as 1860, in the second vol. of its "Transactions." He gives details of upwards of twenty cases gathered from others, and one of his own. He has entered so freely into the subject that I must refer you to his elaborate and most interesting paper. Subsequently in vol. viii. there are two more cases narrated by Mr. Mitchell and Mr. Dunn, both instructive and valuable, as well as notes of one by Dr. Greenhalgh, introduced in the after discussion. What I desire is to supplement these records by *five* more cases, because from 1866 till the present date I can find none recorded where such are usually to be found. I trust they will be acceptable, and assist in elucidating the subject.

CASE I.—In October, 1861, I was sent for hurriedly one afternoon to attend Mrs. McE., aged twenty-six, in labour with her fourth child. All her previous labours had been natural and easy. On this occasion I found her, to me at that time, in a very peculiar condition. She was in bed tossing about, her countenance was anxious, pulse scarcely perceptible at the wrist, breath cold, lips blanched. She said she was very uneasy with intense abdominal distension, sense of suffocation, oppression and feeling as if she were going to die. I made a vaginal examination and found the os about a shilling's size, membranes *very tense*, but no coloured discharge. Head presenting. I asked her if she could account for her being in this faint condition; she said, "No; my husband and I were out next door to visit some friends last night. I went to bed well enough, I got up as usual, and shortly after I began to feel faint, and here I am."

I immediately began to administer restoratives and nourishing foods, milk, beef-tea, brandy, &c. &c., and as there was no regular labour pain, but one continuous feeling of distension, with a gradual dilatation of the os uteri, I sat myself down to wait and expect. Fortunately the faintness subsided somewhat, but still the anemic appearance remained, and the same bursting dull aching. I now, about three hours after arrival, gave a full dose of ergot, waited about half an hour, and then as the os uteri was thin and dilatable ruptured the membranes, when there was a tremendous gush of blood and water, immediately the child followed and the mother fainted and nearly died; by dint of great exertion I brought her to, then compressed the uterus with both hands with all my own weight, delivered the placenta by external pressure only, and what one might call a hatful of clots. I kept up firm pressure for upwards of an hour, and had the satisfaction to find my patient gradually rally without any post-partum hemorrhage. She simply lived so to speak, with a minimum of blood in her body. The child in this case was blanched also, and of course dead.

On making further inquiry next day, I found that Mrs. McE., on her way downstairs from her friends' house, had accidentally slipped down two steps (for one) with a jerk, and this had evidently separated the placenta by shaking it off.

The placenta was cup-shaped.

The only remarks I make on this case are the notable symptoms, extreme distension, pallor, faintness, and the *tense* nature of the membranes, there being no period of relaxation as during a natural labour. I would also note how slight a cause will detach the placenta when it has neared its day of natural separation.

This patient has had, since I attended her, several still-born children, on account of fatty degeneration of the placenta, and in her last confinement in 1873, in the neighbourhood of Glasgow, died from an analogous cause.

CASE II.—On the 2nd of April, 1867, at 8 A.M., I was



summoned to attend Mrs. T., aged thirty-four, in labour with her fourth child. I found her in a faint condition and complaining of a feeling of distension in the abdomen, and other symptoms, such as are detailed in Case I.

There was the same tense membrane presenting with vertex and ineffectual pains. I had to leave her on account of having to attend the Civil Court. I said I should be back as soon as possible, I ordered beef-tea, brandy, egg, milk, &c., to be in the meantime administered, and perfect quiet in bed to be maintained. I returned about 12 noon, when I found that there was more dilatation of the os and some coloured discharge of the usual character which indicates dilatation of the cervix and os. I now thought fit to rupture the membranes when the child was soon born, accompanied and followed by a large flow of blood and clots and a separated placenta, clotted and cupped on its uterine surface. This patient set down over-reaching herself on the previous day as the cause of the dislocation of the placenta. The child was still, and the mother did well, and was some time afterwards confined by me of a healthy child.

CASE III.—Mrs. R., aged nineteen, July 1st, 1867. In labour with her first child, during the course of labour the placenta separated from its attachment near the fundus, and copious effusion of blood took place into the uterine cavity, concealed at first, but afterwards showing itself as the labour advanced. The child was still-born, and the patient though having a narrow escape of her life, ultimately did well and has been confined two or three times by me since without any untoward event.

CASE IV.—This case was reported to me by my friend, Dr. Caskie of Goswell Road. I was sent for on January 14th, 1867, at 11 P.M., to Mrs. C., a French lady, aged twenty-six years, who had usually enjoyed good health. She was reported to be in labour with her fourth child. On entering the room I found the patient in bed crying from constant aching and cutting pains in the back and loins. She said they were worse than she had had ever before, and

earnestly desired to be relieved of them. My attention was called to the great pallor of countenance and rapid feeble pulse (120 per minute). On examining her per vaginam, I found the os soft, undilated, and not affected by the pains. I learned that during the afternoon while she was walking across the room she felt something give way and drop suddenly to the bottom of the abdomen, and she nearly fainted away. She gradually recovered a little after resting in the recumbent posture, but continued to feel exhausted. About 6 P.M. she began to experience pains which increased in severity until 9 o'clock, when they began to act as powerfully as when I saw her. I ordered her to be kept in bed and have frequent draughts of tea and coffee, and I administered a draught containing a quarter of a grain of tartar emetic. I visited her again at one A.M., as I had been sent for to relieve her agonizing pain. I found her now more exhausted, and gave her some morphia, which did not do any good. At 2.30 she had copious discharge. At this time the os was the size of a crown piece and the membranes *tense*, head presenting. I ruptured the membranes, the head followed immediately, the pains altered magically, becoming more tolerable, intermittent and expulsive. Soon the head emerged, followed by a gush of coagulated blood, the body was extruded more feebly, with considerable accompanying hemorrhage. Child was still-born. Immediate sustained pressure by the hand was practised over the hypogastrium, clots continued to flow freely, and imperfect uterine contraction followed. The placenta was retained by encysting, and though partially separated, required manual interference for its removal. Some trouble arose from feeble uterine contraction and post-partum hemorrhage, yet the patient made a good recovery.

*Remarks.*—This undoubtedly was a case of accidental concealed hemorrhage, and bears out the peculiar features of the first case—viz., distension, continued suffering, *i.e.*, without interval, faintness from loss of blood, continued *tense* condition of the membranes. No external hemorrhage on first examination and onset of labour, and lastly, the large amount of clots expelled from the uterus with the feebleness of contraction, clearly manifesting serious and dangerous

internal concealed hemorrhage. It is to be observed in this case the patient felt the separation of the placenta, that in the first case it was not felt, probably because the patient went immediately to bed on getting home.

CASE V.—On Monday 15th March, 1875, Mrs. M., aged thirty-two, expecting daily to be confined of her fifth child, was very much upset and alarmed on seeing her youngest daughter have a convulsive attack consequent on whooping-cough. Mrs. M. felt faint but had no pain; for the next few days she had occasionally a cold faint feeling but no distinct rigor. She complained to me of feeling weak and she looked blanched to a moderate degree, but not so much as specially to arouse particular attention to her, more so as she continued about the house attending to her duties as best she could. I had no means of comparing her present appearance with what it might have been before the child had the fit, as I was not in attendance till that event. On the 19th and 20th Mrs. M. said, "I feel distended and as if my labour was coming on every minute." On Sunday the 21st at half-past three P.M. I was summoned on account of the onset of labour; I then found that Mrs. M. was passing small clots of blood, the liquor amnii draining off gradually. I made an immediate vaginal examination. Suspecting there might be some presentation, I passed my hand well into the vagina and swept my finger well round the os; I could not detect such presentation but a normal vertex one. The os was about a florin in size and somewhat rigid.

Labour came on slowly with a good deal of blood and water oozing away, but not sufficient to excite much alarm. By-and-by, at 8 P.M., the child was born dead, a fine boy, but with the appearance of having been dead for some days. It was partially livid, especially the lower limbs and scrotum.

With the child came a gush of thin blood, and on compressing the uterus through the abdominal walls, I found it unusually large, but not doughy. Firm pressure brought away the placenta and a large quantity of hardened clotted blood. The placenta was covered over on its uterine

surface with hard, dark clots, some of which were altering in colour, and very adherent ; while the extruded clots, each as large as a closed fist, were of the same character—firm, fibrinous, and changing colour—indicating that they had been formed for several days.

On trying my patient's pulse I found it 130 and very feeble, which immediately suggested post-partum hemorrhage; this began after I had removed the soiled clothes, and before I had put on a bandage. Dipping my hand into cold water, till it tingled, I applied it to the uterus through the abdominal walls, and rapid, firm contraction began and continued a little, with a fall of the pulse to 120 ; then more oozing followed, and was checked again by the same means, until there was complete cessation of blood-flow, and a fall of the pulse to 84. I put on a firm binder and ample pad, and the patient has done well.

This was undoubtedly a case in which the mental emotion had produced sufficient partial contraction of the uterus to cause separation of the placenta to its entire extent. It is probable—nay, most likely, that the blood-clots were in this case external (so to speak) to the membranes, for they *followed* the expulsion of the placenta, also that they were retained in situ, *i.e.*, where they were formed, else we should have had some external manifestation of hemorrhage days before delivery. As to the hemorrhage after the liquor amnii had drained off, that is easily explained, because the relaxation of the membranes, by loss of distension, permitted the liquor sanguinis and a little clot to filter downwards and out.

I would also note the condition of pulse when hemorrhage is going on, and also when it has ceased. It is a point that is too often left unobserved. In my own practice—now reaching some two or three thousand cases—I have never found post-partum hemorrhage come on when the pulse was low—I mean under 90. When over that number I make a point of “looking out for squalls;” and when hemorrhage has been severe, if once controlled and the pulse sinks, my experience teaches me that my patient is safe and may be left. But though the hemorrhage may cease if the



pulse keeps up, there is still very great danger of a fresh outbreak ; and in the majority of cases it occurs.

I have taken the trouble to go carefully over all the cases narrated by Dr. Braxton Hicks, 23 in number, then adding Mr. Mitchell's and Mr. Dunn's to my own five cases, and if I include that mentioned by Dr. Pagan, and one by Dr. Greenhalgh ("Obs. Trans.," vol. viii. p. 288), we have a total of 32.

It is remarkable that in all these cases the symptoms were nearly alike, so that on analysing them I find that the chief diagnostic symptoms are—

I. Sudden fainting and collapse, with blanching.

II. Continuance till delivery, more or less, of the same ; but such as to cause great alarm to the medical attendant.

III. The intense continuous stretching pain, which patients described as "bursting."

IV. The tense state of the membranes, if unruptured, also continuous.

V. In most cases the existence of all the symptoms of excessive bleeding without *external* manifestation by the presence of discharge of blood ab utero.

The *ordinary* cause of separation of the placenta is that it has ripened, it is prepared to fall off. Sometimes it ripens too soon and degenerates, so may be shaken off like an untimely fig by a sudden blast.

As we near the natural term of gestation, there is more danger of sudden disturbance ; weight, size, vascularity, and sluggishness of circulation are factors which tend to hasten that separation which sooner or later must inevitably occur.

As to the *immediate* causes of the separation of the placenta from the uterine surface, I have been able to find record of it in 20 cases.

(a) *Six* began with sudden spasms, producing possibly an irregular or partial contraction of the uterus.

(b) *Eleven* of the cases are due to violence (subjective) such as violent exertion, sudden missing of a step, cough, and the like.

(c) While *three* only are due to direct violence—ab externo.

Unfortunately the immediate causes in the other recorded cases are not mentioned, though other details are minute enough. Of all the cases (32) 19 died, and 13 recovered. Of the cases recorded by myself all recovered (Dr. B. 4, Dr. C. 1).

When I was narrating my first case to the late Professor Pagan, he questioned my mode of treatment—he said, “You ought, sir, to have ruptured the membranes *at once*.” To that I demurred: when he said, “Whatever you did, sir, you saved your patient.”

This little anecdote suggests the discussion of the treatment.

I quite agree with Dr. Hicks that the indication is to empty the *uterus* of its contents, but whether that should be done immediately (if possible) or later is another point.

When called to a patient who is in the condition described above—in a state of sudden collapse, faintness, &c., with little or *no labour* (as in most cases) going on—is it reasonable to interfere at once? I think not. It is better to give stimulants and nourishment, with *perfect* rest, on purpose that the blood so suddenly effused, may have time to clot and block up the bleeding vessels. If the uterus is distended *full*, as described in nearly all the cases, we cannot have much more loss of blood, for there is sufficient pressure in the meantime to arrest it, for if the blood does not escape externally there is a limit to the quantity effused. So it becomes our duty to rally the patient, and wait until such time as sufficient relaxation of the parts has taken place to enable us to deliver with ease and rapidity.

But, on the other hand, if we rupture the membranes early, as recommended in cases of ordinary accidental hemorrhage, where there is partial placental presentation, we shall only make the original wound bleed afresh (and it was on this point Dr. Pagan disagreed with me). We are then doing the opposite of that which is indicated in ordinary accidental hemorrhage. In the latter we desire the presenting part of the child to press on the bleeding surface, and this is attained by causing the uterus to contract on the

fetus, and so press it downwards as a wedge on the placenta. But in the former kind of hemorrhage, the moment the membranes are ruptured the tension (of distension) is abstracted, and the placenta being relieved separates further, and fresh bleeding results, with fatal rapidity in several of the cases narrated.

Now my view is supported by the result of my treatment, and also by this remarkable statement by Dr. Hicks ("Obst. Trans.," vol. ii. p. 77):—

"How far . . . . the rupture of the membranes can be relied on, cannot be gathered by the details now before us, for in them we find that in nearly the whole of those that died the membranes had been ruptured," but an important little word is omitted, which I now add after carefully studying the cases, and that word is "*early*."

I quite hold with rupture of the membranes, but not till the *fit* time, of course to be judged of by each one in his own case.

In three of the successful cases narrated by me, the membranes were not ruptured till some hours after the summons to attend, and when ruptured the child followed immediately.

Another objection to early rupture of the membranes, is that the portion of uterus to which the placenta is attached is prevented from contracting by the presence of blood clot, and membrane. So that though the tension, distressing to the mother, may be relieved by the rupture, we have no contraction of the placental part of the uterus, with consequent narrowing or closure of the sinuses, but instead a formidable tendency to fresh bleeding.

In three of the cases narrated by Dr. Hicks death was almost immediate, from excessive loss. Cesarean section was performed without favourable result.

It has been stated that the early evacuation of the waters will hasten labour, and this is the reason given for early rupture of membranes; from the experience of many cases of ordinary and extraordinary labour I am of quite an opposite opinion, and I think my statement will be borne out by those who have extensive midwifery practices.

Bandaging the patient after evacuation of the liquor

amni is good practice. I have not required it, as I trust to the compressing power of my left hand, especially when aided by the use of cold.

One may adopt the use of Dr. Barnes's bags, as I have pointed out that one of the diagnostic signs is continued tenseness of the membranes. I should prefer to use the bags, before rupture of the membranes, just as a substitute for what the membranes ought to do, but *do not*.

Then as regards version, I look upon it as quite unjustifiable. The state of affairs is quite different from *placenta prævia*. You may, in *placenta prævia*, pass in the hand and remove the placenta, or rapidly bring the fetal body to press on the bleeding surface; but in accidental separation at or near the fundus the very operation of version would put the placenta and clots to the risk of being further dislocated in the act, besides the possible delay in extraction would give time for increased and, may be fatal, hemorrhage.

In some cases, if possible, it is desirable to use the forceps, but the same caution must be used; let the patient rally a little before sudden evacuation—for the shock of such operation sometimes proves fatal, without hemorrhage, and much more so if it does exist. I would also recommend transfusion in serious cases; it would be useful in rallying the patient, and thereby we could save time, or, at all events, we should have time to spare, till the uterus became sufficiently relaxed to enable us to effect delivery without excessive danger.

“The infrequency of concealed accidental hemorrhage makes it more important that every case which occurs should be fully reported” (loc. cit. 78).

---



## Reports of Hospital Practice.

---

### ST. MARY'S HOSPITAL, MANCHESTER.

#### CASE OF EXTRA-UTERINE FETATION.

*Under the care of MR. CULLINGWORTH.*

ELLEN D., aged thirty-six, married, was admitted into St. Mary's Hospital, Manchester, April 20th, 1875.

Though married nearly eleven years, she had never been pregnant up to the occurrence of the present symptoms. She says that the catamenia were due on the 7th September, 1874, and failed to make their appearance, then and subsequently. About the time when she ceased to menstruate, she first noticed some swelling of the abdomen; the size of the swelling has gradually increased to the present time. Since September of last year she has suffered from time to time from severe paroxysmal pains in the lower part of the abdomen. Once or twice she had a discharge from the vagina of the appearance of dirty water, with a slight admixture of blood. Three or four weeks ago she says that she became generally puffy and swollen over the face and whole of the body; the greater part of this edema quickly subsided. From that time, however, all her symptoms became aggravated; she lost flesh, became very weak, and suffered from pain and tenderness of the abdomen, with nausea and vomiting. On the 4th of January, fifteen weeks before her admission, she consulted Mr. G. Morley Harrison, who told her she was pregnant.

The patient was a native of Bristol, and came to reside in Manchester soon after the age of twenty. Shortly afterwards she began to suffer from epilepsy, in consequence of which she had to cease working in the factory some years ago.

On admission, she wears a suffering aspect; her face is pale, with a circumscribed flush on the cheek; she walks with difficulty, and is much emaciated. There is very slight edema of the legs and feet, the skin of which is dry, harsh,

and over-sensitive. The temperature in the axilla at noon on the 22nd was  $102^{\circ}5$ ; pulse 123; respirations 34. No cough, but much tiresome expectoration of viscid mucus. Tongue slightly furred and moist, except down the centre, where it is dry and red, especially towards the tip; appetite poor; thirst considerable; bowels constipated; urine normal in quantity,\* deep in colour, turbid, of strong odour, sp. gr. 1022, slightly albuminous, containing, under the microscope, bladder-epithelium, but no kidney *débris*.

The abdomen is considerably enlarged, the fulness being most marked towards the left side, and extending upwards to midway between the umbilicus and ensiform cartilage. There is intense tenderness over the whole enlargement, especially on the left side, and the patient is in constant pain. From time to time such severe exacerbations of pain occur that she cries out. The swelling is smooth and even, and everywhere dull on percussion; a resonant note is obtainable at the extreme left flank, and over a much larger area at the right flank. Fluctuation can be felt to a limited extent. The following are the measurements:—Girth at umbilicus, 31 in.; girth of left half of body at umbilicus,  $15\frac{3}{4}$  in.; girth of right half of body at umbilicus,  $15\frac{1}{4}$  in.; distance from umbilicus to pubes, 6 in.; distance from umbilicus to ensiform cartilage,  $6\frac{1}{2}$  in.; distance from umbilicus to left anterior superior spine of ilium, 8 in.; distance from umbilicus to right anterior superior spine of ilium, 7 in.; upper limit of dulness on the left side,  $1\frac{1}{4}$  in. below ribs; upper limit of dulness on the right side, 2 in. below ribs; upper limit of dulness in middle line,  $3\frac{1}{2}$  in. above the umbilicus.

The liver dulness extends from an inch above the nipple to an inch above the lower edge of the ribs.

The *areolæ* of the nipples are very dark, and milk can be made to exude from both breasts.

On vaginal examination, the mucous membrane of the vaginal canal is soft and tender; the cervix uteri lies high

---

\* The quantity of urine passed on the 24th was 35 oz.; on the 26th, 44 oz.; and on the 27th, 39 oz.

up and to the left, and is normal in size, with small circular os; the body of the uterus can be felt in front of the cervix, there being uterine ante flexion. The uterine sound, when strongly curved, passes the normal distance in a direction forwards and to the left.

23rd.—Pain very severe; temp.  $100^{\circ}9$ ; pulse 102; respirations 20. A subcutaneous injection of morphia to be given and repeated daily; and an effervescing saline mixture every 4 hours.

24th.—11 A.M. Great relief from the morphia; slept the greater part of yesterday and through the whole night; temp.  $101^{\circ}$ ; pulse 106.

25th.—12.30 P.M. Temp.  $101^{\circ}5$ ; pulse 115. An exploratory puncture was made at a point three inches to the left of umbilicus by means of a small syphon trocar; three ounces of dark red, turbid fluid were withdrawn, of sp. gr. 1015, slightly alkaline in reaction, containing, under the microscope, many pus corpuscles and some blood globules. My colleagues and myself now concluded that we had to deal with a suppurating cyst, probably ovarian.

26th.—Temp.  $102^{\circ}6$ ; pulse 120; catheterism required.

27th.—Temp.  $100^{\circ}5$ ; pulse 114; resp. 24; catheter not necessary.

28th.—Temp.  $100^{\circ}2$ ; pulse 100.

29th.—Temp.  $99^{\circ}5$ ; pulse 110. By means of a long curved needle, set in a handle, a drainage tube was passed through the tumour, the needle entering two inches and a half to the right of the median line, and two inches below the level of the umbilicus, and emerging four inches and a half to the left of the median line, and on the same level as the aperture of entrance. The apertures thus made were seven inches apart.

30th.—A little thin purulent fluid is draining off continuously; temp.  $101^{\circ}5$ ; pulse 106.

May 2nd.—After becoming, during the last day or two, very scanty, the discharge assumed this morning an abominably offensive odour, and increased somewhat in quantity; to the unaided eye the appearance of the discharge was the same as before.

4th.—Less discharge ; temp.  $102^{\circ}7$  ; pulse 104.

July 1st.—The offensive discharge has continued more or less during the past two months, and the patient is anxious for something to be done. The girth at the umbilicus is now less by five inches than on admission ; the tumour everywhere tympanitic on percussion ; temp.  $102^{\circ}0$  ; pulse 120. I proposed at a consultation that an exploratory incision should be made the following day, with a view to the removal, if practicable, of what we still considered to be a suppurating ovarian cyst. If this could not be accomplished I proposed to empty and wash out the cyst, leaving the wound sufficiently open to allow of the periodical washing of the interior of the cyst with some antiseptic fluid. This was agreed to.

2nd.—The patient having been anesthetized by means of the bichloride of methylene, an incision, three inches in length was made in the median line, extending from half an inch above the umbilicus vertically downwards. On reaching the sac, its walls were found so firmly adherent to the abdominal parietes, that all hope of removing it was abandoned. A free incision was accordingly made through the anterior wall of the sac, corresponding to the external wound, and on passing in the hand, the remains of a putrid fetus were found lying at the bottom of the cavity. The length of the fetus was 14 inches ; that of the upper extremity 6 inches, that of the lower 5 inches, and that of the head and trunk, 9 inches. The bones were almost completely denuded of their soft coverings ; none of them were, however, wholly detached, owing to their being loosely held together at the articulations by shreds of soft tissue. The contents of the thoracic and abdominal cavities were reduced to a pultaceous mass, in which the viscera were quite undistinguishable. The inner surface of the sac was firm, and to all appearance healthy ; no trace of placenta or funis could be made out. After the extraction of the fetus, the sac only contained a small quantity of horribly offensive fluid and some *débris*. Warm water, mixed with a little Condy's fluid, was now injected by means of Higginson's syringe, until the returning fluid ceased to be turbid. A suture was placed at the upper



and lower ends of the wound ; in the centre an opening two inches long was left, and the edges of the sac and abdominal wall were brought together by a stitch on each side. A dressing of boracic lint was applied and the patient carried to bed. The urine drawn off by the catheter three hours after the operation was extremely offensive. During the afternoon the patient complained of a good deal of pain ; a morphia suppository ( $\frac{1}{4}$  gr.) was accordingly administered at 1.30 P.M., and another at 3.40 P.M. With a view to diminish the troublesome expectoration five drops of oil of cubebs were given on sugar every eight hours.

8 P.M. Temp.  $101^{\circ}2$ . One-third of a grain of morphia hypodermically.

The patient slept from 9.50 P.M. to 3.5 A.M. and awoke hungry. Three teaspoonfuls of milk and water were given, and she slept again from 3.40 A.M. to 5.55 A.M.

3rd.—9 A.M. Temp.  $99^{\circ}$ . 11 A.M. Temp.  $100^{\circ}2$  ; pulse 124 ; tongue moist ; urine scanty, deep coloured, acid in reaction, moderately albuminous. The sac was syringed with a tepid lotion of Condy's fluid, which brought away much offensive matter. After this the patient slept a good deal, and expressed herself as feeling very well. During the day she took a pint of milk.

7 P.M. Temp.  $100^{\circ}9$ . 9 P.M. Temp.  $101^{\circ}4$  ; pulse 126. No morphia was given. The patient slept fairly during the night, and drank a pint and a half of milk.

4th.—9.30 A.M. Not so well ; dyspnea and constant expectoration of viscid mucus, which, accumulating in the throat, gives rise to much discomfort. The sac again syringed with same result as yesterday. Complains of paroxysmal pain in the abdomen. An enema administered with but slight result, and a turpentine stupe ordered to be applied to the chest.

During the day the patient perspired profusely ; she took a quart of milk and four ounces of brandy.

9 P.M. Temp.  $102^{\circ}2$  ; pulse 129. Much worse ; dyspnea and abdominal pain increased. Three grains of carbonate of ammonia were ordered every four hours in a saline mixture, and a linseed poultice to the abdomen.

During the night she was restless. Her nourishment consisted of a quart of milk and a pint of beef-tea ; four ounces of brandy were also given.

5th.—9 A.M. Patient still worse, her face wearing a pale, livid, mottled appearance. Temp.  $102^{\circ}4$  ; pulse 190 ; resp. 60. Discharge from interior of sac quite inoffensive ; satisfactory action of bowels after an enema.

At 4 P.M. she improved, and became more cheerful.

7 P.M. Temp.  $101^{\circ}6$  ; pulse variable, 120 to 135 ; resp. 40 to 50 ; breathing less noisy ; dyspnea and abdominal pain less.

9.15 P.M. Temp.  $101^{\circ}1$ .

During the night she slept badly, and had a severe attack of coughing.

6th.—8.30 A.M. A little better ; pulse 154 ; temp.  $100^{\circ}1$  ; countenance has lost its lividity and mottling ; syringed the wound for first time since the 4th ; the discharge was slight, and emitted no bad odour. The two middle stitches were removed. At half-past three the dyspnea returned, and at half-past six became suddenly alarming, and remained so for fifteen minutes, the lividity being intense. At 9.30 P.M. the number of respirations was 56. The patient slept a little during the night ; bowels acted twice ; urine passed voluntarily.

7th.—9 A.M. Temp.  $102^{\circ}6$ . At half-past ten the accumulation of mucus was so distressing that a mustard emetic was administered with some relief ; this was repeated at half-past two P.M.

During the latter part of the day there was slight delirium ; and at 5.45 on the morning of the 8th she died, having survived the operation nearly six days.

*Autopsy.*—11 A.M., *July 9th.* Some post-mortem rigidity ; no evidence of decomposition ; no tympanitic distension of abdomen ; no appearance of any healing process in the abdominal wound.

The heart's cavities were normal, and its valves healthy ; a coloured clot was found in the pulmonary artery, which did not occupy the entire calibre of the vessel.

The larger bronchi were somewhat congested ; there was

no mucus in their interior. The lungs were moderately congested throughout ; at the right apex and left base were some small patches of consolidation, evidently due to lobular pneumonia. The inflammation had advanced further in the right apex, where the solidified patches presented a grey appearance ; at the left base their tint was deep red.

The kidneys were normal in size, and, to the unaided eye, healthy in appearance.

The spleen was rather firmer than usual, but otherwise healthy.

The liver was slightly congested, pale in colour, and friable in texture.

The abdominal muscles having been divided by a transverse incision, the omentum was found firmly adherent to the peritoneal covering of the sac, as also were several of the intestinal coils ; these adhesions were of old standing, and impossible to separate. There was some injection of the vessels of the adherent coils, and a very few flakes of recent lymph on their surface. No evidences of general peritonitis were observed. The abdominal wound measured 3 inches in length, and opened directly into the sac, which was very strongly adherent to the abdominal wall. The cavity of the sac measured  $5\frac{1}{4}$  inches in length, and  $5\frac{1}{2}$  inches in breadth ; its wall was very thin in places, so that it tore readily on attempting to peel off the adherent intestines. A gum-elastic catheter having been passed along the urethra, the bladder was found occupying the median line, with its fundus intimately adherent to the cyst-wall. The uterus measured  $3\frac{1}{2}$  inches in length, and  $2\frac{3}{8}$  inches in its greatest breadth ; its cavity was  $2\frac{7}{8}$  inches long ; a small sessile polypus, half an inch in length, was attached to the fundus, and projected into the cavity ; the os was circular and even. It lay to the left side, and was adherent to the wall of the sac by a few easily lacerable bands attached to the left side of the fundus ; it had evidently been dragged out of position by these adhesions. The right ovary and Fallopian tube were normal. The left ovary was normal. On passing a bristle along the left Fallopian tube at its uterine opening, it was found per-

vious to the length of  $\frac{1}{4}$  in. Within the sac an opening, large enough to admit a crowquill, was found at the left side, extending obliquely downwards and to the left, in the direction of the upper corner of the uterus, to a distance of half an inch, evidently forming part of the inner portion of the Fallopian tube. Between this canal and the shorter one leading from the uterus, the tube was for a short distance obliterated. With the exception of its innermost inch, already described as being partly pervious and partly impervious, the whole of the left Fallopian tube had become merged in the walls of the sac.

The inferior mesenteric and splenic veins were large and full.

The rectum was free from adhesions.

---

## THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

OCTOBER, 1875.

---

### THE MEDICAL STUDENT'S FINAL EXAMINATION.

AT this time medical journals and introductory lecturers unite in tendering good advice to all those about to enter the medical profession. Kindly admonitions are scattered broadcast, and he must have a dull head who cannot gather from them a large amount of useful information. It has, however, been wisely said that, the eye only sees what it brings with it, and it is therefore questionable whether the student when he listens or reads gives his attention earnestly to any thought but that of obtaining the best diplomas in the easiest way. The result of the dominance of this master idea is that he only permanently absorbs those portions of the addresses which give him useful hints in the direction he



desires. Recommendations relating to his physical, moral, or religious welfare, receive a respectful welcome, as also do the words describing the noble character of the profession he is about to enter, and the grave responsibility incurred by all those who practise it ; but what he takes most to heart is the information how he may with the greatest facility secure from examining boards the legal medical qualifications he covets. These ornamental sheets of paper or parchment once obtained, he firmly believes his success will be certain and his fortune as good as made. How far from true is this bright picture ! Armed with the most splendid diplomas Universities or Colleges can grant, he has still another examination to pass, one as important to his professional advancement as any he has undergone, as compulsory in its character and much more damning should he fail in passing it satisfactorily. Society constitutes this final examining board. Wherever and whenever the young medical man commences practice, there and then will the said investigation begin. The most minute circumstances relating to him will be considered. His personal appearance, dress, home and equipage, will all in turn be commented upon. His actions and habits will be closely watched, and any symptoms of vulgarity, intemperance, or immorality, will be keenly inquired into and duly noted. His words will be carefully weighed and any lack of wisdom or integrity in their meaning or refinement in their use will at once be thrown into the scale of condemnation. It matters not how many letters a man may legally place after his name, they will have no power in procuring him practice if he be dirty, slovenly, vulgar, coarse, intemperate, or immoral. Amongst the higher classes the examination is much more subtle and strict. It would be difficult to define the numerous social qualifications which it is requisite that a medical man should have to insure his success among the Upper Ten Thousand, but it is certain that flaws much less glaring than those just enumerated would be sufficient to cause failure. It is not pleasant to have to admit it, but the fact unfortunately cannot be denied that many men with very mediocre medical attainments have by simply possessing the necessary social qualifications obtained

large and lucrative practices, whilst others extremely learned in their profession, and lacking these attainments, have remained in poverty all their lives. Let students therefore continually bear in mind this final examination of the world and lose no opportunities of training themselves for it. They will be wise not to shun entirely the amenities and accomplishments which render society agreeable and smooth, and enliven the course of life. Study will not be retarded by a fair allowance of relaxation. Awkwardness and bashfulness in a medical man are painful to his patients and injurious to his practice. The ease and presence which he should have can only be obtained by constantly mixing with good society. Medical and social education should proceed together, and the aim of the student should be to become the highest of all created beings—a scientific gentleman.

## Abstracts of Societies' Proceedings.

### ANNUAL MEETING OF THE BRITISH MEDICAL ASSOCIATION.

*(Abstracts of Papers read in the Obstetric Medicine Section.)*

Dr. J. MATTHEWS DUNCAN, *President, in the Chair.*

*Meeting, August 4th, 1875.*

#### *Embryotomy, its Various Modes of Procedure: With Illustrative Cases.*

By A. KEILLER, M.D. Edinburgh.

Dr. KEILLER exhibited a considerable number of instruments, old and new, and drew particular attention to a modification of Zeigler's forceps which he had devised, so as to constitute a small cephalotribe.

Dr. MOIR (Edinburgh), while acknowledging great advantage from the introduction of the cephalotribe, thought it right to remark that he had operated with the perforator, small forceps, and blunt hook, without any very great difficulty, many times before the introduction of the newer instrument.

Dr. STEELE (Liverpool) said that, in his own practice the  
No. XXXI.—VOL. III. K K

cephalotribe had been but seldom necessary. It was, however, very desirable to have the most perfect possible instrument for the rarer and most extreme cases.

Dr. BASSETT (Birmingham) said that Dr. Braxton Hicks's cephalotribe had answered every requirement in his (Dr. Bassett's) hands.

Dr. ATTHILL (Dublin) was not enamoured of the new instrument introduced to the notice of the Section. He thought it too small to exert the necessary force.

Dr. GIBSON (Newcastle-on-Tyne) thought the cephalotribe ought to be much more generally used.

Dr. KEILLER briefly replied to the several speakers.

*On a Form of Accidental Hemorrhage Alternating with Watery Discharge.*

By I. HARRINSON, F.R.C.S., Reading.

Mr. HARRINSON described two cases that had lately come under his notice, in which, after a fall in one case and a shock in the other, hemorrhage occurred at monthly intervals, there being also a continuous watery discharge. In both cases, the *placentæ* were pale and soft, and there was partial separation of the membranes.

*A New Kind of Midwifery Forceps, and an Obstetric Thimble for Rupturing the Membranes.*

By DAVID GORDON, M.D. Edinburgh.

The forceps was described as possessing the following characteristics :—1. The blades are separable from the handles, and much curved where they join the shanks. 2. The shanks or stems are very close to each other, and have sockets to receive the ends of the handles. 3. The handles are curved, and imitable by a movable screw. They have nipples to glide into the sockets of the stems, with strong catch-springs to fix them there. 4. The handles, united by the screw, are easily attached to the blades, and no locking is needed.

Dr. GORDON also showed a thimble which fits the index finger, exposes the point of the finger opposite to the nail, and has a sharp point which curves over the free edge of the nail.

Dr. SWAYNE (Clifton) doubted whether the forceps would prove generally applicable.

Dr. STEELE (Liverpool) thought that the so-called improvement was really a retrograde step.

Dr. BASSETT (Birmingham) thought that the best kind of midwifery forceps was the long one with double curve.

*Ovarian Dropsy: Some Points in its Pathology and Treatment.*

By PROTHEROE SMITH, M.D. London.

Dr. SMITH called attention to the greater frequency of ovarian dropsy now, as compared with what was known of its occurrence forty years ago. He invited discussion as to its predisposing cause, and the means best calculated to arrest it. He stated his grounds for believing that, to a certain extent, the increase in the disease may find some explanation in the general abandonment of venesection in practice in this country. He advocated the adoption of blood-letting, under certain restrictions, as a prophylactic measure, when indicated by symptoms diagnostic of uterine and ovarian engorgement. He divided his paper into three parts by asking: 1. Can any reliable prophylactic means be devised to lessen the frequency of ovarian disease, by obviating *in limine* its cause? 2. What is the best manner of preparing patients for ovariectomy, and of performing the operation, with a view to success? 3. What treatment, after the operation, is best calculated to insure the recovery of the patient? Dr. Protheroe Smith hoped the answers to these questions elicited in the course of discussion would greatly enrich, if not exhaust, a theme of such grave importance. Whilst calling upon those whose more extensive experience specially qualified them to speak on the subject, he gave the result of his own observations as to several important points regarding the preparation of the patient for ovariectomy, the mode of performing the operation, and the after-treatment.

Dr. GRIFFITHS (Swansea) thought that it was very doubtful whether Dr. Smith was right in stating that ovarian dropsy was more frequent now than formerly. He also thought it was an unsupported statement that this condition was due to congestion; and as for prophylactic measures, who was to say when ovarian dropsy was impending?

*On the Prevention and Management of Miscarriages.*

By ARTHUR W. EDIS, M.D. London.

A brief allusion was made to the mortality occasioned by miscarriages, the life of the fetus being invariably sacrificed, and the mother's life often jeopardized; and not only this, but the fecundity of the female was often destroyed from the effects of uterine disorder following a miscarriage, to say nothing of the distress and suffering often occasioned. In a series of 2000 cases observed by the author, there were no fewer than 1147 miscarriages compared with 4588 children born at full time. Miscarriages were far too lightly esteemed, both by the public at large as well as by practitioners. Patients with well-marked flexion of the uterus, constitutional syphilis, chronic metritis, and numberless other affections, were allowed to go on aborting without sufficient pains being taken to prevent a recurrence. The prevention of miscarriages depended entirely upon proper measures being employed to avoid the contingency of a



recurrence of the cause or combination of causes that induced the expulsion of the ovum in the first instance. To say that a patient had acquired the "habit of aborting" was merely asserting our ignorance of the cause, and expressing in other terms the fact that the woman aborted because she aborted. As regards the management of miscarriages, the necessity of a vaginal examination was strongly insisted on. The influence of ergot in the early stage in arresting threatened miscarriages, was favourably alluded to; and the employment of carbolic acid injections where any portion of the products of conception were retained was urgently recommended. The author concluded by urging a more careful study of the subject; miscarriages being frequently the starting-point of a long course of uterine distress: dysmenorrhea, sterility, &c.—An interesting discussion followed, in which Dr. BASSETT, Dr. JOHN HADDON, and Dr. SIMPSON, joined.

*Placenta Prævia.*

By W. DONOVAN, L.R.C.P. Edin., Carrignavar.

Dr. DONOVAN said that he believed the cause of hemorrhage in *placenta prævia* to be that the part of the placenta lying directly over the os internum uteri, is unattached by blood-vessels or otherwise, and, therefore, does not expand uniformly with that part attached to the uterine walls. Consequently, during the latter months of pregnancy, the rapid enlargement of the uterus causes an amount of strain on the zonal vessels of the placenta sufficient to rupture them.

*Case of Inversion of the Uterus of three months' standing.*

By J. H. EWART, Esq., Manchester.

Elizabeth V., aged twenty-two, married, was attended by a midwife in her first confinement on March 27th, 1874. She was at full term, and the labour went on well to the end of the second stage; there was then, according to her account, some difficulty with the after-birth, which the midwife endeavoured to overcome by pulling at the cord. Eventually a surgeon was called in, and he and his assistant attended the patient up to the time of her admission to St. Mary's Hospital, Manchester, on the 6th July, 1874, *i.e.*, for upwards of three months. She stated that she lost an alarming quantity of blood at the time of her confinement, and that she had had more or less hemorrhage ever since. Sometimes she had lost a very considerable amount of blood; the last occasion on which this occurred was about a fortnight before her admission. Various injections had been given to her for the purpose of controlling the loss; no vaginal examination, however, was made after the confinement.

On admission to the hospital, the patient presented a most blanched appearance. She complained of no pain; the appetite was pretty good; the pulse 100 feeble. A small pear-shaped tumour, bleeding readily when touched, was found in the vagina.

On the following day (8th July), the patient was placed under the influence of chloroform, for the purpose of further examination. The tumour was smaller than I should have expected an inverted uterus to be; it bled very readily; on passing a finger into the rectum and pressing the fingers of the other hand at the same time deeply above the pubes, no intervening substance could be felt. The absence of the uterus from its normal position was further demonstrated by passing a sound into the bladder, and again by the impossibility of introducing the uterine sound by the side of the tumour, the neck of which was tightly constricted. Being now satisfied that the case was one of inversion of the uterus, I tried digital pressure, and persisted in it for some length of time (I regret I did not notice the time accurately). Considerable force was used; as much, indeed, as I deemed justifiable. I therefore desisted, with the intention of employing the elastic stem-pessary described by Dr. Barnes in the first volume of the "Obstetrical Transactions," and in the event of that failing, of incising the neck, as recommended in the same communication. Some of those present, however, thought that further efforts should be made with the hand, and I reluctantly gave my consent. Pressure was accordingly resumed; and after some time the operator announced that he had succeeded in reducing the inversion. It was obvious to the onlookers that the success was due to the exertion of extreme force; and, on examination, two fingers could be passed into a rent in the anterior vaginal wall just where it formed the cervix uteri. There was also some laceration of the perineum, but at what stage of the proceedings this happened is uncertain. After the operation, the patient's pulse was pretty good, and her general condition was not alarming.

*July 9th.*—She complained of soreness and a little abdominal pain. Temperature normal; pulse 110.

*July 10th.*—She felt chilly; had no pain. Temperature 98°; pulse 104.

*July 11th.*—She felt better. Temperature normal; pulse 100.

*July 12th.*—There was a good deal of whitish discharge. Temperature normal; pulse 100.

*August 8th.*—Since the last note, the patient had not had a single bad symptom; the vaginal rent had healed, a very slight thickening marking its site; the uterine sound passed two inches in the normal direction. The woman then became an out-patient, and preparations of iron were prescribed for her on account of her extreme anemia. She subsequently went to Southport for six weeks; and on October 21st she again presented herself at the out-patient room, looking much improved in colour and general appearance. She had menstruated twice since leaving the hospital. The uterus was normal in position; the sound passed rather more than two inches; the os was somewhat flabby, and presented the appearance of a little superficial ulceration, which soon yielded to the application of nitrate of silver; the situation of the vaginal rent could not be discovered.

I saw my patient after some time, when there were signs of commencing phthisis ; after this I lost sight of her, and am now unable to obtain any trace of her.

Inversion of the uterus is not a very common occurrence, only fifteen cases having been reported in the English journals since the year 1871. It does not seem necessary to enter into the details of these cases ; suffice it to say, that nearly all of them are said to have been due to traction on the cord.

In the *British Medical Journal* for January 28th, 1871, Dr. Tylecote reports a case of spontaneous inversion twenty-four hours after labour. Dr. Braxton Hicks relates a similar case in the *Journal* for August 31st, 1872 ; and also another, where the inversion followed delivery by forceps ; and in the first volume of the *OBSTETRICAL JOURNAL* (page 319), Mr. J. Prankerd narrates a case where inversion occurred during the effort of straining at stool seven days after labour. With regard to the possibility of spontaneous inversion, and for an explanation of this phenomenon, I beg to refer my hearers to Dr. Hicks's paper in the *British Medical Journal* for 1872.

The treatment of these fifteen cases varied ; most of them were reduced by pressure of one kind or another ; two died before medical aid could be procured ; in two, the uterus was removed as a last resource in order to save life ; and in two others Dr. Barnes succeeded in reducing the inversion by the method to which I have already alluded, and the value of which cannot be over-estimated.

Dr. Barnes has clearly pointed out how an inverted uterus *ought* to be treated ; it has fallen to my lot to bring before the profession a case which teaches how it *ought not* to be treated ; for the satisfactory termination of the case does not prove that the treatment was one to be recommended.

How many women would have recovered after such severe injuries ? and what would have been the result if the rent had occurred posteriorly instead of anteriorly ? Now, how are such accidents to be avoided ? We are directed to use as much force as is justifiable : what is the measure of justifiable force ? What amount of strain will the vaginal walls bear ? Speaking from my experience in the case here recorded, I would urge that the utmost care, care even approaching timidity, be used in the application of manual pressure to chronic cases, and that, in the event of failure by this method, recourse be had to the air-pessary, or even incision of the neck, inasmuch as such an operation must be far less serious than rupture of the vaginal wall.

*Remarks by WILLIAM KELLY, M.D.*

I have recently had under my care a case very parallel to that related by Dr. Ewart, with one important exception—viz., that the inversion was *not* reduced. I append a brief account of the case.

The patient, aged twenty-four, had been delivered of her first child by a midwife in December last. She was admitted into the Taunton

and Somerset Hospital on March 22nd, suffering from extreme exhaustion and uterine hemorrhage, the latter having continued more or less from the time of her confinement. She had received medical treatment before coming to the hospital, but no vaginal examination had been made. The state of inversion being ascertained, she was put under chloroform, and for more than an hour efforts were made with considerable force to effect reduction; but they were ineffectual. Some amount of constitutional disturbance followed, and a second attempt was not made for a month (after the next catamenial period had passed over); this proved equally unavailing with the first. The patient having soon recovered from the effects of this second operation, and the uterine hemorrhage with its attendant exhaustion having also disappeared, she was anxious to leave the hospital, as the displaced uterus caused her little inconvenience. She was, however, prevailed upon to submit to a third attempt at reduction; but, this being followed by no better results than those which had gone before, her husband removed her from the hospital, and the case has now been lost sight of.

In employing taxis, I followed, or rather I endeavoured to follow, the directions given by Marion Sims in his "Clinical Notes on Uterine Surgery"—*i.e.*, after pushing the body of the uterus up within the cervix, he says the reduction should be completed "by compressing the fundus laterally, and deeply imbedding the thumb in the cornu uteri, by which means we slide one-half of the organ at a time through the os internum, instead of the whole fundus, which presents a greater diameter." The first stage was easily got over; but all the sustained pressure I could employ failed to open the internal *ring* (for that was the form the part assumed) for the passage of the fundus. I varied the position of the patient, and I also used alternately both my right and my left hand.

---

*Meeting, August 5th, 1875.*

*Memoir of the Life of William Smellie, M.D.*

By ALFRED H. MCCLINTOCK, M.D. Dublin.

Owing to the limitation of time, he had to confine his paper to the personal history of Smellie, about whom he had been able to collect some new and interesting particulars. He was a native of Lanarkshire, and was born some time in the year 1697. He studied midwifery in Paris under the celebrated Gregoire. About the year 1722 he settled down to general medical practice, in the neighbourhood of Lanark, where he remained till the year 1739, when he changed his residence to London. Soon after this, he had the celebrated William Hunter residing in the house with him. Smellie began to give lectures on midwifery, and to attend labour cases at their own homes, along with his class, about a year or two subsequently to his settling in



London. These lectures attracted large numbers of pupils ; for, in the course of ten years, he says he had nine hundred pupils, exclusive of females. The first volume of his "Midwifery" appeared in 1752, and was the result of thirty years' experience, and of six years' careful preparation. It was followed at different times by two other volumes, chiefly taken up with the narration of cases, there being about 540 of these recorded in the two volumes. This work at once attracted great attention, and was translated into French, German, and Dutch ; so that it soon came to be regarded as the highest obstetrical authority. It will ever hold the place of a most valuable standard work upon midwifery, and be carefully read by all earnest students of this branch of medicine. This treatise of Smellie's did more to improve the practice and raise the character of the British school of midwifery than any other work that ever appeared before or since, and has most justly immortalized the name of its author. Towards the end of the year 1759, Smellie resolved to leave London. Accordingly, having made over his class, museum, and teaching appliances, to Dr. Harvie, he left London, and returned to his native county of Lanark. This Dr. Harvie is supposed to be the same who married a niece of Smellie's, and to whom he bequeathed his property, &c., Smellie himself not having had any issue by his marriage with Eupham Borland. Having established himself near to the town of Lanark, in a place called after him, "Smyllum," or "Smylane," he spent his leisure hours in preparing the third volume of his "Midwifery." He only just lived to complete this great work. Mr. Onslow, writing of Smellie in 1821, said he did not know where Smellie died and was buried. However, Dr. Maxwell Adams, of Lanark, cleared up this matter by the discovery of Smellie's tomb in the old churchyard of Lanark, on which it is stated that "he died March 5th, 1763, aged 66." He bequeathed a sum of 200*l.*, all his books, and some other matters, to the Grammar School of the town of Lanark. There is a portrait in oils of Smellie in the Royal College of Surgeons, Edinburgh. This is supposed to be the one mentioned in his will as having been painted by himself. Where Smellie received his medical education or obtained his degree, are questions that yet remain to be answered. He was a man of close and accurate observation, of great diligence, very methodical, and withal of a very philosophic turn of mind, which made him candid in confessing his faults, and ready to admit the merits of others. He inaugurated a new era in English midwifery, and contributed very materially in overcoming the strong prejudices then existing against the use of the forceps, and against male obstetric practitioners. His writings contained many and original observations, especially upon the mechanism of parturition, the thickness of the gravid uterus, the process of labour ; the shape, application, and use of the forceps ; the management of the placenta, &c. The cases recorded in his second and third volumes, not only were a mine of clinical experience that would amply repay the reader, but they abounded in observations rich in practical wisdom. So re-

markable a man as Smellie could not possibly avoid having detractors and opponents, and foremost among these was Dr. John Burton, of York—the original of Sterne's Dr. Slop. But Smellie treated all their attacks with great indifference, and only once condescended to reply to them.

Dr. PRIESTLEY (London) had listened with the greatest possible pleasure to the memoir, and was glad to be able to announce to the Section that Dr. McClintock had undertaken the editorship of Smellie's works on behalf of the New Sydenham Society.

### *The Examination of the Female Bladder.*

By J. MATTHEWS DUNCAN, M.D. Edinburgh.

Dr. DUNCAN proposed that, after dilatation with the dilator of the shops—an instrument much in need of improvement—a speculum be introduced similar to the rectum-speculum.

Dr. PRIESTLEY (London) asked what were the results as to frequency of urination and power of retaining the urine.

Dr. LEGAT (South Shields) asked if Dr. Duncan had found female *urethræ* to vary considerably in size.

Dr. STEELE (Liverpool) asked if the mode recommended revealed a large surface of the mucous membrane.

Dr. Moore, Dr. Lombe Atthill, Dr. McClintock, Dr. Copeman, Dr. Protheroe Smith, Dr. Young, Dr. Wallace, and Dr. Chambers took part in the discussion.

Dr. MATTHEWS DUNCAN said that, so far from having met with any ill effects, the dilatation had, in his hands, given so much relief to the symptoms, that a repetition of the proceeding was desired on the part of the patients. He had found that female *urethræ* differed extremely in calibre. In reply to Dr. Steele, he said his impression was, that the whole surface of the bladder could be brought into view by the specula now shown and recommended.

### *Intemperance in Women, with Special Reference to its Effects on the Reproductive System.*

By JOHN HADDON, M.D., Manchester.

Dr. HADDON first considered the existence of intemperance amongst women. Secondly, he pointed out its causes, the principal of which he believed to be:—1. The common practice of using stimulants in some form as a beverage in the family, and as a mark of hospitality to strangers; 2. The frequent prescription of stimulants by the profession, and; 3. Domestic or other cares which make many drunkards. He then considered its effects on the woman's constitution, and pointed out some symptoms likely to be met with, among which he specially referred to its tendency to cause irregularity of the menses, menorrhagia, and abortion. He next endeavoured to show that no child-

bearing woman should use any alcoholic stimulant; and concluded with some remarks on the remedy for female intemperance.

Dr. E. LONG FOX (Clifton) strongly objected to the view that the habit of drinking was in any measure due to the dangerous prescribing of medical men.

Mrs. GARRETT-ANDERSON (London) thought the prevalence of intemperance in women had been greatly exaggerated; but she was ready to acknowledge that it was much too common, and she could not wholly free medical men from blame in the matter.

Mr. Morgan (Lichfield), Dr. Priestley, Mr. Meacham (Manchester), and Dr. Moir, also took part in the discussion.

### *Obstetrical Statistics.*

By J. G. SWAYNE, M.D., Clifton.

Correct obstetrical statistics, especially of cases attended in private practice, are of great importance just at the present time, when the principles and practice of midwifery are undergoing radical changes. The records of private practice will supply what we cannot obtain from lying-in hospitals—viz., correct statistics of midwifery practice amongst the richer classes of society. Dr. Swayne then referred to statistics derived from his own practice, in order to show that the process of labour, especially in the third stage, is attended with greater danger amongst these than amongst the poor, in consequence of the greater liability to hemorrhage and adhesion of the placenta. He also referred to his own statistics of the varieties of cranial presentation, and pointed out that they differed from those usually given, in assigning a greater frequency to the fourth than to the third position of the head. With respect to forceps operations, his own figures show that the modern practice of employing that instrument with much greater freedom, is attended with very good results, but that further researches are needed to determine the relative advantages of the long forceps and turning, in deformity of the pelvis. With regard to puerperal convulsions, they show that there is no good reason for abandoning the old practice of bleeding, whilst fully bearing out the utility of anesthetics. Lastly, good statistics of private practice are more than ever needed in order to throw light upon the etiology of puerperal fever, a disease which has been lately very prevalent, and of which the prevention is a far more promising subject for consideration than the cure.

Remarks were made on the paper by Dr. Bassett, Dr. Nesfield, Dr. Steele, Dr. W. Macdonald, Dr. Moir, and Dr. Keeling.

### *On Uterine Flexions and Displacements; and their Mechanical Treatment.*

By J. G. S. COGHILL, M.D., Edinburgh.

Dr. SINCLAIR COGHILL confined his remarks mainly to the mechanical treatment of uterine flexions and displacements, only re-

ferred incidentally to their pathology. He alluded to the association of the Edinburgh Medical School with this department of medicine ; and, in this connexion, paid a warm tribute to the practice and inventive skill of the late occupant of the midwifery chair, Sir James Y. Simpson. He referred to the reaction which set in against his teaching and example, explained its causes, and suggested that probably the rule *in medio tutissimus ibis* in this as in other debated questions would be the safest guide. The author next pointed out the leading principles on which this treatment should be based, in the various forms of uterine distortion and dislocation. He concluded by deprecating strongly the *nimia diligentia* in this as in other departments of practice. Several interesting cases were described in illustration of the views submitted ; and a variety of instruments, several of original design, were exhibited, and their use demonstrated.

*Nitric Acid as a Caustic in Uterine Practice, and its Superiority as such to Nitrate of Silver.*

By JAMES BRAITHWAITE, M.D., Leeds.

Nitric acid is the caustic which, of all others, is the best adapted for use in cases of chronic inflammatory disease of the os and cervix uteri, resulting in erosion or ulceration. Nitrate of silver is inefficient, and requires frequent reapplication, to atone for its defects both in degree and in the nature of its action. Nitric acid, on the other hand, acts as a caustic in these cases with certainty, and neither does too much nor too little. Its application is productive of little or no pain ; and, when it has once been properly applied in some cases, no further speculum-examination is required, such reliance may be placed upon its effects. If an examination be made, which is always better, it need only be after an interval of a month, and then the acid may be applied again to any spot which appears to require it. The resulting sore has a very strong tendency to heal, and does so partly by contraction and partly by fresh formation of mucous membrane, which is not cicatricial in appearance. The contraction is greater than follows the application of any other caustic, and is the very thing required to insure the permanence of the cure. The contraction in cases of cervical catarrh is only contraction to a healthy size of the canal, provided the acid is used with proper care. The peculiarly lasting and permanent action of nitric acid enables us to do away with the repeated speculum-examinations, so distasteful to both patient and surgeon ; and gives the latter a feeling of confidence of success which he cannot have with any other caustic. The use of nitric acid, common as it is in other diseases, is referred to by very few writers, and is entirely omitted by most of our standard authors upon diseases of women, all of whom recommend nitrate of silver, or mention its use as the usual practice.



*On the Management of the Lying-in Woman.*

By THOMAS WHITESIDE HIME, B.A., M.B., Sheffield.

Dr. HIME began his paper by stating that the old and still established theory which represents the lying-in woman as being in a state similar to that of a person after a serious amputation, the uterus being compared to the part operated on, is unscientific and untenable. Parturition is a physiological process, the fulfilment of a natural function, and has no analogy with an operation, which is an interference with function. Amputation, whether the result of disease or accident, involves consequences which have no analogue in the process of parturition. The uterus after labour is no more comparable to a stump after amputation than the uterus after or during menstruation. After natural labour (to which Dr. Hime's paper chiefly referred) there is nothing comparable to the collapse succeeding a major amputation; there is no fever, no suppression of secretions, no suppuration, or, if pus be present, it is not derived from the uterus at all, but from the vagina or external genitals, in the great majority of cases. The insignificant rise in temperature, from  $0.5^{\circ}$  C. in *multi-paræ* to  $0.8^{\circ}$  C. in *primiparæ*, is due to normal physiological and not to morbid action, being the effect of muscular exertion, increased activity of the lungs, liver, and other organs, when relieved from the pressure of the gravid uterus, and is only fleeting. Milk fever is far more talked of and written about than seen, and is of rare occurrence. The rise in temperature which accompanies the commencement of mammary activity is slight, temporary, and unaccompanied by mental depression or constitutional disturbance of any kind. Operations performed immediately after labour will yield kindly, of which Dr. Hime related several instances in his own practice. Regarding parturition as a normal physiological process, Dr. Hime urged the importance of a decided alteration in the common mode of treating lying-in women as *patients*, and confining them to bed for ten or twelve days on low diet; the ordinary puerperal dietary being such as would certainly not be given to any patient after amputation. He urged that water-gruel, barley-water, tea, and dry toast, should be abandoned for milk, eggs, good soup, chickens, and other digestible meat, to be given from the first, and of course in quantities suitable to the conditions of individuality, want of exercise, &c. Stimulants are decidedly injurious, except in special cases. It is often urged that, as a large amount of waste uterine tissue, &c., has to be got rid of, low diet should be adhered to; but milk has also to be secreted, and any how health and vigour will promote excretion, and the performance of all vital functions better than a state of debility. Opium, ergot, and other drugs should only be given under necessity. The child should be applied as soon as the mother's state permits; if there be no milk at first, only for a minute or so to encourage its secretion, and the involution of the uterus. The binder is more of an *euthanasia* than a benefit after the first twelve hours, but not so

the early removal into a fresh bed, and room, if possible, and this may be done within forty-eight hours. The woman may sit up in bed for a short time from the first, a continual maintenance of the recumbent posture for ten or twelve days being as injurious as it is unnecessary, and most patients may be on the sofa on the fourth or fifth days. Above all things, the medical attendant should see that his directions are carried out, and not trust they will be so, especially as to the speedy removal of soiled linen, &c.; not that its presence, any more than the neighbourhood of privies, want of ventilation, &c., will *per se* develop metria any more than typhoid; otherwise eight or nine-tenths of lying-in women must inevitably suffer from it, a result equally certain if medical men could convey the germs of disease with them as readily as is assumed. Cleanliness and ventilation always tend to preserve health and check disease, but they are no more needful for the lying-in woman than good nourishing food. After natural labour a woman is not in a diseased state, and the maintenance of health and vigour will be the most successful means of averting all risks.

*Notes of a Case of Transfusion by Aveling's Apparatus.*

By GEORGE HOGGAN, M.D. Lond.

The case was one of secondary hemorrhage after removal of a diseased cervix uteri. The patient sank.

*Friday, August 6th.*

*Notes on a Case of Triplets complicated by Double Uterus.*

By A. G. DUNCAN, M.B., Crimond.

This was a very rare and interesting case, in which there was a depression in the abdomen indicating a division between two tumours formed of the two halves of the uterus, one containing two children, and the other one. The children, all females, were born alive; their united weight was twenty pounds. On subsequent examination, the uterus was felt to be divided by a septum.

*On Dysmenorrhea.*

By E. GARRETT-ANDERSON, M.D.

In this paper, Mrs. Garrett-Anderson discussed the following questions:—1. How far is the mechanical theory of dysmenorrhea supported by facts? 2. What is the relation between mechanical or obstructive dysmenorrhea and the so-called neuralgic, congestive, and rheumatic forms of the complaint? 3. To what extent ought the mechanical theory, if we accept it, to guide our treatment? With regard to the first question, Mrs. Garrett-Anderson agreed with Dr. Marion Sims and Dr. Barnes, that the essential cause of dysmenorrhea

was retention of the uterine secretion. This view was supported by the curative influence of parturition. The author differed, however, from Dr. Sims when he denied the existence of constitutional dysmenorrhea; for in a large number of cases the retention might depend on a constitutional condition. The anemic, congestive, and rheumatic forms of dysmenorrhea were commented on; also that dependent on uterine flexion. Mrs. Garrett-Anderson did not believe in neuralgic dysmenorrhea, as the term was commonly understood. The form thus described might depend on obstruction, or on abrasion of the os, with endometritis of the cervix or fundus. Cases of ovarian origin were believed not to be common in early life, nor to be often primary. "Intermenstrual" dysmenorrhea was not dysmenorrhea at all, and was probably due to ovarian congestion. In regard to the treatment, Mrs. Garrett-Anderson pointed out that there were facts which seem to indicate that, in accepting the mechanical theory of dysmenorrhea, it is not necessary to adopt in the first instance and in most cases a mechanical line of treatment. Various constitutional conditions frequently gave rise to obstructive dysmenorrhea, which could often be removed by constitutional measures.

Dr. ATTHILL (Dublin) thought that, in girls menstruating recently, dysmenorrhea might be due to anemia; but in others to some form of inflammation. He did not agree with the view of childbearing curing obstruction. He did not think flexion *per se* the cause of dysmenorrhea. A perfectly healthy uterus could not flex itself. After marriage, dysmenorrhea occurred from endometritis. In the cases where dysmenorrhea began after marriage, there was nearly always a painful point at the os internum; and the pain could be produced by touching this point. He thought mechanical treatment was carried to too great a length. He did not agree with Mrs. Garrett-Anderson that no mechanical treatment should be adopted before the age of thirty, as it sometimes was useful at a much earlier age.

Mr. SPENCER WELLS (London) thought the few words in which Mrs. Garrett-Anderson alluded to ovarian dysmenorrhea scarcely did the subject justice. It might be observed that, two days before menstruation, pain was often present, alternating at different periods in one side and the other. No doubt it was due to constitutional causes, but also to inflammations; and the pain arose before the excretion found its way into the uterus and vagina. He thought that the introduction of a sponge-tent before the menstrual period often was of great use.

Dr. A. SIMPSON (Edinburgh) remarked that it was overlooked in the paper that, after dysmenorrhea was established, it was rare to find that there was not some organic affection of the uterus; but it was not easy always to say the exact cause of the pain, as that might be various. He did not think there was any medical practitioner in Great Britain who would not at once, in a young girl even, make a vaginal examination, and would not at once have recourse to surgical

treatment. He alluded to cases in the unmarried, and also to the state of the uterus during the menstrual period. In his experience, the uterus became erect during menstruation ; and this was a fortunate thing ; for antelexions were so common that, without this erection, dysmenorrhea would be much more common. He alluded to the effects of marriage in relieving pain, but without pregnancy ; and this led to the mechanical treatment being introduced. The treatment was often used by men who did not know the value of it.

Dr. STEELE (Liverpool) thought the many differences of opinion could be removed if dysmenorrhea were regarded as a symptom, and not as a disease. He agreed with Mr. Spencer Wells, that many cases were of ovarian origin. Sufficient attention was not given by the author of the paper to the state of the excretions of the bowels. The treatment must be purely eclectic.

Mr. HARRISON (Reading) asked Mr. Spencer Wells whether the use of tents was free from danger.

Mr. SPENCER WELLS thought the danger arose from the way in which the tent was used. If it were carefully introduced, and not left too long, he did not see any danger.

Dr. KEILLER (Edinburgh) expressed his hearty approval and admiration of the paper. It was just what he would have expected from his old pupil. He thought that much danger was done by indiscriminate surgical and mechanical treatment. He did not now use the dilating instruments so frequently as he did when a younger man.

Dr. MCCLINTOCK (Dublin) expressed his approval of the paper, and seconded the observations of Dr. Keiller. He was satisfied that the large majority of cases which came under his own observation could be treated by medical means.

Mr. FREER added his opinion to that of Dr. Steele, that the overloading of the intestines must not be overlooked. It was due to two causes : sedentary occupation, and the ligature worn for the sake of symmetry, which tended to keep up a state which led to irritation of the uterus.

Dr. LEGAT (Shields) observed that there was one point not alluded to. The pains of dysmenorrhea resembled after-pains. These varied in different persons ; and there was no obstruction and no flexion in these cases.

Dr. G. BUCHANAN (Glasgow), being a pure surgeon, could not express an opinion on the subject ; but he moved a vote of thanks to Mrs. Garrett-Anderson for her excellent paper.

This was seconded by Dr. PRIESTLEY, and carried.

Mrs. GARRETT-ANDERSON returned thanks for the compliment paid her. She did not intend to convey the idea that endometritis did not occur before marriage. She did not mean that surgical treatment should not be adopted before the age of thirty ; but in London she thought it was too frequently adopted in young girls, such as frequently, coming from the country, became anemic and



subjects of dysmenorrhea. Nor did she mean to imply that there was not ovarian pain ; but she thought it was secondary. She was not competent to give an opinion on Dr. Simpson's question, but was under an impression the reverse of that stated by him. She included Dr. Steele's treatment under the ordinary routine treatment. In reply to Dr. McClintock, she said she believed that obstruction might exist. Although a sound could be passed, the passage would be very different when in a state of congestion. She hesitated about doing good with pessaries without postural treatment at the same time. She used Chambers's and often Wood's pessaries. The danger of pessaries ought never to be overlooked. As to the question of Dr. Legat, no doubt the pains were much the same, both being due to irregular contraction of the uterus ; but she was not prepared in the present discussion to go into the subject of after-pains. She concluded by thanking the meeting for her reception.

### *Syphilitic Placenta.*

By ANGUS MACDONALD, M.D., Edinburgh.

Syphilitic disease of the placenta had of late attracted some attention on the Continent, but had commanded little attention from the British profession. The paper embodied the result of a careful microscopical examination of two undoubted specimens of the disease. The chief difficulties that lay in the way of ascertaining the true nature of the disease were twofold. 1. It was liable to be mistaken for fatty degeneration of the placenta, and had been so mistaken by excellent observers, such as Kilian and Robin. 2. It was frequently almost impossible to arrive at a satisfactory proof of constitutional syphilis till the discovery by Wagner, of Berlin, of osteochondritis syphilitica. Microscopical and chemical examination of the placenta were sufficient to show that such cases were not fatty degeneration. It was easy also to prove in a fetus born dead, and even macerated, whether it was constitutionally syphilitic or not. If the fetus were syphilitic, there would be a band of tissue between the bone of the shaft and the cartilage of the epiphysis of the long bone, in a condition of inflammatory irritation. This band was bounded by very irregular outlines both towards the cartilage and towards the true bone, and consisted, according to the advancement of the disease, either—1. Of cartilaginous cells, hypertrophied and greatly increased by proliferation, as also prematurely infiltrated with earthy matter ; or 2. Of the above, combined with premature sclerosis of the intercellular tissue, and premature osteogenic formation within the cartilage, and arrest of true bony transformation ; or 3. The higher degrees of inflammation might come on, softening and interruption of the connexion between bone and cartilage, and inflammatory exudation with even suppuration. The results of those changes might be seen both by the naked eye and the microscope, as the reddened or greyish yellow band was quite visible to the naked eye,

and the hardened prolongations of premature calcified cartilage were easily seen and felt. The change in the placenta was equally distinct, but varied—1. According as the father was primarily affected by syphilis; 2. According as the mother was first affected; 3. According as both were syphilitic early in the pregnancy. If the father were primarily affected, the villi were the site of the disease in the first instance. They were the seat of a peculiar cellular hypertrophy and multiplication, named by Fränkel “disfiguring granulation-cell disease,” and which consisted of an immense multiplication of the cellular contents of the villi and of the epithelial mantle of the villus, together with an increase of the thickness of the wall of the included vessel. This cellular multiplication and increase proceeded outwards from this vessel as a centre, and the rows of connective tissue nuclei were seen to be arranged in circles, reminding one of the appearance of an Haversian canal. In consequence of this cellular multiplication, the villi were enormously increased in size and weight, the vessels were ultimately completely obstructed, and by-and-by the hypertrophy was followed by atrophy and abortion of the villus. The unaffected portions of the placenta were liable to become congested; extravasations were likely to be hence formed, and the ultimate result was suffocation of the fetus. If the mother were primarily affected, the disease attacked the maternal placenta, and consisted essentially of increased growth of the connective tissue framework of the placental decidua, and enormous hypertrophy of the large cells of the decidua, leading to obstruction of the villi by compression. The affection described by Virchow, Slavjansky, and Kleinwächter as endometritis, placentaris nummosa, was probably syphilitic disease of the maternal placenta. If both father and mother were primarily syphilitic, or became so in the early months of pregnancy, both conditions exist conjointly. In two placentas examined by Dr. Macdonald, the history proved syphilis of father and of mother; the bones showed well developed osteochondritis syphilitica, and the placental tissue was affected by both forms of the syphilitic degeneration of tissue. From his researches, Dr. Macdonald drew the following conclusions.—1. A large number of intra-uterine deaths from diseased placenta are due to the existence of constitutional syphilis in either or both parents, and the death is the result of progressively increasing defective blood supply, owing to the changes described above. 2. The changes taking place in the placenta give the organ a pale appearance, and increase its size; and, in consequence, the appearance it presents is liable to be mistaken for fatty degeneration. This mistake is easily prevented by microscopical and chemical examination of the diseased organ. 3. The bones of the dead fetus will (as they will show osteochondritis syphilitica, if the constitutional disease be present) afford a valuable and infallible means of deciding whether the corresponding placenta is syphilitic or not. 4. Medicines thought to be beneficial by their action upon the blood, as oxygen given in such cases, most probably are useful, if

they really do good, as blood-depurators, and may be beneficially replaced by iodide of potassium and other accredited anti-syphilitic measures. 5. On the whole, there is little good to be expected from premature labour in such cases, as the child, though born alive, is saturated by the disease. We may expect better results by anti-syphilitic measures acting through the mother on the placenta and the child at the same time.

Mr. LAWSON TAIT (Birmingham) thought the view of the paper an error, and referred to his own recent paper on the subject. The change occurred in the arteries, and was analogous to the changes in vessels of the kidney described by Dr. Johnson. It was a conservative change.

Dr. SWAYNE (Clifton) made observations on the size of the placenta.

Dr. MACDONALD replied.

*Note of Inquiry with reference to some Points in the Management of the Third Stage of Labour.*

By J. WALLACE, M.D., Liverpool.

In this inquiry Dr. Wallace considered the rules of guidance and practice generally followed ; first, as regards the treatment of the placenta ; and secondly, that of the uterus subsequently. The position of the placenta was shown to be most frequently on the middle zone of the uterus ; and next, on the fundal zone ; and according to its position was traced the mechanism of expulsion. Here it was pointed out that Smellie and others knew the true mechanism of expulsion and delivery of the placenta, which has been recently brought under the notice of the profession by Matthews Duncan, Lemser, Caseaux, Leishman, and others, in contradistinction to the descriptions given by Baudelocque, Schultze, &c. The different methods recommended by various obstetric authorities of management of the placenta were referred to, and the author's views and practice stated. The paper was concluded by a reference to the uses and abuses of the binder, a routine habit of practice being condemned both in that and in the habit of giving ergot, opium, cordials, &c., after delivery. It was especially shown that the binder, as applied with pads, converted an abdominal organ, as the uterus is at that stage, into a pelvic one, and hence caused, instead of prevented, flooding, and was one of the main factors in producing subsequent uterine trouble in the way of flexions and displacements.

*Vomiting in Connexion with Pregnancy.*

By H. FLY SMITH, M.B. Oxon.

Pregnancy causes a generally exalted condition of the nervous system. Reflex action is readily exhibited. Nausea and vomiting

in the early months of pregnancy are coincident with the development of the corpus luteum in the ovary. The gastric symptoms appear usually in the morning, because the blood has become tainted with an extra amount of carbonic acid gas breathed during the night. Exposure of the skin to fresh air acts reflexly on the morbidly excitable centre commanding the pylorus; closure of this valve causes the stomach to empty itself by way of the esophagus instead of the duodenum. The vomiting may proceed to an obstinate and uncontrollable form, endangering life by inanition. This, too, occurs apart from any organic lesion, whether of the digestive track or other organ; the uterus and ovaries may also be apparently in a normal condition. Treatment directed immediately to the stomach has frequently failed. Observers having found that application of leeches to the cervix has been followed by remission of the gastric symptoms, have therefore thought that inflammation of the cervix originated the reflex action of the stomach. In some cases redressment of a retroverted womb has succeeded. Dr. Graily Hewitt asserts that there is always in these cases flexion of the cervix. Dubois observed that having dilated the cervix for the induction of labour, the vomiting forthwith ceased without interruption to the pregnancy. Dr. Copeman of Norwich has recently published three cases, when similar treatment was followed by a like success. All authors agree that, in extreme cases, drugs fail. It will appear then, that the uterus itself should be attacked with the view of subduing inflammation if it exist, or redressing the womb if it be displaced, or dilating the cervix as an empirically successful treatment.

*Neuralgic Dysmenorrhea.*

By CHARLES R. DRYSDALE, M.D.

The author thought that a salutary revolution was now setting in against the surgical doctrines held by Dr. Marion Sims and others upon dysmenorrhea and its causes. Dr. Drysdale very rarely indeed witnessed any case where he had found any service to arise from operations on the uterus; whilst he had seen some cases of pelvic abscess and pelvic peritonitis occur from such interference. He was lately consulted by a patient, single, aged thirty-two, who had suffered since the age of sixteen from dysmenorrhea, and who, on consulting two eminent specialists, was advised by the one to have recourse to incision of the cervix, and by the other to wear a pessary. In this case, the uterine sound passed in its normal direction without difficulty, and the patient had no leucorrhea nor prolapse of the organ, which was quite normal in size. There was no ulceration of the os uteri, although another eminent specialist had considered this as the cause of her dysmenorrhea. The author held that there was still too great a tendency to expect to find an evident physical cause for all painful menstruation. Spasm and neuralgia were quite sufficient to account for the vast majority of cases. Membranous shreds, also, were



frequent causes of obstruction to the monthly flow. An illustrative case was recorded. The rational treatment of dysmenorrhea commencing at an early period, consisted not in the use of pessaries, or of incision of the uterus, surely ; but in the use of cold baths in the morning, with short walks in the open air afterwards ; in hot baths, a few days previously to the menstrual periods ; and in palliative treatment of the paroxysms by means of antispasmodics at the epoch of pain. Marriage sometimes cured such cases at once ; at other times, it was of no use.

*Case of Rupture of the Umbilical Cord during Delivery, followed by Death of the Child.*

By W. HAINING, M.D., Chester.

The chief points of interest noted in this case were the following. The duration of the pregnancy, as fixed by well-proved circumstances, could not have exceeded 274 days, whilst, according to the mother's showing, it must have been not more than 267 days. 2. Menstruation was prolonged for four, if not five periods after the occurrence of impregnation ; and the continuance of the function, the cessation of which was regarded by the mother, aged eighteen years, as the only certain evidence of pregnancy, caused her to miscalculate her "time." 3. The labour appears to have been completed within two hours of the time at which the pains first called for attention. These were not of the kind required, and their true nature was not apprehended. This time is unusually short for a primipara under ordinary circumstances. 4. The cord was of the usual thickness, but measured only eight inches in length ; the length at the placental end being about five inches and a half. There was no hemorrhage from either of the ruptured ends. 5. The death of the child was interesting medico-legally. It died twenty-three hours after its birth ; there was no suspicion of foul play, and there had been no concealment of pregnancy. The only post-mortem fact noted, was a bruise of the scalp over the left occipito-parietal suture, but there was no internal effusion of any kind. The birth took place in a house, in the presence of married women ; but had it occurred in a privy or water-closet, in the case of a woman who had never confessed her shame, she would, however innocent her intention might have been, have certainly been required to stand her trial upon a criminal charge.

*Remarks on Hooping-cough and its Treatment with Carbolic Acid Vapour, with Description of a Steam-draft Inhaler.*

By ROBERT J. LEE, M.D., London.

Dr. LEE directed attention to the very slight variation in the annual mortality from hooping-cough, to its widespread geographical distribution, and to the results of various kinds of treatment. The

conclusions which he deduced were the results of observation of six hundred cases, and the most important remarks were connected with the rise and fall of the disease at the first or second quarter of the year, and the frequency with which the disease was not diagnosed on account of the absence of the laryngeal spasm or whoop. The use of carbolic acid was recommended as proving more satisfactory than any other kind of remedy; and the method of administering it in the form of vapour by means of the steam-draft inhaler was explained. A solution of one part of carbolic acid in ten of water was kept as a standard for mixture in the proportion of two drachms to four ounces of water. This was introduced into the inhaler, and every four hours its vapour was inspired for ten minutes or a quarter of an hour. Dr. Lee explained the construction of the steam-draft inhaler, and the advantages of its use in other maladies of the respiratory organs, as well as in hooping-cough.

*The Pneumonias of Childhood, as illustrated by Clinical Charts.*

By W. STEPHENSON, M.D., Edinburgh.

The author remarked that the views generally entertained on the subject were in many respects erroneous, and that the manner in which it was handled in our books on diseases of children, failed in presenting it accurately, and led to confounding the different views one with another. After criticising the ordinary terms applied to the inflammations of the lungs in children, as showing the inaccurate ideas existing regarding the subject, he demonstrated the character of the disease in its two chief forms by means of charts of the temperature, night and morning, taken in the Children's Hospital. He showed that by the chart the character of the disease might be recognised at times when otherwise it might be overlooked, or before evidence of its existence could be found in the lungs. The recognition of distinctive types in thermometric charts gave a truer perception of the constitutional condition than could be gained by other means, and furnished more reliable indications for treatment than physical signs.

OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, February 10th, 1875.*

Dr. J. MATTHEWS DUNCAN, *President, in the Chair.*

*Contributions to the Determination of the Diminution of the Uterus after Delivery.*

By AR. SERDUKOFF, M.D., formerly External Assistant to the Obstetrical Clinic at Moscow.

(Communicated to the Obstetrical Society of Edinburgh by Dr. MATTHEWS DUNCAN.)

Parturition, as is well known, consists of a series of uterine shrinkings or throes, which gradually expel the child. Each pain is accom-

panied by a diminution of the blood-supply to the muscular substance of the uterus. Whilst the uterine muscle works, its nutrition is greatly disturbed by the contractions compressing the blood-vessels, and thus interfering with the vascular supply ; the muscular substance itself is destroyed—the products of that destruction being absorbed—and the uterus is diminished in size. The shrinking of the muscular substance of the uterus is observed, not only during labour, but also after delivery. This diminution is called involution of the uterus. The object of my paper is to examine in what manner the uterus is diminished under the different circumstances which occur in the puerperal state. The muscular substance of the womb of a newly delivered woman is absorbed ; from its external layers new muscular tissue is developed ; and, finally, a new uterus is formed. Instead of the old uterus, which weighed, immediately after labour, from two to three pounds, we see at the end of six or eight weeks after delivery, an organ whose weight varies from one to one and a half ounce—thirty to forty-five grammes. A reference to the Tables given at the end of this paper will show that, during involution, the uterus diminishes in length and breadth in an equal ratio. The body of the uterus diminishes in size more rapidly than the cervix. Twelve days after delivery, the uterine body is  $3\frac{2}{3}$ ths (1:0288) larger than the cervix,\* but at the end of the puerperal period the proportion is as two to one.† The time required for the completion of involution is variously stated by authors. Fabricius ab Aquapendente‡ holds fifteen days to be sufficient ; Deventer,§ judging from one case, puts it down at eight days ; Smellie|| gives from eighteen to twenty days. More recent authors, although not agreeing as to the exact time required for the completion of this process, greatly extend its limits. Velpeau¶ gives it as varying from five to six or eight weeks ; Krause, five weeks ; Hohl and Spiegelberg, eight weeks ; Schroeder, six to eight weeks ; whilst Scanzoni extends it to as much as sixteen weeks. From these statements, then, we perceive how widely opinions differ on this subject—even in recent times, authors varying from six to sixteen weeks. From my own investigations, which are not numerous, I am inclined to believe that the period varies from eight to ten weeks.

Ancient authors were undoubtedly aware of the fact that the uterus diminished in size after delivery (*vide* Fabricius), but Deventer\*\* was the first who drew attention to the gradual manner in which this occurred, and to the variations in different cases. Smellie says that the womb of a primipara expels clots with greater force than that of a multipara.

---

\* Lott, "Anat. und Physiolog. des Cerv. Uter.," 1872, p. 109.

† Sappey, "Traité d'Anat.," 1864, p. 664.

‡ "De Formato Fœtu," 1600, p. 142.

§ "Obst. Import. sur le Man. des Accouch.," traduit par T. T. Brumer d'Ablancourt, 1433, p. 42.

|| "A Treatise on the Theory and Practice of Midwifery," 1779, pp. 355-9.

¶ "Traité Complet de l'Art des Accouch.," 1835, t. ii. p. 613.

\*\* "La matrice continuellement tend à se resserrer," loc. cit., pp. 41-2.

Until the present century very few authors, and those very cursorily, have alluded to the subject of this paper. A British physician, Burns,\* says that the diminution of the uterus, after delivery, is a result of the absorption of muscular tissue. Wieland† was the first who tried to determine the extent of the diminution by measuring the length and breadth of the uterus after delivery; the one dimension being taken from the upper part of the symphysis to the fundus uteri, while the second is obtained by measuring between the most prominent lateral parts of the uterus. He supposed that up to the seventh day there was a daily diminution in size of from one to one and a half centimetre, and after that of from a half to one centimetre. At the tenth day he describes the uterus as being situated behind the symphysis pubis, and therefore beyond the reach of measurement through the abdominal wall. But Wieland's results are not faultless, he having first neglected antelexion: and, second, having failed to notice the regularity of the involution. On turning to German literature, we find that Schneider‡ has worked at this subject. This author has measured the uteri of 111 women after delivery. He divided his cases into groups, placing those who suckled their children into one, *multiparæ* into a second, those delivered before the full time into a third, and so forth. But on referring to these divisions we find sources of error which they do not exclude; for instance, some of those delivered prematurely did not suckle their children, and the involution of their uteri was prolonged. Now we do not know whether this was the result of not nursing or of premature delivery, or of both causes.

Wieland and Schneider both neglect a circumstance of immense importance—namely, the duration of labour. We are *à priori* led to pay particular attention to this, because prolongation of labour has some value with reference to the size of the uterus immediately after delivery. Then Hecker, Buhl,§ and Scanzoni|| propose to determine the involution by measuring the organ in those women who died at different times after delivery. From the table of these authors I have taken those cases where death did not apparently arise from disease of the sexual organs, and where, consequently, the uterus has undergone involution almost normally. I have thus got twenty-three cases, including two of my own. From these it is difficult, even if the labours appear to have been at the normal term, to make out a difference from day to day in the diminution of the uterus. For instance, one uterus on the third day after delivery had a smaller size than another on the ninth day. In order to determine the rate of involu-

---

\* "Principles of Midwifery," etc., 6th edit., 1824.

† "Etude sur l'Evolution de l'Uterus pendant la Grossesse et sur le Retour de cet Organ à l'Etat Normal après l'Accouchement," 1858.

‡ "Monatsschr. für Geburtsk.," Bd. xxxi. Heft v. p. 357.

§ "Klinik der Geburtsk.," p. 86.

|| "Lehr. der Geburtsk.," 4 Aufl. Bd. i. p. 356.



tion in this way, it will be necessary to gather many hundreds of cases, so that individual peculiarities may be effaced.

In our Russian literature there is reference to this subject in the work of Sutugin,\* who had copious material for his observations. But my countryman, aiming at a very general result, does not classify the puerperal women in the needful categories; and, therefore, we cannot see how the influence of various conditions of the puerperal state acts on the involution.

Opinions vary much with regard to the regularity of the process of involution. Simpson† and Pfannkuch‡ assert that it is a regular one. French authors deny this, but they do not attempt to explain the cause of the deviation from regularity. It is, of course, a difficult matter to demonstrate a law bearing on this point, because the physical and chemical changes which take place in the uterine muscle are variously affected by external influences acting on different women in diverse ways during the lying-in period. Thus, we may observe in healthy women with healthy uteri, a considerable difference in the progress of uterine involution. An apparently healthy womb may go through the process of involution less naturally than a diseased one, provided the former is influenced by conditions unfavourable to the proper performance of this function, whilst the latter is placed under circumstances highly propitious. Attempts have been made to determine these circumstances. Schneider, however, is the only author who has endeavoured to do so, and he has been unsuccessful.

My investigations into the involution of the puerperal uterus, although strictly of a scientific nature, are of considerable practical interest. This process has not been, as yet, well studied. There is no monograph of value on the subject, and the text-books afford little information. There are chapters on involution of the uterus, but as to the amount of diminution in length and breadth, at different times of the puerperal period, they are silent. Schroeder,§ like Schneider, says that involution does not proceed so regularly in *primiparæ* as in *multiparæ*. But what is meant by the term regularly? A uterus undergoing involution possesses two independent properties which influence it; the first is shown by a quickness of diminution, the second by a regularity or equality of that diminution. For instance, two uteri may diminish in size, on an average, 1-10th daily. One of them may diminish daily exactly this 1-10th, whilst the other diminishes to the extent of 1-16th of its size on the first day, and 1-7th on the second, &c. Here we have the same *rate*, but not the same regularity of diminution. Now we do not know what Schroeder understands by the term regularly. Does it express a

\* "A Medical Account of the Obstetrical Establishment at St. Petersburg," 1872, p. 150. (Russian.)

† "Clinical Lectures on the Diseases of Women," 1872, p. 587.

‡ "Archiv für Gynäcolog.," Bd. iii p. 372. § Loc. cit., p. 871.

medium normal quickness of the process, or an actual equality or regularity of it? Moreover, the relation between involution and after-pains is not explained in the text-books. Besides the interest attached to the study of this subject for its own sake, other circumstances, which have important theoretical and practical bearings, induced me to undertake these investigations.

The pathology of puerperal fever has long been a subject of discussion amongst authors. The variety and complexity of the circumstances under which this dangerous disease may arise, have greatly increased the difficulty of its elucidation. Such circumstances may be external to the patient, or they may affect directly the sexual organs. From this, it is evident that it is most important to make out, as far as possible, the phenomena of natural as well as of morbid puerperality. Having fixed data for comparing a morbid organism with a healthy one, we can note the primitive morbid processes in the sexual organs; and afterwards we may, by careful study of the external pernicious agencies, be able to arrive at a solution of an interesting question, whose practical application may benefit one-half of mankind.

There are several remedies in the Pharmacopeia which have the reputation of being able to influence the contractility of the uterus; these are, ergot of rye, borax, Indian hemp, savin, cinnamon, digitalis. With the exception of the first mentioned drug, however, their efficacy is very questionable. The incompleteness of our knowledge with regard to the value of so-called oxytocics, results from the difficulty of determining their influence on the patient. I think much might be done in this direction by experimenting with these drugs during the puerperal period, and noting their effect on the involution of the uterus. We take for granted that the innervation of the uterus is little, if at all, affected by varying conditions of health of the organ. It may show itself by activity of the excitant nerves which strengthen uterine action, or of inhibitory nerves. Those remedies which excite the sympathetic or enfeeble the sacrales, may cause contraction of the uterus whether a woman be pregnant, recently delivered, or a virgin; whilst those remedies which act in an opposite manner will diminish uterine contractility.

By employing any of these remedies during puerperality, and watching the involution of the uterus, it will be possible, if we use scientific care, to determine with precision its efficacy in regard to the uterine contractility. In this way the merits of some abortives, such as savin and aloes, may be disproved, while that property may be found in others hitherto unknown.

Besides, this determination may help to explain those feverish conditions which may depend on rapidity of involution,—a process involving great destruction of muscular tissue, absorption of the products of its metamorphosis into the blood, and oxidation of the products.\*

---

\* Schroeder, "*Schwang. Geburt und Wochenbett*," 1867, pp. 174-5.

The same may be said of the insufficient involution of the uterus, described by Simpson as sub-involution.

All the measurements of the 150 puerperal women I have made with my own hands, and can vouch for their accuracy. In taking them, attention has been paid to the following points:—1. Before measuring, the woman must empty her bladder. This she must be made to do even although she asserts that she has recently urinated. If she has any difficulty, the catheter is to be employed. 2. The position must be a thoroughly horizontal one. If the anterior abdominal wall is tense, the uterus has a lower position. If abdominal breathing is well marked, the measurement must be taken between inspiration and expiration, strong inspiration pushing the uterus downwards. No cases, where the uterus may have contracted adhesions to structures, which may interfere with its involution, are admitted into my tables. 3. During the first day after delivery, two measurements were taken; the first, from a half to three hours—rarely six—after delivery; the second, some time during the remaining hours of that day. On the following days one measurement only was taken, and that between ten and twelve in the morning. 4. The uteri were measured through the anterior abdominal wall by means of a tape divided into centimetres. The length is first determined. It being desired to have the uterus not under the influence of an after-pain, and yet to have the distinct solidity produced by contraction, I measured immediately after producing a contraction induced by rubbing the fundus uteri. Having placed the smaller three fingers of one hand as a tangential plane to the most prominent part of the fundus uteri, with the other two fingers I hold the tape. The middle finger of the other hand is firmly pressed on the superior border of the symphysis, whilst the thumb and forefinger secure and tighten the tape, bringing it into a straight line. Then I measure the distance between the two hands by noticing where, at each end, they cut across the tape. I act in this manner if the axis of the uterus coincides nearly with the axis of the body; but in those cases where the uterus is inclined to one side—to the right, as often happens—and as such an inclination is hardly distinct, except during the first two or three days after delivery, it is indispensably necessary to remove this complicating circumstance. The fundus uteri is to be cautiously pushed towards the mesial line, and left easily lying there, and then the measurement in length is to be taken in the manner which I have just described. 5. The breadth of the uterus is determined in the following manner. Three fingers of one hand are placed over the most prominent part of one side of the uterus, while the other fingers of the same hand grasp one end of the measuring tape. The other hand is arranged in a corresponding manner over the other side of the uterus, and the tape stretched tightly between the two; and then, by the eye, note is taken of the points of the tape cut by the planes of the fingers tangentially touching the uterine sides.



In this manner it is found that a uterus diminishing in size day by day will have its length as well as its breadth lessened, if involution be going on under favourable circumstances; and the degree of involution will to a great extent correspond with the amount of diminution of the uterine measurements. During the first few days of the lying-in period, the relaxed abdominal wall does not in any way influence the measurements. At any time when obtaining the measurements the abdominal wall is pressed lightly, and offers almost no obstruction to the exact determination of the size of the uterus, and this is particularly true of its breadth.

I determine the involution of the uterus by observing a diminution in its size. I shall now consider the feasibility of this, and how nearly the dimensions by my method of external measurement correspond with the true dimensions.

With regard to the breadth, its determination is not affected by variations of healthy conditions (Pfannkuch). In the case of the length of the uterus, three or four days' constipation affects the results, but in all my cases castor-oil was given every two days, and this element of fallacy avoided. Antelexion of the womb, which takes place during the first few days of the lying-in period, also affects it; and the greater the antelexion or anteversion, the smaller will the measurement be that is obtained by the method here described, and *vice versâ*. These inclinations of the uterus forwards have a causal connexion with the circumstances of puerperality. If all the women that I measured had been in the same circumstances—especially those which have an influence on antelexio uteri—that factor would be the same in all, and would therefore have no value. In order to escape as far as possible from this source of error, I made strict inquiry into the previous circumstances of all my patients which might have an effect on the amount of anteversion or antelexion during the puerperal state. I then divided them into groups, according to the knowledge thus obtained, placing into each group those women who have approximately the same degree of antelexion. The chief conditions which tend to produce anteversion of the uterus during the puerperal state are the following:—1. A greater relative size of the uterine body in comparison to its inferior part; 2. A considerable yielding of the uterine tissue at a line between its middle and inferior part; 3. A tense condition of the round ligaments, most marked in *primiparæ*; 4. A distended condition of the intestines.

To estimate correctly the length of the uterus, it is necessary to get rid of, as far as possible, those conditions which tend to produce anteversion. With this object in view, I first examined the cases of those women who were delivered at the same date, and whose uteri on the first day after delivery were of the same size; second, I separate *multiparæ* from the twinnings; third, I consider the *primiparæ* by themselves; and, last, I pay particular attention to the free evacuation of the bladder and rectum. I must here



mention, that I attempted to ascertain the length of the uterus post-partum by means of a sound introduced into its cavity. When this was done I also procured, in the same cases, the external length measurement. I have only made a few such observations, and therefore do not intend to say anything very definite with regard to this method.

The length obtained by internal measurement is greater than that procured by the external method, and there must always be a difference between them. This difference should in various days be approximately the same, for the ante flexion does not change during the first days of the puerperal state. Yet the difference is not quite constant, as may be seen by referring to the following Table :—

	Minimum.		Maximum.		Average.
1st day,	1.1 centimetres.		7.8 centimetres.		3.5 centimetres.
2nd „	2.5 „		6.2 „		4.2 „
3rd „	3.0 „		7.0 „		5.0 „
4th „	3.5 „		5.4 „		4.2 „
5th „	... „		... „		5.1 „

We cannot altogether explain the difference expressed in this Table by referring to ante flexion of the uterus. Many authors, however, assert that, while ante flexion of the uterus is well marked during the first few days after delivery, it rapidly decreases, and that accordingly the difference between the internal and external measurements will also be diminished after these first few days have elapsed.

I have already stated that generally the diminution in length and breadth of the involving uterus go on in the same degree, the latter, however, being the more constant. This being so, I shall compare it with both the methods for obtaining the length of the uterus. The results of the external method bear a pretty constant relation to the results obtained by measuring the breadth of the uterus. The relation between the results of the external and internal measurements being, as we have just seen, not at all constant, it follows that the external mode of ascertaining the length of the uterus is to be preferred to the internal one.

The disadvantages of the internal method of ascertaining the length of the uterus are as follows :—

1. From the curvature of the sound being always the same, and from the fact that with each measurement we have a varying axis of the uterus corresponding to the curvature of the sound, it follows that we do not by this method ascertain the natural length of the uterus as it lies ante flexed or anteverted, but only an artificial length as it is straightened out.

2. The sound is not always easily introduced ; its point is frequently to be felt with difficulty or not at all through the anterior abdominal wall and thick uterine wall, and you can never be certain that at each observation the point touches the same part of the fundus ;

accordingly, we may have an apparent length which is less than the real one.

3. This method is not unaccompanied with danger, however carefully it may be employed. I therefore prefer the external method, carried out with the precautions before mentioned.

Of two measurements made during the first day, I have taken the mean. I thus noted a dimension every day. All these dimensions, from the first day to the last inclusive, I added together, the result of this addition being represented by the letter ( $s$ ). If the figures thus obtained be divided by the number of days ( $j$ ), then we get the average dimension during the period ( $\frac{s}{j}$ ). If thus we determine the length, we obtain the average from the fundus uteri to symphysis pubis; if the breadth is made out, then we get the average between two lateral points of the uterus. From a dimension of the uterus obtained on the first day we subtract the dimension obtained on the last day, and divide the remainder ( $d$ ) by the number of intermediate days ( $j-1$ ). Now we determine a diminution of the uterus in one day ( $\frac{d}{j-1}$ ). Therefore, in order to *determine the absolute* degree of involution, we require to know the quantity a uterus is diminished during one day. For this purpose the degree in which the uterus has diminished in one day ( $\frac{d}{j-1}$ ) is divided by the dimension of the uterus during one mean day ( $\frac{s}{j}$ ).  $\frac{d}{j-1} : \frac{s}{j}$  is the coefficient of involution ( $T$ ). For instance, a uterus—

At the 1st day,	12 centimetres
„ 2nd „	10·8 „
„ 3rd „	9·72 „
„ 4th „	8·748 „

Here  $s = 41,268$ ;  $j = 4$ ;  $j-1 = 3$ . The degree of diminution in length is represented by

$$T = \frac{3 \cdot 252}{3} : \frac{41,268}{4} = 0 \cdot 105.$$

In this manner the above formula gives the absolute rate of involution. It allows us to compare uteri of different sizes; in other words, those whose relative dimensions are not equal.

I determine the degree of regularity of involution from the most constant dimension, that of breadth, in the following manner:—Let me take, for instance, the involution of the uterus in the case of *thrombus vaginæ*. The degree of involution  $T = \frac{1 \cdot 6}{8} : \frac{119 \cdot 7}{9} = 0 \cdot 015$ .

The daily diminution of the uterus should be  $\frac{1 \cdot 6}{8}$  centimetres = 2 mm.,

if the process go on regularly. For the sake of comparison, I take two forms of diminution, a real and an ideal one.

	Real diminution.	Ideal diminution.
1st day,	13·5	13·5
2nd „	14·0	13·3
3rd „	14·8	13·1
4th „	14·0	12·9
5th „	13·2	12·7
6th „	13·6	12·5
7th „	12·7	12·3
8th „	12·0	12·1
9th „	11·9	11·9

I then notice the difference in the involution on the same days between the two forms of diminution. We perceive the following differences : for the first and last days = 0 ; for the second, 0·7 ctm. ; for the third, 1·7 ; for the fourth, 1·1 ; for the fifth, 0·5 ; for the sixth, 1·1 ; for the seventh, 0·4 ; for the eighth, 0·1 ctm. We next determine the average difference, which tests the degree of regularity of involution, and will show how regularly the involution has gone on. In the case alluded to, this regular diminution is  $\frac{5\cdot6}{7}$  ctm. = 0·8 ctm. = 8 mm.

The author then examines 150 cases which he has tabulated, and arrives at the following conclusions :—

1. Involution of the uterus goes on more rapidly during the first few days of the puerperal period than it subsequently does.
2. Involution of the uterus of healthy women goes on well and with regularity.
3. Involution, where the uterus is the subject of disease, such as metritis, endometritis, or parametritis, goes on more slowly, and this varies with the amount of disease.
4. The permanent contraction which takes place during the first few hours after delivery is a common occurrence. When it passes off an increase in size begins to take place.
5. In women delivered at the full time, involution goes on more quickly and regularly than in those prematurely confined.
6. Length of labour retards involution.
7. In *primiparæ* involution of the uterus goes on very regularly, but more slowly than in young *multiparæ*. In aged *multiparæ* involution does not go on so well.
8. In women who suckle their children, involution during the first four days does not go on so quickly as in those who do not nurse. But, subsequently, the involution is quicker, though less regular.
9. After-pains are not necessary for a favourable involution ; in fact, we are as well without them.

10. In order to determine the involution of the uterus, you should only measure its breadth.

11. Involution of the uterus goes on proportionally in length as well as breadth.

12. Super-involution and sub-involution occur as distinct uncomplicated pathological conditions.

---

## Obstetric Summary.

### *On the Causation of the so-called Cephalhematoma.*

Dr. Heinrich Fritsch writing on this subject (*Centralbl. f. d. Mediz. Wissensch.* No. 29, 1875), says that as yet no one has diagnosed before birth the existence of a cephalhematoma. When the advancing head is driven forward by the pains, and closely pressed against the pelvic walls, the elasticity of the bones of the head, and the resistance from the soft parts of the pelvis, cause a recession of the head. The scalp, closely applied to the maternal passage at the greatest periphery or only on one side, is held back, and forms as it were with the soft parts of the mother one mass. Danger may result from this close adhesion in two ways. First, in the farther descent of the head the vaginal walls may be loosened from the underlying tissues, and prolapse and descent of the vagina follow. Secondly, in the recession of the head the scalp may be loosened from the parts beneath. The pericranium is detached from the bones, since here the connexion is not elastic as in the upper layers.

Through this breach of continuity a gradual flow of blood takes place; the formation of a tumour is for a time prevented through the close pressure of the head against the maternal parts. But after the birth the blood continues to collect till the pressure of the blood in the cephalhematoma is greater than that in the bleeding vessels. The greater the extent of the detachment of the pericranium, the greater the cephalhematoma. The tumour never forms in the middle of the vertex, and is seldom met with in the narrow pelvis, because here for many reasons the folds of the scalp more rarely occur.

A cephalhematoma may be produced by the forceps, or in too great rotation.

---

### *Chloral in Puerperal Convulsions.*

Dr. Portal (in *Bull. Gén. de Thérapeutique*, August 15th, 1875) relates three instances where chloral was successfully employed. Albuminuria was present in each. The first was attacked six hours after labour, the two others during parturition. One was delivered naturally, during the attack, of a still-born child; in the other case forceps were applied, on account of the pains having disappeared when the attack had ceased. The first had had twenty-four attacks, coming



on regularly every quarter of an hour ; the second eight ; and the third seven attacks. Ninety grains of chloral were administered in each case. In the two latter, twenty-five milligrammes of morphia were also injected. All the patients recovered. In six previous cases treated by leeches and inhalation of chloroform, the author "had six deaths to deplore."

---

*Medico-Legal Aspect of Abortions.*

Dr. Leblonde (in *Ann. de Gynecol.*, August, 1875) has collected a series of eleven cases from which he endeavours to prove the medico-legal value of the integrity of the membranes in abortions in the early months of pregnancy.

His conclusions are thus stated :—

1st. When abortion occurs "en bloc"—*i.e.*, the embryo is contained in the sound membranes, which are unbroken—abortion is probably spontaneous, or at least has not been produced by agents which determine the expulsion of the ovum without implicating the membranes.

2nd. When the membranes are ruptured, but healthy, in all probability abortion has been provoked.

3rd. When the membranes present pathological alterations, we can form no conclusion from an examination of the expelled product, though probably the abortion results from disease of the ovum, and that it is due to spontaneous production.

---

BOOKS, PAMPHLETS, AND PAPERS RECEIVED.

"On the Relation between Diabetes and Food, and its Application to the Treatment of the Disease." By Arthur Scott Donkin, M.D. London. Smith, Elder and Co. 1875. Pp. 186.

"Report on Diseases of Women for the Year 1874." By Paul F. Mundé, M.D. New York. 1875.

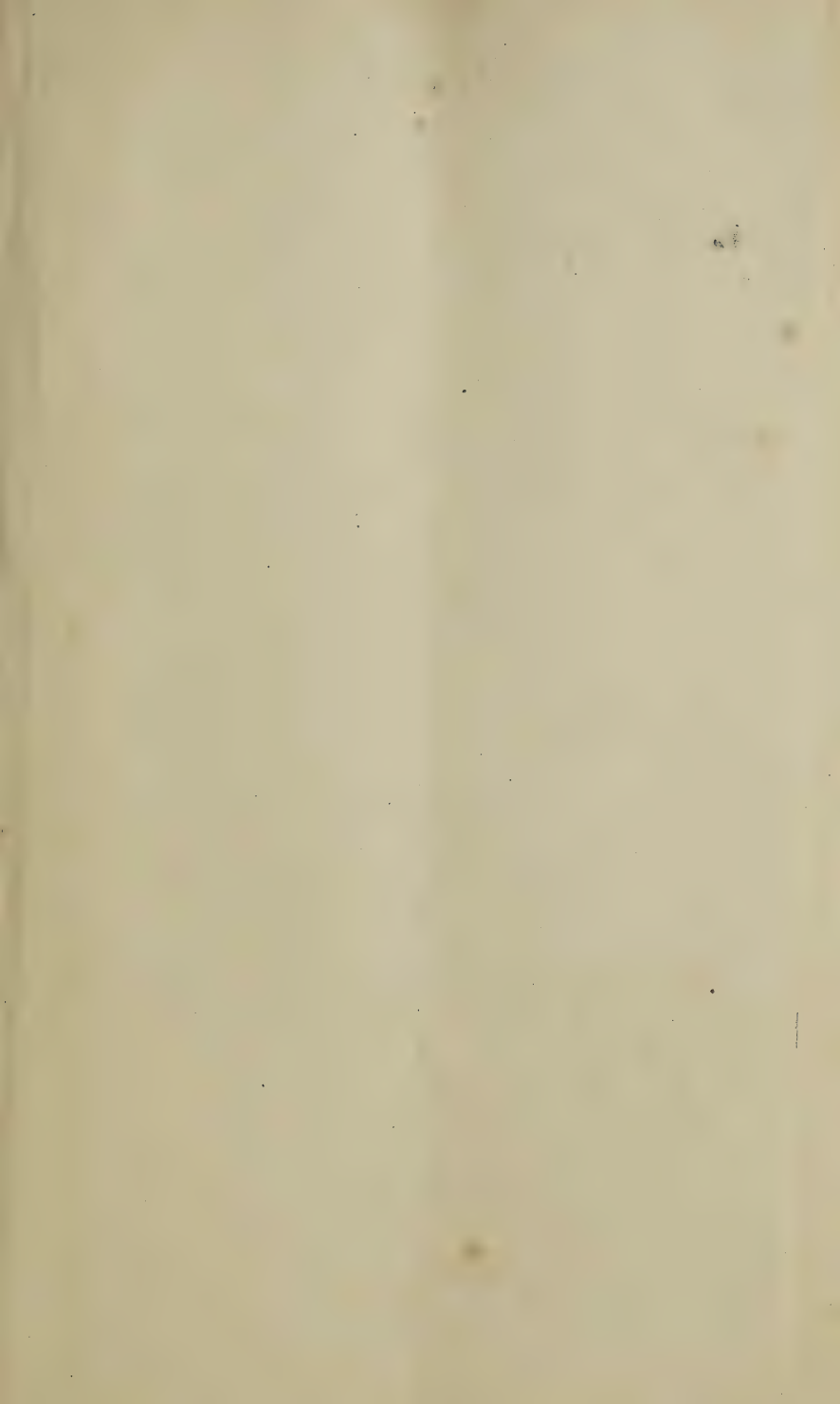
"The Climate and Diseases of America." By Dr. Johann David Schoepff. Translated by J. R. Chadwick, M.A., M.D. Boston. 1875.

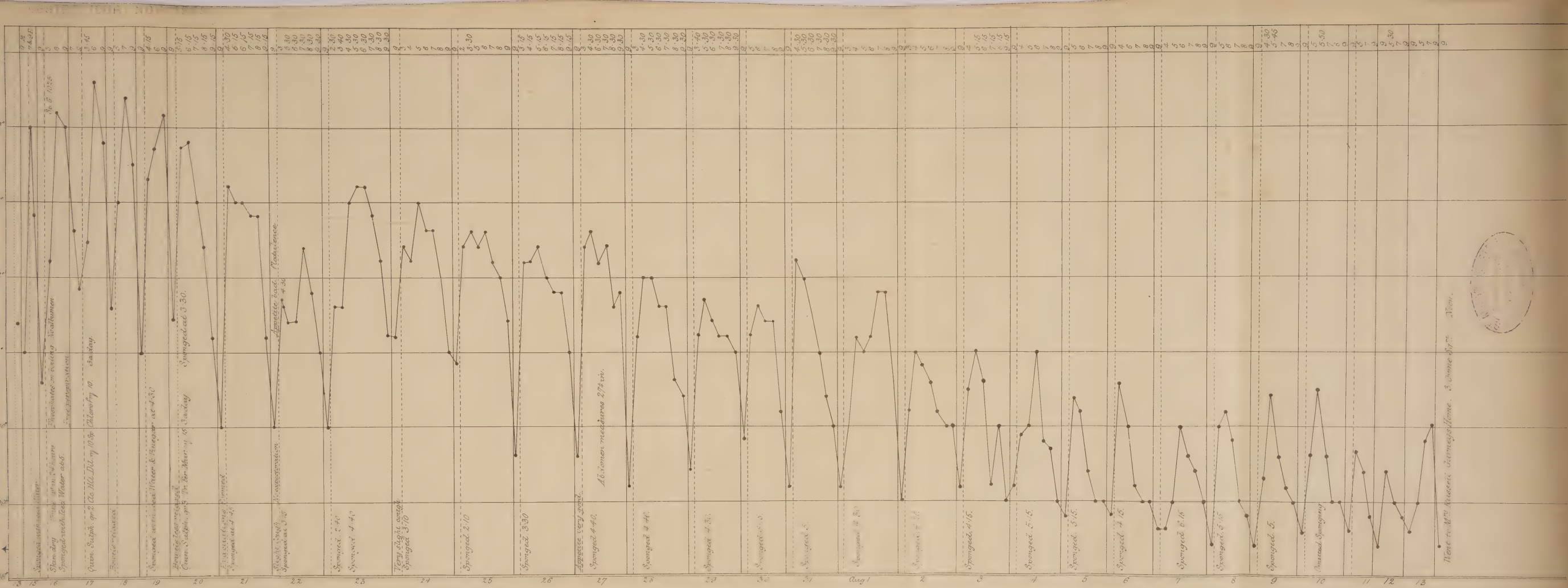
Communications have been received from Dr. Madge, Dr. Giles, Dr. John Brunton, Mr. Lawson Tait, Dr. Edis, and Dr. John Williams.

---

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.





None to Mrs. Russell, Samaritan Home, 3, some 875. Nov.

# THE OBSTETRICAL JOURNAL

OF  
GREAT BRITAIN AND IRELAND.

~~~~~  
No. XXXII.—NOVEMBER, 1875.  
~~~~~

## Original Communications.

---

### ABDOMINAL CYST WITH PULMONARY COMPLICATION.

By GEO. GRANVILLE BANTOCK, M.D.

Physician to the Samaritan Free Hospital for Women and Children.

*Abdominal Cyst (probably Parovarian); Chronic Pneumonia, left side; high temperature reduced by external use of cold; disappearance of the cyst, and recovery from the pulmonary affection.*

ON the 13th of July, 1875, E. S., aged fourteen, a dress-maker, presented herself at the out-patient department of the Samaritan Free Hospital, before my colleague, Dr. Clement Godson, complaining of "a swelling in the stomach." It was elicited that the menses first appeared soon after she had attained the age of twelve, were absent for nearly a year after that, that they returned at regular and normal intervals for three months, and that they had again been absent for the last three months. Her cheeks were flushed, and she had an anxious expression; the tongue was red, rather dry, and glistening; skin hot and dry, and the temperature was  $101^{\circ}4$  (about 2.30 P.M.). A cursory examination gave the following results:—Abdomen evidently distended by a cyst which reaches as high as the umbilicus; fluctuation is very



distinct, and the walls of the cyst appear to be very thin. There is no tenderness. Percussion note clear in both flanks and in the epigastrium close up to the cyst. Per vaginam : the cervix short ; os normal size ; body of uterus pushed downwards and backwards into utero-rectal pouch ; on bimanual examination, no fluctuation can be felt by the finger in the vagina.

No examination of the chest was made, and as the poor girl was too ill to continue an out-patient, Dr. Godson transferred her to my care for admission into the Hospital. She was accordingly sent home for the day with a simple effervescing mixture, to be taken every four hours.

Next day she entered the hospital—in the evening—and was at once put to bed. Perspiration to be encouraged as much as possible. At 9 P.M. the temp. was  $102^{\circ}8$  ; pulse 136.

15<sup>th</sup>.—Patient has slept well, perspiring a little in the night. Temp.  $101^{\circ}$  at 9 A.M.

At 4.30 P.M. Examination made with following results :—Skin hot, slightly moist ; tongue clean and red ; considerable thirst ; appetite small ; bowels open ; urine free, high coloured ; cough troublesome, but no expectoration. Resp. 34 ; pulse 128. Has no pain in the chest, but the breathing appears shallow. On auscultation, both lungs are resonant *anteriorly*, without râles or increase of vocal resonance ; respiratory sounds almost puerile. *Posteriorly* :—Right lung resonant all over ; no râles nor increase of vocal resonance. *Left* :—At base, and including lowest third of lung, there are indistinct crackling, slight sibilus on coughing, and increased vocal resonance, and the respiratory sounds are muffled. Upwards over remainder of lung there is increase of vocal resonance only.

On examining the abdomen again, the condition was found the same as two days before, and the following measurements were taken, viz. :—Circumference at umbilicus,  $31\frac{1}{4}$  in. ; from ensiform cartilage to umbilicus,  $5\frac{1}{2}$  in. ; pubis to umbilicus,  $7\frac{1}{4}$  in. ; right anterior superior spine of ilium to umbilicus,  $7\frac{1}{4}$  in. ; left anterior superior spine of ilium to umbilicus,  $7\frac{1}{4}$  in. There is no tenderness in any part of the

abdomen. No history could be obtained of a rigor, or any condition pointing to the commencement of the pulmonary symptoms, and it was as late as a week ago that she first became aware of the increased size of her abdomen.

*Diagnosis.*—"Probably a parovarian cyst, complicated with chronic pneumonia."

*Treatment.*—For the effervescing mixture was substituted the following:—Liq. am. acet. fl. dr. j, sp. ætheris nitr. fl. dr.  $\frac{1}{2}$ , every two hours in 2 fl. oz. of water. *Diet*:—As much milk as the patient can drink, bread and milk, beef tea, &c., but essentially a milky, farinaceous diet, an egg if desired.

According to instructions, at 7, temperature being  $104^{\circ}$ , the patient was sponged back and front with iced water. At 9 she was perspiring freely, and the temperature had come down to  $102^{\circ}8$ .

16th.—Restless night, but no cough. At 9 A.M. temp.  $100^{\circ}6$ . 3.30 P.M. Skin hot and dry; tongue clean; not much thirst. Temp.  $102^{\circ}2$ ; pulse 128; resp. 28. Urine in 24 hours 1 pint, passed in two quantities only, smelling rather strong, slight alkaline reaction, sp. gr. 1025, throws down phosphates on boiling, but no albumen. At 5, sponged with iced water. At 6, temp.  $104^{\circ}2$ , and at 9, temp.  $104^{\circ}$ , when she was perspiring very freely.

17th.—At 7 A.M. temp. was  $102^{\circ}6$ , and by 9 it came down to  $101^{\circ}8$ , after which she had an egg for breakfast. At noon she dined off sole and custard, and in the afternoon was examined by Dr. Day, who reported as follows:—

"Mother died of dropsy. Father living, and in good health. Phthisis not hereditary. Very pallid and delicate, and continuously sweating (wakes up from sleep perspiring); has a slight, irritable cough, but has never had hemoptysis. Tongue red, clean; thirst. Pulse 130; resp. 36. *Thorax*:—On respiration the chest is lifted up, as it were, by forced action of the respiratory muscles, instead of by the gradual elevation due to the air fully entering the pulmonary tissue. The chest is fairly resonant in front, and there are no moist sounds; the percussion note along the line of the right clavicle is duller than the opposite side, and expiration is a little harsh and prolonged below it. Vocal vibration is dis-

tinct in this situation, but not so on the left side. In the right supra-spinous fossa the respiration is a little whistling, and the note on percussion less resonant than on the other side. In the right axilla respiration is perceptible and good, but it is very defective in the infra-mammary region, and there is general dulness (this is from abdominal pressure). The right back is resonant to the eighth rib ; below this it is dull. *Left side* :—Respiration is heard throughout, except in lowest lobe ; a little dry rhonchus is heard at the left base, which arises from pressure, because the size of the abdomen does not allow the lower lobe to expand freely ; lower lobe dull on percussion. There is an increase of vocal resonance in both backs, but no moist sounds. Throughout the posterior portion of the lungs the respiration is feeble, defective, and wanting in softness. A little harsh respiration is heard in the left axilla, but none below.

The abdominal pressure accounts for the limited breath-sounds in lower lobes of lungs and infra-mammary regions. If the temperature was normal I should not consider that the chest contra-indicated an operation, supposing the patient's health was seriously suffering from the morbid growth. At present it cannot be undertaken.

The condition is very like tuberculosis.

At 3.45 P.M. temp.  $102^{\circ}5$  ; pulse 126 ; resp. 28. Prescribed quin. sulph. gr. 2, acid. hydrochlor. dil.  $\mathfrak{m}$  10, sp. chlorof.  $\mathfrak{m}$  10, every 4 hours.

6 P.M.—Temp.  $104^{\circ}6$ . 8.30. A little sick. 9.30. Temp.  $103^{\circ}8$ .

18th.—Patient had passed a fair night, but in the morning again a little sick ; tongue very dry, and bowels rather relaxed ; less thirst, and scarcely any cough. At 9 A.M. temp.  $101^{\circ}6$ . At 5 P.M. temp.  $103^{\circ}$  ; pulse 130. At 7, temp.  $104^{\circ}4$ , and at 9, temp.  $103^{\circ}5$ . In order to estimate its value, the sponging was omitted yesterday and to-day.

19th.—9 A.M. Temp.  $101^{\circ}$ . Slept well. Appetite bad. Bowels not so relaxed. At 4.30 P.M. sponged with iced water and vinegar.

20th.—Bowels more quiet. Ordered—Quin. 3 grains, tr. fer. mur.  $\mathfrak{m}$  15, three times a day.

21st.—Stools now formed. It would be tedious to relate the daily symptoms, &c. ; suffice it to say that the body was sponged daily when the temperature was ascertained to be decidedly rising. She was sick several times, and suffered much from flatulence ; but on the

27th.—The appetite was very good ; bowels regular ; tongue looked more healthy and less red ; she has no thirst, and no cough. On examination, the abdomen now measures only  $27\frac{1}{4}$  in. in circumference at umbilicus, and the cyst is no longer to be felt. Confirmed by Dr. Godson. No vaginal examination made.

August 1st.—Occasional stitch in left side on taking a deep inspiration. Resp. 24 ; pulse 107 ; highest temp.  $101^{\circ}2$ .

3rd.—Circumference at umbilicus  $26\frac{1}{4}$  in. Appetite very good ; eats meat daily ; bowels act regularly ; no cough ; perspiration limited to a few hours following the sponging. General appearance of the patient much improved ; gaining flesh.

6th.—Examination of chest this day. *Anteriorly*.—Respiratory sounds normal. *Posteriorly*.—Slight dulness on percussion on left side about an inch above lower angle of scapula, over a patch about 2 inches in diameter. Here the vocal resonance is almost *ægophonic*. The breath-sounds are feeble, distant, and bronchial. Superiorly they are slightly bronchial, as well as under axilla, and the vocal resonance is in excess. At the extreme base the sibilations have disappeared, and the vocal resonance is not so pronounced. The right back is normal.

10th.—Highest temperature  $100^{\circ}5$ . Sponging discontinued.

14th.—Left the hospital for Mrs. Russell Gurney's Home. To continue the mixture for another week.

*Remarks*.—The most interesting point in this case, and that which specially deserves recording, is the rapid disappearance of the abdominal cyst. When I first examined the patient, in conjunction with Dr. Godson, I arrived at the conclusion that the cyst was of parovarian origin, for the following reasons—viz., the age of the patient, its undoubted



unilocular character, and the thinness of the walls ; and in this I was supported by my colleague. The presence of high temperature suggested the idea of inflammation of the cyst, but the absence of tenderness or feeling of distension at once negatived it, and it was therefore assumed that the hyperpyrexia was due to some pulmonary complication, which surrounding circumstances and the condition of the patient precluded us from verifying. After the patient's admission, the existence of the cyst, which could not be regarded as producing any mischief by pressure, and did not call for special treatment, was overshadowed by the grave pulmonary complication, to which the treatment was entirely directed. Thus it happened that on the 27th of July (13th day) Dr. Godson and myself were struck by the decrease in size of the abdomen and disappearance of the cyst, as far as we could ascertain by external palpation. Yet it was evident, at a later period, that the cyst had not attained its smallest dimensions ; for on the 3rd of August (seven days later) a further decrease had taken place, and she now measured only  $26\frac{1}{4}$  inches, and this coincident with general improvement in the patient's condition, and apparent increase of flesh. Of course it cannot be absolutely affirmed that the cyst was parovarian\* (the diagnosis was one of probabilities), but the history of the case tends to confirm the opinion at first formed. The disappearance was evidently due to absorption of the contents of the cyst, and not to their discharge through an excretory channel with direct communication, for there was none either by urethra, vagina, or rectum. And when we consider the nature of the parovarian cyst and its relations to surrounding parts, it would be difficult to conceive such a result, except as a sequence of inflammatory adhesion and ulceration, while it is not difficult to perceive how, by the increased activity of the skin (and probably the febrile state had some share), the balance was restored, as so often happens after a simple tapping. The view might be adopted that the febrile state and the cyst stood in the relation of cause and effect, but one would in such case expect the cyst

---

\* See paper in "Obstetrical Society Transactions," vol. xv. p. 105 *et seq.*

to be very tense and more or less tender, which was not the case.

The case is scarcely less interesting as regards the pulmonary complication. The gradual progress to localized consolidation, and its equally gradual subsidence point, as I regard it, to a condition of chronic or subacute pneumonia rather than tuberculosis, and it will be seen that my colleague—Dr. Day—though inclining, did not commit himself, to the latter view. There is no history of hereditary tendency to tuberculosis, while the free perspirations noticed in Dr. Day's report were the result of the treatment rather than of the disease. When the patient first came under observation the skin was for the most part dry and burning, and it was not till the sponging had been begun that its action was thoroughly restored. From that time the body was swathed in flannel, and the perspiration was very free. Bearing in mind the extraordinary results obtained by others\* in the treatment of hyperpyrexia, and my own observations—notably in a case of convalescence from ovariectomy, in which septicemia threatened, and in which by sponging the body till cold (with iced water), the temperature was reduced  $4^{\circ}.8$  in three hours, as well as in the febrile affections of children—I determined to avail myself of the aid of this powerful remedy. The result was beyond my expectations. The mode in which it was used was as follows:—The patient being laid on her face, and the body sufficiently exposed, the back and sides were sponged as rapidly as possible, taking care, by frequent dipping, to keep the sponge cold, turning it round after each stroke, until the skin felt icy-cold. As soon as this was effected, the body was rapidly dried, a broad strip of flannel was laid across the back, the night-dress was adjusted, and the patient turned on her back. The front of the body was then treated in the same manner, the two ends of the roller—wide enough to reach from the axilla to the ilium—were then drawn across so as to overlap. Afterwards the lower extremities were subjected to the same process, with this exception, that they were not wrapped up.

---

\* Wilson Fox, "On Hyperpyrexia," &c.

In the course of about half an hour free perspiration had set in, and was encouraged ; and, as the temperature fell, the patient was relieved of some of the bedclothes—carefully, so as to avoid a chill. At first the process was very agreeable, but as the temperature became reduced it was less so. The time selected was when the temperature was decidedly rising—usually about 4 P.M.—and it was found that if done as the temperature was only beginning to rise, the result was not so satisfactory. Iced water only was at first used, but on the 19th of July (fifth day of admission) vinegar was added in the proportion of about two of water to one of vinegar, and this was continued throughout.

The accompanying chart gives a bird's eye view of the course of the temperature, and is in itself an interesting study, on which I need not further dwell.

I saw the patient several times after her admission to the Convalescent Home, and it was very interesting to observe the gradual subsidence of the pulmonary lesion until, at the end of five weeks, no trace remained. She was, moreover, looking remarkably well ; had no cough nor perspirations.

She returned home at the end of September.

---

## THE MUCOUS MEMBRANE OF THE BODY OF THE UTERUS.

By JOHN WILLIAMS, M.D. Lond.

Assistant Obstetric Physician to University College Hospital.

THE expression "Mucous Membrane of the Body of the Uterus" has been used to denote that layer on the inner surface of the organ which is superficial to the "Muscular Wall," the term muscular wall being applied to the whole thickness of the uterine parietes in which muscular fibre-cells are found.

The structure of mucous membranes varies considerably in different situations. A typical specimen—such as that of the stomach—consists of a superficial layer of epithelium, connective tissue, glands, blood-vessels, nerves, and lymphatics, together with a stratum of muscular fibre-cells resting upon

a layer of areolar tissue named "submucous." The said layer of muscular fibre-cells is called *muscularis mucosæ*, and in the situation referred to sends prolongations upwards between the gastric glands; that is, the glands enter into but do not pass through the stratum of muscular fibre-cells which enters into the formation of the mucous membrane.

The elements enumerated vary much in quantity in different membranes. The muscularis may be of some thickness, scanty, or even wanting; the glands few or many; the membrane rich in blood-vessels or but slightly vascular. The mucous membrane of the body of the uterus has been described as being thin, soft, pale, smooth, glandular, and closely adherent to the subjacent tissue. At the same time, it has been long known that it varies at different periods in thickness, consistence, and vascularity. At one time it measures in some situations  $\frac{1}{4}$  inch, or even more, in thickness, and is of a dark red colour from injection of its vessels; at another time it is of extreme thinness, and quite pale; and it may be even absent, for muscular fibre-cells have been seen running to the inner surface of the uterine wall. The term mucous membrane has not been used to include any portion of the muscular wall until recently. In a paper read before the Obstetrical Society of London (*Obst. Trans.*, vol. xvi. page 206) I stated that I regarded the uterus not as an organ possessing a mucous membrane, but as a mucous membrane whose muscular fibre-cells had undergone great development. Since that time I have had opportunities, through the kindness of Professor A. H. Garrod, of examining the uteri of a considerable number of different animals, and though the result has not been to confirm the above opinion in its entirety, yet it has been to show that the mucous membrane of the body of the uterus possesses a very highly developed muscularis—that is, a considerable portion of the thickness of the uterine wall is formed by *muscularis mucosæ*.

It is not necessary for me to enter separately into the evidence which shows that the mucosa uteri possesses a muscularis, inasmuch as it is included in that about to be brought forward to determine the exact thickness of the

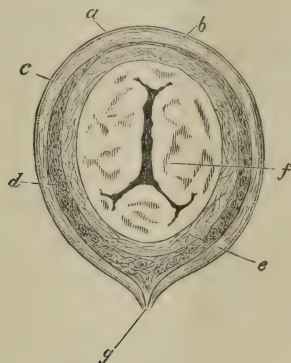


uterine wall which belongs to that tissue. Mucous membranes generally are attached to those subjacent by a layer of areolar tissue. In some cases this union is loose, and permits a certain degree of movement of one tissue on the other. This is especially the case in hollow muscular organs. In the human uterus the submucous layer of areolar tissue has not been found. I think, however, that its situation may be discovered by an examination of the uteri of the lower animals.

In a cross section of the uterus of the common Roe deer I have seen a thin layer of connective tissue dividing the muscular wall into two layers. Between it and the peritoneum is a thin stratum of muscular tissue, the fibres of which appear to run in a longitudinal and circular or transverse direction. Between it and the uterine cavity is a distinct muscle, which is of about the same thickness as that which separates it from the peritoneum. On the surface of this muscle lie the soft tissue and glands, usually called the mucous membrane. There is a marked and abrupt distinction between this soft tissue and the underlying stratum of muscular fibre-cells; and the glands which are lined by columnar epithelium do not appear to penetrate deeper than the soft tissue. In the layer of connective tissue which divides the muscular wall into two strata the blood-vessels run. This is a point of no little importance, inasmuch as the position of the blood-vessels is the only indication which we find in the human uterus of the situation of the submucous tissue. It should be borne in mind that the branches which supply the mucous membrane of the stomach lie in the submucous connective tissue.

If we make an incision through the wall of the stomach or intestine, the edges of the wound retract, and the mucous membrane protrudes through it. A similar appearance is presented by the uterus of a sheep when cut across. The peritoneum (*a*) and the subjacent layer (*b*, *c*) of the uterine wall become retracted, and the central portion (*e*, *f*) protruded. Immediately under the peritoneum is a thin layer of muscular fibre-cells arranged longitudinally (*b*); next to this is a layer of transversely or circularly arranged muscular fibre-cells (*c*). The stratum formed by these two sets of muscles is thickest on that part of the cornu which

is opposite to the attachment of the broad ligament ; they become thinner as they approach the latter situation, and in great part pass into that structure. On the inner surface of



Section across cornu of sheep's uterus (diagrammatic) :— *a*, peritoneum ; *b*, *c*, longitudinal and circular fibre cells ; *d*, connective tissue and blood-vessels (submucous) ; *e*, muscularis mucosæ ; *f*, soft tissue usually called mucous membrane ; *g*, broad ligament.

the circularly arranged fibres is a layer of connective tissue (*d*). This is well-marked near the insertion of the broad ligament, but very thin in the part opposite. The vessels run between the layers of the ligament and enter the stratum of connective tissue, and give off branches to ramify in the mucous membrane. This layer of connective tissue is doubtless the "submucous." On its inner surface is a stratum of muscular fibre-cells (*e*) of some thickness, and still more inwards is the soft tissue of the mucous membrane. Glands lie in the soft tissue and penetrate into the subjacent muscular layer. The whole structure protruding in a cross section—that is, the soft tissue and the subjacent muscular layer—can be without much difficulty peeled off from the layer of circular fibres, just as the mucous membrane of the stomach can be peeled off from the proper muscular wall of the organ. There can be little doubt, I think, that the true mucous membrane of the sheep's uterus includes not only the superficial soft tissue, but the stratum of muscular fibre-cells beneath it and internal to the larger branches of blood-vessels.

Having described the structure of the cornua, that of the

body becomes clear. The two cornua become united, and the septum between them absorbed in the formation of the body of the uterus. The vessels enter at each border between the layers of the broad ligaments, send branches across the anterior and posterior surfaces under the peritoneum, but separated from it by a thin layer of muscular fibre-cells, the outer of which are arranged longitudinally, and the inner transversely. This layer is thickest in the median line, and becomes gradually thinner towards the borders where the broad ligaments are attached. The whole thickness of the uterine wall internal to the layer of connective tissue in which the vessels run, is the mucous membrane. As has been already stated, the submucous tissue has not been found in the human uterus; at the same time, it must be added that it has been looked for in a place to which analogy does not point as its seat—that is, between the soft tissue and the muscular fibre-cells. The only indication I have found of its position is the situation of the larger trunks of the blood-vessels, and this corresponds with its position in the uterus of the sheep. The vessels approach the borders of the uterus between the layers of the broad ligaments, and before entering the uterine wall, divide. They do not, however, send branches directly towards the inner, but across the anterior and posterior surfaces of the organ, under the peritoneum, and separated from it by a layer of muscular fibre-cells. This layer varies somewhat in thickness in different uteri. The greatest thickness I have seen it attain is less than  $\frac{1}{4}$  inch. Around the vessels in this situation connective tissue seems to be more abundant than elsewhere, though it does not divide the muscular wall into two distinct layers as in the sheep. According to this view, rather more than three-fourths of the thickness of the wall of the uterus is mucosa.

The next point to be considered is the presence of glands. It has been already stated that in the stomach glands enter, but do not penetrate through the *muscularis mucosæ*. A similar condition is met with in the uterus. Glands lined by columnar epithelium are found at certain periods in the

meshes formed by the fibrous bundles of the muscular wall. This has been lately confirmed by Professor Turner and Dr. Underhill. Glands have also in some instances been traced to groups of round cells in the muscular wall, and such groups are found scattered through the uterine parietes; and if these groups of cells be the deeper portions of glands, the latter penetrate much further into the wall than has been supposed. Glands, however, have not been as yet traced to their deep terminations, and until that is done we must fall back upon the comparative study of the submucous areolar tissue of the uterus in order to determine the limits of the mucous membrane.

The changes which take place from month to month in the uterus—that is, the removal of the so-called mucous membrane and its renewal by proliferation of the superficial *laminæ* of the muscular wall—show that a portion of that muscular wall is *muscularis mucosæ*. It had been observed long ago that the mucous membrane of the body of the uterus was wanting in the uteri of persons who had died at the close of the menstrual flow. I have had opportunities of confirming this observation. Dr. Underhill has published a very valuable note which bears on this point (*Edin. Med. Journ.*, Aug. 1875). It contains a description of the uterus of a woman who died “when she had barely finished menstruating.”

He says, with reference to the wall of the uterus—“On making sections of the parts thus hardened, it was found that the superficial layers of the mucous membrane were wanting. On the surface of what remained, and between the upper *laminæ* of the membrane, was a mass of débris, consisting of round and spindle-shaped cells, smooth muscular fibres (or what appeared to be such), blood-corpuscles, and gland epithelium. Below these *laminæ* there appear to be rows of glands alternating with muscular and fibrous structure—that is to say, the glands have their long axes parallel to the free mucous surface; some, however, were cut across obliquely or transversely.”

It seems to me from the above description, which corresponds very much with what I have figured and described, that the uterine mucous membrane (using the expression in its



ordinary sense) had been in part removed, and that what was left was in process of removal ; because muscular fibre-cells ran not only to the surface of the uterine wall, but were found in the débris covering it. Dr. Underhill states that he cannot endorse my views as to the shedding of the entire mucous membrane during menstruation, but this appears to be due to the fact that he attaches a meaning to the term mucous membrane other than the usual one—the one adopted in my paper referred to by him—for he makes it include that portion of the muscular wall where glands are found.

The theoretical objection to the view that the so-called mucous membrane of the uterus is renewed by proliferation of the superficial *laminae* of the muscular wall is based on the fallacy of regarding that wall as a muscle pure and simple. I have pointed out in this paper that it is much more—that it is indeed part of the mucous membrane itself, and that the terms muscular wall and mucous membrane as they are generally applied to the uterus are misnomers.

Dr. Engelmann has impugned the accuracy of the view that the mucous membrane of the uterus is removed and renewed again every month, in the following terms :—

“ In two of our cases, girls who died suddenly a few days before the catamenial period, the precise time of which could unfortunately not be ascertained, and in a number of others in which we had no record of the time of menstruation, the mucous membrane presented already the less compact, tumefied, appearance characteristic of the menstrual condition, while no signs of ovulation, no recently ruptured follicles could be found in the ovaries.

“ In other cases, in which the catamenial discharge was said to have ceased several days before death, and in which well-developed corpora lutea were found, the mucous membrane was still more or less tumefied, though not to the same degree as during the continuance of the hemorrhage.

“ This is the condition I have always found existing soon after the cessation of the catamenia ; in not one of the many uteri examined at such periods was the mucous membrane, or even its superficial layer, found wanting ; a fact which I should have deemed wholly unnecessary to mention, were it

not that theories such as Pouchet's, that the mucous membrane is shed at each catamenial period, seems still to be accepted by some, and quite lately it has again been revived by Williams, who would have a new mucous membrane formed in each inter-menstrual period only to pass away as sanguineous débris with the next catamenial flow. He tells us that 'when hemorrhage has taken place into the membrane, it undergoes rapid disintegration and becomes entirely removed.' After having established this physiological improbability, to which we can find nothing analogous in the human system, he endeavours to explain the extremely rapid restoration of the membrane by a physiological incongruity. As he has left nothing of the mucous membrane from which its restoration may take place, he allows it to spring from the muscular tissue. To use his own words: 'The membrane is produced by proliferation of the elements of the muscular wall of the organ, the muscular fibres producing the fusiform cells, the connective tissue the round cells, and the groups of round cells in the meshes formed by the muscular bundles the glandular epithelium.' By such fallacies alone can theories like the above be explained." (*American Journal of Obstetrics*, May, 1875, pp. 39-40.)

Again, with reference to the time of the discharge of the ovum, he writes on page 42: "These observations are moreover fully corroborated by cases cited by Dalton in his prize essay 'On the Corpus Luteum of Menstruation and Pregnancy,' two of whose cases especially tend to prove the position here taken, and I will cite both, as they cover a time of the catamenial period not observed by me. In the first case, in which death occurred during the menstrual period, an enlarged Graafian vesicle, not yet ruptured, was found in the ovary. In the second, death had taken place at the termination of the period, and the ovary revealed a Graafian vesicle, prominent, and on the point of bursting."

I have quoted the foregoing paragraphs because they contain a statement of the material for the determination of the point at issue, at Dr. Engelmann's disposal. The theoretical objection I have already dealt with, and I simply wish to point out that it appears from the above extract—

1. That in no case examined by Dr. Engelmann was the date of the menstrual flow known.

2. That his cases consist of those in which patients had died a "few days" before menstruation, cases in which death had occurred "several days" after the catamenial discharge had ceased, and a number of others in which there was no record of menstruation.

3. That Dr. Engelmann has never examined a case in which death had taken place during or at the termination of the menstrual flow.

I submit that from such data it is not possible to speak with any degree of probability, much less certainty, of the state of the uterus towards the close of the menstrual flow, and consequently of the removal and renewal of its so-called mucous membrane. I further submit that the only method likely to throw any light on this subject is the examination and comparison of a number of cases forming a series, embracing the whole period of four weeks.

## ON THE AVOIDANCE OF THE FREQUENT USE OF THE SPECULUM

IN CERTAIN CASES OF UTERINE DISEASE, BY THE  
EMPLOYMENT OF NITRIC ACID AS A CAUSTIC,  
INSTEAD OF NITRATE OF SILVER.

By JAMES BRAITHWAITE, M.D. LOND.

Lecturer on Midwifery and Diseases of Women and Children at the Leeds School of Medicine; Co-Editor of Braithwaite's "Retrospect of Medicine;" Assistant Surgeon to the Leeds Hospital for Women and Children.

CASES of ulceration or erosion of the os uteri, with or without endo-cervicitis, are so common, and in hospital practice so numerous, that some more efficient and less troublesome caustic than nitrate of silver is urgently needed for their treatment. I have carefully and fully tried the action of most caustics (including carbolic acid), and found none to answer the purpose so well as nitric acid. The great fault of nitrate of silver is the fugitive nature of its action; its influence seldom extends beyond five or six days, even when rubbed upon and held in contact with the parts. It is more a stimulant than a caustic, as it produces no slough, and

causes extreme turgescence of the capillaries immediately below the surface influenced, as evidenced by the occurrence of hemorrhage, often sufficient to obscure the parts before it has been removed from contact with them. At the second examination we often find the ulceration or erosion little if at all altered in appearance. This defect must be atoned for by the frequent reapplication of the remedy, necessitating each time the use of the speculum. I believe in this evil really lies the source of the opinion held by some eminent men, that these diseases require little or no local treatment, for our opinions are often unconsciously influenced by our wishes. Nitric acid, on the other hand, is a really efficient caustic, producing a slough, which is peculiarly firmly adherent, and which consequently necessitates a healthy effort by the subjacent parts for its separation. The only other caustic which produces a slough of the same character is nitrate of mercury. Nitric acid moreover requires no special preparation; it does not spread like potassa c. calce, nor is its action so deep; it produces little or no pain and no hemorrhage. These advantages are trivial compared with the fact, that when once it has been properly applied, in many cases no further interference is necessary, and thus the frequent use of the speculum may be done away with. When the second examination is made, it should be after the lapse of a month, and it will then sometimes be found that there is a small spot requiring a fresh application of the acid, but often the sore appears to be quite healed or to be healing satisfactorily. The fresh mucous membrane which forms is not cicatricial in appearance, and when healing is going on satisfactorily, it has a sharply-defined edge, and being of a pale rose colour, contrasts strongly with the bright red of the sore. The contraction is greater than follows the use of any other caustic; but this is a great advantage, for on account of the relaxed state of the tissues, it is just what is required to insure the permanence of the cure.

The acid is best applied by means of a small and tightly rolled piece of cotton wool, which is to be placed by an ordinary speculum forceps in contact with successive portions of the surface until the whole is covered with a white eschar. In



a case of chronic endo-cervicitis, the acid should be applied to the interior of the open cervical canal, and if it is not open the case is not one suitable for the treatment. The contraction which accompanies healing is only to a healthy and natural degree. Provided the caustic has been used with ordinary prudence, I have never seen anything but good follow its use, and the ease with which a chronic case of cervical catarrh, with ulceration or erosion, may be cured by it is something marvellous. The bulk of my cases have been hospital out-patients, and the comfort the use of nitric acid has been in their treatment is very great, both in certainty of result and in saving my own time. Without local treatment very little can be done for these patients, for hygienic treatment is generally impossible, and medicinal treatment alone is useless. I shall not take up your space by details of cases, although I have copious notes of about forty. By trial of the remedy a proper estimate of its value will soon be formed.

The use of nitric acid as a caustic is so familiar to us all, especially in the treatment of some diseases of the rectum, that I had some hesitation in bringing the subject before you, and should not have done so but that I believe it is only used by two or three medical men engaged in the treatment of diseases of women. It is mentioned incidentally at the conclusion of a paper by Dr. Lombe Atthill, upon its application to the interior of the uterine cavity, that he uses it habitually in the diseases in question; and Dr. Roe of Dublin, in an analysis of 164 cases of uterine disease,\* relates a case of extensive ulceration in which he employed it. Mr. Robert Ellis has recommended the use of a saturated solution of nitrate of silver in nitric acid, and I believe Dr. Bennett has mentioned it also; but these writers are exceptions to the general rule. It is not mentioned by Tilt in his admirable work on uterine therapeutics, nor by any other of our standard authors upon diseases of women, all of whom recommend nitrate of silver, or mention its use as the usual practice.

---

\* "Dublin Journal of Medicine and Science," August, 1872.

## Reports of Hospital Practice.

---

### GUY'S HOSPITAL.

*Case of Complete Occlusion of the Vagina and Retention of Menses, consequent upon an Abortion.—Relief by Operation.*

*Under the care of* DR. BRAXTON HICKS *and* DR. GALABIN.

Mrs. G., aged twenty-eight, aborted about the middle of the fourth month of her first pregnancy, in October, 1874. No interference was necessary to remove the ovum. She was confined to her bed for many weeks after the abortion, and suffered much abdominal pain. When convalescent, she discovered for the first time that some obstruction had been produced in the vagina. No vaginal examination had been made by her attendant since the time of the abortion. In February, 1875, she began to suffer from spasmodic pain in the abdomen, no menstrual discharge having in the meantime appeared. In March she again had recourse to her medical attendant, who passed some instrument (apparently a metal catheter), by means of which a few small clots were liberated. This was repeated several times, but the pain was not relieved, and in June an attempt was made to restore the occluded canal by separating the tissues with blunt instruments and by the fingers. No anesthetic was given. On this occasion no menstrual fluid was liberated. About a week after the operation the feces began to pass by the vagina, and this continued up to the time of her admission.

She was admitted into Guy's Hospital on August 25th. The vagina was found to be completely occluded, and converted into a shallow pouch, at the bottom of which was a smooth opening into the rectum, admitting one finger very easily. For the last month the pain had come on only once a day. It was violent in character, and lasted for six or seven hours.

On August 30th Dr. Braxton Hicks commenced the

operation for the restoration of the vagina, no anesthetic being given. The cicatricial tissue was carefully torn apart with a blunt knife by the aid of the finger. Some rather considerable bleeding occurred, and when some advance had been gained the operation was, for the time, suspended, oiled lint being placed in the wound. The patient continued to have paroxysmal pain each day, but it was subdued for the time by the injection of  $\frac{1}{3}$  grain of acetate of morphia subcutaneously.

On September 27th, in the absence of Dr. Braxton Hicks, the operation was resumed by Dr. Galabin. The vagina at this time was still a *cul de sac*, barely an inch and a half deep, bounded by very dense cicatricial tissue. Chloroform was given upon this occasion. The fundus uteri could then be felt, somewhat retroverted, but reaching as high as the umbilicus. On being tilted forward, it could be seen to contract powerfully from time to time. A transverse incision was made through the superficial cicatrix, and a passage was then gradually torn by a blunt instrument, a catheter being kept in the urethra. A large cavity was at length reached, having smooth walls. It appeared to consist in the main of the distended uterus, but no os or cervix could be detected. The opening was enlarged to a size admitting two fingers, and about fifteen ounces of dark, thick treacly fluid (not offensive) were expelled. A pad and bandage were then applied. The same fluid continued to flow away in some quantity until about midnight, but ceased from that time. The evening temperature was  $100^{\circ}$ , and pulse 100, but the patient suffered no inconvenience, and from the following day pulse and temperature were normal. The next morning the cavity was injected with a weak solution of permanganate of potash, which brought away much grumous blood. This injection was repeated twice a day.

The opening which had been made showed a great tendency to contract, and on October 2nd its dilatation was commenced by the smallest sized Barnes's dilating bag. This was introduced for a few hours each day, and on the 4th was replaced by the medium sized bag. On the 8th the canal was so far dilated that the bag could with difficulty

be retained. The upper part of the cavity within had now much contracted, and its fundus could just be reached by the examining finger. At a distance of  $1\frac{1}{4}$  inch below the fundus was felt a firm ring of apparently muscular tissue, the length of the whole cavity being  $2\frac{3}{4}$  inches. Its lower part showed little tendency to contract, and was made up partly of a pouch extending backward and to the right. Nothing like an external os or cervix uteri could be detected.

The patient still continues nearly in this condition, and the recto-vaginal fistula remains for further operative treatment.

*Remarks.*—The operation in this case presented some difficulty on account of the close adhesion which had been formed at the lower part of the cicatrix, between the rectum and the bladder, so that scarcely more than a quarter of an inch of dense tissue appeared to intervene between the rectum and a catheter in the urethra. On this account the recto-vaginal fistula had unfortunately resulted from the first attempt at operation before the patient's admission into the hospital. It was also impossible to discover the position of the cervix uteri, and this would seem indeed to have been entirely destroyed or blended with cicatricial tissue. The shape and the behaviour of the lower part of the distended cavity appeared to show that it was not composed of uterine tissue, and the firm ring afterwards felt at a distance of  $1\frac{1}{4}$  inch from the fundus seemed to correspond to the internal os uteri. The cavity was, therefore, probably made up partly of the distended body of the uterus, and partly of a remnant of the upper part of the vagina.

---



---

# THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

NOVEMBER, 1875.

---

## “QUICK WITH CHILD.”

How many years has not this legal phrase furnished a text for the contempt and ridicule of obstetric and forensic writers? Why should everything in this country relating to State obstetric medicine remain in such a barbaric and disgraceful condition—our midwives uncared-for, and the laws relating to pregnancy in a state incongruous and utterly at variance with science and common sense? Again and again these gross and discreditable inconsistencies have been laid bare, and once more they have been exhibited by Dr. Barnes in his introductory address at St. George's Hospital. It is to be feared, however, that the constant exposure of these legal absurdities to medical audiences will have no remedial effect. A better chance of success might be obtained by the Obstetrical Societies taking up the subject and appointing a committee to report upon it and point out the necessary reforms. It is not likely that Government will spontaneously move in the matter, but if pressed by an authoritative body it might be induced to consider and amend the despicable and defective laws which relate to the pregnant condition. The first question to be decided is—When ought the living entity, whether it be impregnated ovum, embryo, or fetus, to be taken under State protection? and the second is—Who shall decide its existence, or period of existence, in the womb? Some have maintained that the fetus before birth is nothing more than a human parasite, or higher species of intestinal worm devoid of soul, mind, or human attributes, and having no special claims for consideration or protection. The vulgar and antiquated notion is, that the embryo acquires life at the period of quickening; that is, when a woman first chances

to become cognisant of its movements. The scientific and generally received belief by all educated persons now is, that the life of the embryo commences at the moment of conception, and that from this period to the time of its fullest fetal development, it is a being demanding most careful protection. The law is very contradictory on this point. It punishes severely any one who attempts to procure abortion, whether the woman upon whom the attempt is made has or has not quickened, or is or is not pregnant; and yet in the case of a woman who has committed murder capital punishment is not delayed, although she may be far advanced in gestation, if twelve ignorant matrons upon examining her can be found to declare that she has not already quickened. It arrogates to itself therefore the right to destroy a life and commit an act, which if done by any other body or person would be criminal. In cases of property it, on the other hand, takes the embryo under its protection from the earliest date of conception. "It is a strange anomaly," says a late writer on medical jurisprudence, "that by the law of real property, an infant *en ventre sa mère* may take an estate from the moment of its conception, and yet be hanged four months afterwards for the crime of its mother." There can be little doubt that the doctrine of quickening in relation to the capital punishment of pregnant females ought to be abandoned, and the bare proof of pregnancy adopted according to the law of France, as a plea in bar of execution. The number of instances in which the jury of matrons have proved incorrect in their diagnosis, and have had their decisions reversed by medical experts, proves how absurd is the present method of determining the existence of pregnancy. The Americans are in advance of us in legislation on this subject. When pregnancy is pleaded in bar of execution, it is enacted that the sheriff shall summon a jury of six physicians to determine the point. Some such plan as this ought to be adopted in our own country. The existing mode is out of harmony with scientific progress, unworthy of a nation which calls itself civilized, and ought, with the other preposterous enactments which environ this subject, to be speedily abolished.

## Abstracts of Societies' Proceedings.

---

### OBSTETRICAL SOCIETY OF LONDON.

*Meeting, October 6th, 1875.*

WILLIAM S. PLAYFAIR, M.D., *Vice-President, in the Chair.*

Edward Chaffers, F.R.C.S. (Keighley), and Amos Rogers, L.R.C.P. Ed., were elected Fellows of the Society.

Before entering upon the ordinary business of the evening, Dr. PLAYFAIR alluded to the loss sustained by the Society by the death of its Honorary President, the late Sir Charles Locock, who had rendered valuable aid to the Society when it was first formed, by accepting the Presidency, and taking a special interest in furthering the aims of the Society.

He announced that the unanimous feeling of the Council was in favour of asking Dr. Arthur Farre to accept the vacant post, which was received with acclamation by the Fellows present.

Dr. GERVIS exhibited a pessary removed from a patient, aged fifty-six. Fifteen years previously it had been introduced into the vagina to relieve prolapsus of the womb. Four years ago she began to feel a weakness in the lower back; two years ago her legs became weak, and she was unable to walk without assistance; for six months she has been nearly bedridden. For the last six months she has had discharge from the vagina—occasionally tinged with blood, and highly offensive. She can neither stand nor walk, and is able to move her legs in bed with difficulty—being attacked two or three times during the night with cramp in them. On examination, a large, round metallic pessary was found imbedded in the vaginal walls, and was removed by the aid of bone forceps, a considerable quantity of brownish purulent discharge, horribly offensive, escaping. All the paralytic symptoms were relieved after its removal.

Dr. CLEVELAND remarked that apart from the interest attaching to the long impaction of a foreign body in the vagina, there was the notable fact that a quantity of highly offensive putrid matter must have been locked up for a considerable time without the supervention of septicemia. The case tended to show, in connexion with the recent debate before the Society, the necessity for the co-existence of some peculiar condition of the general system before absorption of morbid matter, followed by blood poisoning, could be effected.

Dr. BANTOCK had met with a similar case to that given by Dr. Gervis—the pessary being oval in shape, and composed of horse-hair, covered by india-rubber.

Dr. ROGERS referred to a case he had before mentioned to the Society, of a black boxwood pessary, which had been left in eighteen years and had been forgotten by the patient.

Dr. EDIS also alluded to a similar case he had brought before the Society, where a large oval pessary, the size of a goose's egg, had been left in the vagina for eleven years. The case having been supposed to be cancer.

Dr. PLAYFAIR thought these archaic instruments would soon be out of use, most of us had seen them. He wondered why the ulceration had not extended into the bladder and rectum. As regards Dr. Cleveland's remark, he (Dr. Playfair) had noticed that fetid matter of intense virulence might be present, provided there was an absence of an absorptive surface of a freshly-formed wound.

Dr. GERVIS, in reply, stated that in removing the pessary a slight laceration took place, the fetid fluid passed over it, and yet there was no septicemia.

Dr. HAYES exhibited the placenta and still-born fetus from a patient, aged twenty-two, affected with syphilis. The placenta was pallid, soft, and friable; the villi packed with fat globules. The fetus appeared healthy. There were two questions involved in explanation of the death of the fetus, whether it resulted from syphilis or from fatty degeneration of the placenta.

A Report by Drs. Aveling and Hayes on Mr. Wallace's case of monstrosity was read, as also on a specimen exhibited at a previous meeting.

A case of *complete Inversion of the Uterus*, occurring immediately after labour was communicated by Mr. FREDK. H. GERVIS. The patient, aged forty-one, was delivered of her sixth child, after a slow and tedious labour. The placenta was adherent. After waiting twenty minutes it was felt projecting through the os, and was gently drawn down, when the uterus was found to be inverted. The placenta was immediately peeled off. The hemorrhage was slight, but on looking up the patient was found to be in a state of syncope, and apparently dying. Brandy and ammonia were administered, and, after the patient had rallied somewhat, the inversion was reduced—counter-pressure being applied externally to the cervical ring. Galvanism was applied to contract the uterus after its return. The patient recovered perfectly.

Dr. GALTON remarked that the mode of reduction mentioned by the author, although successful in this case was one hardly to be recommended in general. The author described the reduction as effected by pressing two or three fingers up into the fundus of the uterus. This plan would necessitate the passage of four folds of uterine tissue through the os, instead of two folds, which would alone require to be passed through were the reduction accomplished by the more mechanically correct and usually recommended method of grasping the whole body and returning first the part latest in coming down.

Dr. GRIGG inquired whether there were any severe cramps and pains in the thighs as well as shock. He remembered a case in his



own practice where the uterus came down with the greatest ease, without any traction having been exerted.

Dr. BANTOCK thought the treatment was confirmed by the result. He did not agree with the preceding speaker, Dr. Galton, in thinking that the uterus was doubled up by pressing on the fundus, for we were told that the body of the uterus felt hard. It appeared to him that the reduction began at the cervix.

Mr. GERVIS explained that the uterus was exceedingly rigid and hard, and it was difficult to get a fold of the uterus. Whether the fundus or the cervix went up first he did not know. In a previous confinement she had had severe cramps.

Dr. CLEVELAND thought the whole question of inversion of the uterus of such an important character that he hoped he might be excused for inquiring more minutely as to the amount of traction on the cord that had been used. He could conceive that owing to the administration of ergot before the expulsion of the placenta, especially if it happened to be adherent, the uterus might take on that irregular and spasmodic action which, with only moderate traction on the cord, might eventuate in inversion. He did not, however, for a moment doubt that Mr. Gervis would satisfactorily answer his question.

Dr. GERVIS remarked with reference to Dr. Cleveland's suggestion as to the production of inversion, that so accurate an observer as Dr. Tyler Smith had in one instance seen the uterus invert itself, and it was now generally allowed that this accident could arise quite independently of traction. He would like to ask whether any Fellow present had ever seen the inverted uterus re-invert itself. He had read recently of such a case, but imagined it must be at all events an occurrence of extreme rarity.

Dr. HAYES observed that when we consider what difficulty sometimes occurs in getting the placenta away, the cord giving way first, it was scarcely probable that Mr. Gervis could have exerted sufficient traction to produce the inversion.

Dr. AVELING remarked that inversion may be caused by *vis a tergo*, and even post-mortem from development of gas in the abdomen.

Dr. GRIGG mentioned a case, occurring five days after labour, where no ergot had been given.

Dr. ROGERS had only seen one case, which he replaced by means of an air pessary. He thought Mr. Gervis's plan was the best.

Dr. GERVIS referred to a case reported in one of the early volumes of the "Transactions," where the uterus had re-inverted itself.

Dr. WILTSHIRE admired the courage and candour of Mr. Gervis in bringing forward his case. After fifteen years' experience in practice he would scarcely use immoderate traction; still, the process of inversion might have been commenced before. It was a question whether it was not better to peel off, and not to pull. We ought to be much indebted to Mr. Gervis for bringing the case forward.

Dr. GALABIN read a paper on the occurrence in normal labour of

lateral obliquity of the fetal head. After entering into the views formerly accepted, that there is normally a lateral obliquity of the fetal head to the plane of the pelvic brim, he asserted his belief that the conclusion indicated by theory is confirmed by observation—namely, that in easy labours there is no perceptible lateral obliquity; but that if the head meets with considerable resistance, an obliquity is produced, in most cases of such a kind that the right parietal bone lies deepest in the plane of the brim.

Dr. PLAYFAIR thought the Society was very much indebted to Dr. Galabin for his erudite paper. The point at issue was one of great interest, and he had failed to satisfy himself whether obliquity really existed or not, or whether Dr. Galabin's suggestions were sufficient. He (Dr. Galabin) had neglected to mention a very elaborate treatise on the subject by Dr. Hodge, who had given much attention to the subject.

Dr. GERVIS quite concurred in all the President had said as to the value of Dr. Galabin's paper. It was, however, difficult to discuss such a paper without reading as well as listening to it. It would appear to favour the opinion that there was no necessary contradiction between the two opposite views, but that obliquity of the head, depending upon some undue pelvic resistance, was essentially a character of a labour in some degree abnormal, while in perfectly normal labour the equilibrium between the propulsive force and the resistance of the parturient passage being, as Dr. Galabin expressed it, stable, obliquity did not occur. If further research proved this view accurate, it would account for much of the present dissonance of opinion.

Dr. GALABIN thanked the Society for their patient attention. The subject was more interesting with a fetal skull and a pelvis than in describing dry mechanical details.

Dr. J. BRAXTON HICKS read a Paper on a dissection of a uterus pregnant about three and a half months, the placenta being *prævia*, and fibroids extensively developed in the uterine walls. Attention was directed to three points,—1st. Whether there was a circular sinus at the margin of the placenta; 2nd, The distribution of the curling arteries in the decidua serotina; 3rd, Whether there was any change induced by pregnancy in the fibroma. The first was doubtful, and is a point yet to be carefully examined. As regards the second, the decidual processes, consequently the separation into lobules, commence later than the third and a half month; and 2. There is no proof that any branch from the supplying vessels of the serotina pass into the interstitial space. As to the third point,—the fibres were about four times larger than in the ordinary fibroma, very much more distinct and separable from one another. A few had attained the size of the colossal fibre of the ordinary pregnant uterus.

Dr. EDIS observed that if the fibres in the fibroma increased during pregnancy there was no reason why they should not equally be involved in the process of involution following parturition, the same as with the ordinary uterine fibres.

## OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, April 14th, 1875.*Dr. J. MATTHEWS DUNCAN, *President, in the Chair.**Report of, and Observations upon, a Case of Puerperal Tetanus.*

By ANGUS MACDONALD, F.R.S.E.

About ten A.M. of the 9th of March, 1875, I was hurriedly summoned by Mr. Bentley, one of my pupils at the New Town Dispensary, to see a patient to whom he had been that morning called, and who, he stated in the note, seemed to suffer from puerperal convulsions. The patient's name was Mrs. Louttet, her age twenty-four, and her residence Ironside Close, 85, Abbeyhill.

She had been married for three years, and had had two children. The eldest, a healthy girl, was born about one year after marriage, and is still alive and well. This child, however, was suckled by the patient during the whole period of her second pregnancy, and even up to the commencement of her labour.

The second confinement took place on the 26th day of February, 1875, at 3.50 A.M., when she was delivered by Mr. Bentley of a male child, after an easy labour of a few hours' duration. All the stages were natural and normal. The placenta was removed about fifteen minutes after the birth of the child, with little or no effort on the part of the attendant.

Judging from the appearance of the second child, as also from the expectations of the mother in regard to the period of her confinement, the labour would appear to have supervened about three weeks or so before the completion of the full term of utero-gestation. Except some slight appearances of prematurity, however, such as deep colour of skin, slightly defectively-developed nails, and rather more than ordinary lanugo on the shoulders, the child was healthy and well formed.

The patient had continued to progress favourably, exposing herself to no special risks, from getting up too early or otherwise, and had looked after the proper action of her bowels by taking twice castor-oil, which had acted well on both occasions, until about 2 A.M. of the 9th of March, when she was noticed to be sitting up in bed, bent forwards. It is to be noticed that on the previous day she had been in her usual health, going about her duties, and making no complaints. When she sat up at this time she complained of no pain, and in fact never spoke, but appeared very restless, rubbing her legs and clutching at her hair.

It is impossible to obtain any exact information from the attendants as regards the first onset of the tetanic spasms, but it is clear that there was heavy breathing from about 6.30 A.M.

On Mr. Bentley's arrival at 9 A.M., the patient was completely unconscious, and there were present strong tetanic spasms, similar to those

described below, only more violent in character. The perspiration stood out in beads on her head and face.

The patient's condition on my arrival was as follows :—

Face pale; general appearance anemic; skin soft and dry, but feels as to temperature natural; pupils somewhat contracted, and almost completely insensible to light; *conjunctivæ* insensible to touch; eyes glazed-looking. Patient completely unconscious both during and between the attacks of spasm. Urine expelled involuntarily. Spasms of a truly tetanic character recur at intervals of about a minute. During these seizures the heels and the occiput are the only parts of the patient's body which touch the bed, the contractions being thus mainly of an opisthotonic character.

During each seizure the muscles of the back are seen to be strongly contracted. The upper extremities are strongly flexed, the fingers being bent forcibly towards the palm, and the thumb turned inwards, and the forearm powerfully flexed over the upper arm. If any predominance of contraction on one side of the body over the other exists, it seems to be on the right; but it is slight, if it exists at all, and seems rather to be an appearance due to the patient lying slightly over in bed towards the left side.

The slightest irritation applied to the skin, such as the touch of a cold hand, is sufficient to throw the patient into complete opisthotonos, which attacks are also induced by every effort at examining the patient's condition.

The muscles of the neck are powerfully contracted during the attacks, more particularly the sterno-cleido-mastoid; but there is no risus sardonicus, and it is only during the more severe attacks that trismus occurs. Every  $1\frac{1}{2}$  minute or so, an attack occurs, and every fourth or fifth attack is much more severe than those intermediate. There is no congestion of the face during the spasms. Though the opisthotonos can be induced at any time by application of external stimuli, the attacks recur at regular intervals of about  $1\frac{1}{2}$  minute spontaneously.

Pulse 74, weak, soft, compressible, and somewhat irregular. Respirations, during intervals between the fits, shallow and quick, being 26 per minute, but during the attacks they become stertorous. Temperature per vaginam,  $101^{\circ}$ .

Attempts to get the patient to swallow induce violent fits, but prove wholly abortive.

On examination per vaginam and per hypogastrium, uterus and vagina seemed perfectly normal; no ulcer or sore to be found. The cervix uteri felt soft, still large and patulous, and from its external orifice some mixed sanguineous discharge soiled the examining finger. The uterus, both as to size and position, seemed perfectly normal, being slightly anteverted, as Credé\* has shown it should normally be

---

\* "Archiv für Gynäkologie," Bd i. s. 96.



about this period. No unnatural fulness or resistance in the pelvis to be recognised. No scar or wound visible on extremities or trunk, except some eczema pediculare of scalp.

A catheter was now passed, and about a tablespoonful of water extracted from the bladder.

This, on cooling, exhibits a profuse deposit of urates, is of acid reaction, but on heating with nitric acid does not give the slightest trace of albumen.

The following treatment was then ordered :—

R Hydratis chloralis . . . ʒiij.  
Aquæ . . . . . ʒiij. Solve.

Sig. A tablespoonful to be mixed with a little warm water and injected into the rectum.

At 11.30 A.M.—The spasms now recur every minute, and last about two seconds, and are accompanied with a stertorous breath or two. First dose of chloral injected, and apparently retained. Some slight amount of feces found in rectum. The act of injection set up a fit of unusual severity, accompanied with grinding of the teeth.

12.30 P.M.—Pulse 76 per minute ; respirations  $24\frac{1}{2}$ . Character of pulse and respirations same as before. Spasms same in character, but recurring fully more frequently.

Another half-drachm of chloral injected and retained.

1.10 P.M.—Eyes have lost their glazed appearance ; pupils rather more dilated ; tongue clean and moist ; lips, however, are dry.

1.20 P.M.—Breathing more stertorous and laboured. Inspiration accompanied with a loud gurgling sound, air entering the lungs very imperfectly. Occasionally a deep sighing inspiration occurs, without stertor or difficulty. Spasms occur less frequently.

Pupils now well dilated, right more so than the left. Pulse 88, slightly irregular, but not so soft as before, being now rather bounding in character.

1.45 P.M.—No spasms have occurred for twenty-five minutes. Inspiration now accompanied with a crowing sound—stridulous.

2.10 P.M.—Breathing not so stertorous ; face flushed in patches ; no fits.

2.12 P.M.—Face very livid ; respiration ceased ; pulse 120, very soft and small ; body and extremities moist and warm ; pupils dilated as before.

2.13 P.M.—Pulse much slower, and regular, 72 per minute.

2.15 P.M.—Pulse at wrist, as also heart's impulse, imperceptible. Patient dead.

For these exceedingly minute and accurately detailed notes I have to express my obligations to Mr. Bentley and Dr. John Playfair, both of whom never left the patient's bedside from the time of our first arrival till she died.

After some difficulty we succeeded in persuading the friends to

allow of a post-mortem examination. This was performed on the 11th by Dr. John Wyllie, with the following results :—

*Post-mortem Examination, Forty-six Hours after Death.*

*Brain.*—On opening the skull, about two ounces of serous fluid escaped from the lateral ventricles. Cerebral convolutions were flattened, and some venous congestion of the cerebral surface present. Both lateral ventricles were filled with dark blood-clots, which passed forwards into the anterior cornua, and backwards into the posterior cornua. The amount of clot seemed to be about half an ounce in each ventricle, and there was some laceration of the ventricular walls. In each corpus striatum there were a vast number of small extravasations, varying in size from that of a millet-seed to that of a pea. This condition was found to extend throughout the entire tissue of both corpora striata. The third ventricle also was filled with blood-clot, which again was found to extend along the iter to the fourth ventricle, and hence to the inferior sulcus between the two lateral lobes of the cerebellum.

As a result of the hemorrhages into their tissue, both corpora striata were torn up and softened.

The venæ Galenæ and the straight sinus were found occupied by a large, dense, firm, and adherent thrombus. This condition extended also into the left lateral sinus, but in its cavity the clot was not so firm. The blood in the right lateral sinus was fluid.

*Heart.*—Right ventricle and pulmonary artery contracted. Some partially decolorized and rather tough blood-clot in them; but this clot was not adherent. Left ventricle empty and healthy. *Liver* healthy. *Lungs* not examined.

*Uterus.*—Cervix somewhat congested. Mucous membrane throughout moistened with a slightly grumous or sero-sanguineous fluid. The placental area still could be made out from its distinctly darker appearance than that of the rest of the mucous aspect. The muscular and peritoneal coats seemed perfectly healthy. The mucous membrane seemed abnormally thin, but otherwise healthy.

A rather noisy assemblage in the adjoining room now became so specially demonstrative, that we had to desist from further examination of the body.

We managed, however, to carry off the uterus for further examination.

There are so many points of special interest in regard to this case, that I have deemed it of sufficient importance to bring it before the Society, trusting that its relation and discussion may prove interesting and instructive.

In the outset, I wish to state that, though I have called this case one of puerperal tetanus, I do not regard it to be a case of ordinary traumatic tetanus, but really one of tetaniform convulsions, arising

from apoplectiform congestions and extravasations. But in naming it as I have done, I have acted not inadvertently, but advisedly, as I have been led to believe that the disease known as puerperal tetanus may be better explained in this manner than by regarding it as essentially the same as traumatic tetanus.

Into the question as between the theory which maintains a purely nervous origin for tetanus, and that which refers its causation to inflammatory or at least congestive changes, I do not at present enter at length, but mean to refer to it further on in the paper. I therefore pass on at once to the consideration of the more salient features of this peculiar case.

While, in some particulars, the case presented phenomena which suggested a close relation between it and ordinary eclampsia, there were a large number of uncommon symptoms, which struck me as differentiating it very sharply from an ordinary case of eclampsia.

I also think that the President, whom I asked to see the case with me, will bear me out in this statement of opinion. The chief distinguishing points were—

1st. The spasms were distinctly *tonic* and not clonic, or mixedly tonic and clonic, as we find in cases of eclampsia.

2nd. The pupils were somewhat *contracted* and *fixed*, instead of *dilated and fixed*, as is the ordinary condition in deep eclampsia.

3rd. The face continued pale during the seizures, instead of becoming congested, as in eclampsia.

4th. The contractions were very markedly more powerful in the posterior aspect of the body, so as to establish a condition of more or less continuous and complete opisthotonos.

5th. The rate of recurrence of the spasms was much more rapid than in any case of eclampsia that I have witnessed, and my experience refers to a large number of cases.

6th. The spasms were capable of being excited and intensified by the slightest external stimulus or irritation, such as the touch of the cold hand, or examining any part of the body by tactile manipulation, which is not the case in eclampsia.

7th. There was no albuminuria present, which we generally, though not invariably, find in cases of puerperal eclampsia.

On the other hand, the suddenness of the first onset of the attack, the stertorous breathing during the individual seizures, the complete unconsciousness of the patient, and her general aspect, with the above-mentioned exceptions, forcibly reminded one of eclampsia. Still, however, the great preponderance of symptoms tend to prove the existence of a tetanic rather than an eclamptic condition.

This being so, we are naturally led to attempt to solve the question, What were the predisposing, and what the proximate, cause of these alarming symptoms and conditions, which collectively formed the phenomena of the case, and resulting from which the death of the patient so speedily followed?

I hesitate not to state, that I firmly believe that that peculiar con-

dition of the blood (septic?), allowed on all hands to exist during the puerperal period, must have been the main predisposing element. The patient, it must be borne in mind, had, in addition to this, been weakened by prolonged lactation, and was consequently in feeble health at the time when her confinement supervened.

A *special*ly vitiated condition of blood in this particular case is further rendered exceedingly probable, by the fact that the mucous membrane of the uterus was imperfectly developed. The ordinary attempts at reproduction of it had manifestly been unduly arrested, as was proved by carefully conducted microscopical examination, whilst the placental area was of an abnormally dark hue. These are precisely the conditions under which we find the formation of venous thrombi; and, as a matter of fact, there had been slowly forming for some days, in consequence of them, thrombus in the venæ Galenæ, in the straight sinus, and in the left lateral sinus.

Into the venæ Galenæ open the venæ corporis striati, the venæ choroideæ, as also some cerebellar veins, whilst, again, the venæ Galenæ pour their contents into the straight sinus, there commingling with the current of blood which passes through the inferior longitudinal sinus, and with the contents of the inferior median and of the superior cerebellar veins.

Those portions of the brain draining into the straight sinus, whether directly or indirectly, must of necessity have been thrown into a condition of intense venous congestion so soon as the impermeability of the straight sinus was established.

This congestion, it appears to me, was the proximate cause of the tetanic convulsions. Such a case as this, therefore, I hold, in its essential features, differs little, if at all, from a case of phlegmasia dolens, or of pulmonary thrombus, except in this particular, that here the thrombus occurred in a portion of the body closely related to the centres of organic life, and the delicacy of whose anatomical arrangements permitted less free deviation from the ordinary boundary of healthy action without leading to consequences immediately disastrous.

Consequently, whilst thrombus of the femoral tends to the establishment of the comparatively innocuous swelled leg, and thrombus of the lung, or of any other organ, leads to results more or less injurious according to the importance and amount of the tissue concerned, thrombus of the venæ Galenæ and straight sinus leads to convulsions of a tetanic character, accompanied, in this case, by destruction of the tissue of the corpora striata, apoplexy into the cerebral ventricles, and followed by speedy death.

Let us now consider in what manner this congestion led to the tetanus.

The investigations of experimental physiologists, and more particularly and lately those of Ferrier and M'Kendrick, tend to establish the belief that the corpora striata and the corpora quadrigemina are the great motor centres for the whole body. These views are also



supported by the experiments of Tenner and Kussmaul, if I rightly interpret the result of their experiments. For it will be noticed that, when they made their sections very deep, so as to interfere with the structure of the corpora striata and the corpora quadrigemina, convulsions of a tetaniform nature resulted from the irritation, whilst the conditions capable of inducing epileptic convulsions on compressing the carotids were found to be more or less entirely lost, *i e.*, as I read it, the motor centres had been first irritated, and then more or less entirely destroyed by the previous sections.\*

Whilst irritation of these great motor centres, as a whole, induces general tetanic spasms, it has been further shown by Ferrier that irritation of the corpora quadrigemina is followed by opisthotonos. We have, therefore, in the condition of venous congestion of these great motor centres, a sufficient irritative stimulus to account not only for the opisthotonos, but also for the existence of general convulsions of a tetanic nature. Why, therefore, it may be asked, did the opisthotonos so decidedly preponderate?

In the first place, I would take leave to notice that the patient was not seen by Mr. Bentley until after she had suffered from the attacks for several hours—in fact, for five or six hours; and we have, in consequence, no means of determining whether the seizures were not of a more general character at first, and even whether they were not eclamptic.

In the second place, after the attack had lasted for some hours, and the extravasations into the ventricles and into the tissue of the corpora striata had advanced so far as to lead to pressure upon and destruction of the tissue of these ganglia, their functional activity must necessarily have failed, and, instead of exalted motor activity in the muscles regulated by them, arrest of such action must have resulted.

Up to the last, however, the corpora quadrigemina were in a different condition. Though the great mass of blood supplied to them drains into the straight sinus, and though, consequently, the same condition which congested and broke up the corpora striata led to irritative congestion of the former ganglia, yet, whether owing to the vessels in relation to them being of stronger tension, or for some other reason, the congestion had not in their case led to extravasation, and consequently we had exaltation of their special function in the form of opisthotonos.

There is some difficulty also in accounting for the sudden onset of the seizures, as from the amount and the firmness of the venous thrombus we cannot doubt but it must have existed for some days previously to the commencement of the fits. But I think that this is rationally and satisfactorily accounted for by the supposition that only immediately before the attack did the venous sinuses become

---

\* Kussmaul and Tenner on "Convulsions after Hemorrhage," experiments 7, 8, 9, and 10, pp. 77, 78.

absolutely closed, having previously admitted an amount of blood to pass sufficient to prevent the occurrence of that degree of congestion capable of determining in convulsions.

And now to consider the general question of puerperal tetanus, and more especially its relation to septicemia. I regard this case as one specially valuable in its bearings upon this point.

That partial tetanus may result from congestion and inflammatory injuries of the cerebral ganglia, and also the upper part of spinal cord, as well as from other organic lesions of these parts, is manifest ; and further, such a result is allowed for in the cases referred to by Tenner and Kussmaul, pp. 96-98, and by Curling, pp. 52 *et seq.*, as also in his Synopsis, at page 122. The theory that tetanus is *invariably* consequent upon some inflammatory lesion of the spinal cord or of its investing membranes, has been always maintained by a certain class of surgical pathologists, and notably by Baron Larrey, whose experience of such cases was certainly very great.

Now, though Curling maintains, and I think rightly so, that lesions of an inflammatory kind cannot be made out in ordinary cases of traumatic tetanus, I conceive that he errs in dwelling too exclusively upon the idea of actual inflammation being necessary to be proved in such cases in order to show that a cause external to the nervous tissue itself produced the disease. He is, in my opinion, thereby led to put too much stress upon a peculiar irritable condition of the nervous system as the element essential to tetanus. After all, it must be remembered that such a condition is a pure hypothesis. If only we could account for tetanus on the ground of disturbed vascular supply to the motor centres within the cranium, we should have something much more tangible to deal with than any imaginary irritability of these centres.

I do not pretend to experience warranting me to pass an opinion of value in regard to ordinary traumatic tetanus ; but from all I can find out respecting it, I am inclined to believe that only a small proportion of the cases can, in the present state of our knowledge of the pathology of the disease, be referred to vascular changes. The great majority of them, indeed, would seem to me to warrant us in assuming that there exists in the nervous system itself, however this may have been induced, a special condition, which is the main occasion of the spasmodic seizures.

On the other hand, my study of the disease called puerperal tetanus leads me to quite an opposite conclusion regarding it. I do not think it is the same in nature as ordinary traumatic tetanus, but that it is more allied to eclampsia ; and a careful study of the recorded cases of it more and more convinces me that most, if not all, of them can be referred to disturbance of vascular supply through venous stasis in the great motor centres, or to some such cause acting similarly to the thrombus, which occurred in this case. In the case before us we have no evidence of any inflammatory action at work in the motor centres ; but we have the most manifest proof of intense venous

stasis, even leading to rupture of the vessels in the corpora striata and disruption of their structure.

Had in our case the vessels remained persistent, and had the extravasations of blood which led to destruction of the tissue of the corpora striata and the formation of apoplectic extravasations, with consequent pressure upon the cerebral lobes and unconsciousness, not taken place, and thus speedy death been averted, it seems to me that beyond reasonable doubt we should have had a case of tetanus which would have resembled much more closely the ordinary traumatic form of the disease. In that case we should have had life prolonged for several days, along with complete consciousness; for we have every reason to believe that the corpora striata and the corpora quadrigemina, the parts concerned in the consecutive congestion, are entirely beyond the centres of conscious existence.

Let us suppose, then, such a case thus progressing until the patient died, not of apoplexy, as in the present instance, but of exhaustion, and that no post-mortem had been, as in our case, granted, we should have been fairly entitled to have recorded it as an instance of true puerperal tetanus, instead of a case of apoplexy with tetaniform convulsions.

On looking over the cases in Sir James Y. Simpson's *Selected Obstetrical Works*—and he has made the best, nay, almost the only extant collection of such cases—I have been more and more struck with the aptness with which the supposition that venous congestion of the great motor tracts in the cranial portion of the spinal cord had been set up by some embolic obstruction, such as occurred in the present case, fits in as an appropriate explanation.

It is much to be regretted that the records of his cases, and particularly in regard to post-mortem appearances, are sadly deficient. But a careful scrutiny of the cases fairly entitles us to hold that exactly the same class of conditions are present which ordinarily are apt to lead to blood-poisoning and the formation of embolism or of thrombus. I would here take leave to remark that Heiberg throws in the authority of his name and of his experience on the side of the occurrence of embolism or thrombus being rather secondary to blood-poisoning than a primary symptom, and points out that we have erred in looking upon the embolism in cases of septicemia as a cause rather than as a result of the poisoned blood.\* But to return to our subject.

The cases recorded by Sir James Y. Simpson are in a great measure connected with abortion, where, owing to hemorrhage, plugging of the vagina had been required, or in which portions of the membranes had been retained, the patients being weak, and weakened with hemorrhage; or they are stated to arise in connexion with premature births, or delivery at full time during the puerperal

---

\* Hjalmar Heiberg, "Die Puerperalen und Pyämischen Processe," s. 3. Leipzig, 1873.



period, coincidently with the onset of feverish symptoms, and usually accompanied with suppression of the lochia. In this class of cases, also, it is noticeable that hemorrhage or other weakening cause is frequently stated to have been present. Cold is usually referred to in the brief records as being the exciting cause of the symptoms. This, however, seems to me to indicate rather the commencing rigor of the inflammatory mischief; and it appears to me that, at most, the cold may have given origin to an endometritis, a paraperimetritis, or some such ailment, from which the tetanic conditions ultimately, though indirectly, arose.

It is further worthy of notice, that the cases usually come on about that period when a phlegmasia dolens is most commonly found to become established—namely, about the tenth or fourteenth day after confinement.

In Dr. William Craig's case, reported to this Society on the 23rd of February, 1870, it is distinctly noted that there was no evidence of metritis nor of peritonitis. But then it is to me exceedingly interesting, as tending to establish the pathological view I have been advancing, to remark, that Dr. Craig states that the patient had on previous occasions suffered from extensive varix of the right leg, which burst some days previously to her confinement; and, besides this, that the patient had suffered from rather severe hemorrhage before the midwife in attendance sent for Dr. Craig to remove the placenta. Also, it is stated, that between then and the ninth day, when the patient began to suffer from symptoms of tetanus, there had existed a very suspiciously quick pulse.

I am not aware what may have been the experience of the Fellows of this Society, but mine has been such as to lead me to keep a sharp look-out in case phlegmasia dolens should supervene, whenever, without manifest cause of an uterine inflammatory nature, I have a quick pulse persisting for some time after labour, say eight to ten days, in a woman of a weak constitution, especially if she has been further exhausted by post-partum hemorrhage.

I may state that Karl Schroeder expresses himself as of opinion that puerperal tetanus must be referred to some septicemic influences.\* Whether these causes act, as I have been led from the study of this interesting case to indicate, is, I frankly grant, as yet extremely uncertain. All I mean to say in reference to this matter is, that the above commentary is a hurriedly prepared and confessedly imperfect attempt to account for the occurrence of the disease called puerperal tetanus, which I feel certain is a disease differing *in toto cælo* from ordinary traumatic tetanus. The more I read and ponder the scanty literature we possess in regard to this obscure subject, the more I feel convinced that no explanation similar to what is commonly given for the occurrence of traumatic

---

\* "Lehrbuch der Geburtshülfe," s. 657. Dritte Auflage. 1872.



tetanus will suit puerperal tetanus; and, on the whole, the predicating of a venous congestion of the cranial centres of motion by the formation of phlebitic thrombi, seems to be, viewing all the facts of the case, the most likely explanation of the phenomena observed.

*Discussion.*

Dr. STEPHENSON remarked, that the case seemed to him to be hardly one of *puerperal* tetanus, as the patient was up and going about, having apparently fully recovered from her confinement, and such an attack might have come on independently of the labour. As to the septic nature of puerperal blood, and the weakened condition of the patient, he did not see how they could account for the seizure, as tetanus is very rare, and were they sufficient to excite it, it would be more common. He was struck by the force of the argument, that these attacks are caused by congestion of the cephalic nerve centres, and thought they were more properly to be referred to these than to the spinal cord. In children, thrombus of the sinuses was not uncommon, as had been shown by Bouchut; he had met with such a case, and there were many recorded in the French journals under the name of eclampsia; these cases, if better observed, would very likely be really tetanic in character. He thought the connexion between these cases and the one in the paper helped to establish Dr. Macdonald's views.

Dr. SIMPSON looked upon the paper as one of great value, and thought Dr. Macdonald had quite proved the case to be tetanic, and not eclamptic in character. One essential point of distinction he thought had been overlooked—namely, that in the case just read, the slightest touch or irritation of any kind was sufficient to produce a violent spasm—a condition which is never found in eclampsia. As to causation, he could not look upon it as due to septic influences; there was no proof of there being any source of septic matter, nor any raw surface by which it could have entered. No collection of pus had been found.

Dr. MACDONALD explained that in using the word septic, he only referred to the general condition of women's blood which obtains after childbirth.

Dr. SIMPSON would like to have seen some reason why the thrombus should have occurred where it did; the thrombus having once occurred, it was easy to understand the congestion and stoppage of the blood in the vessels behind.

Dr. MATTHEWS DUNCAN wished to confirm Dr. Macdonald's view of the case. The great merit of the paper was in showing it to be really a case of puerperal tetanus. There could be no doubt that it was tetanic, and not eclamptic. He was struck by the ease with which the tetanic convulsions were produced in this case. The disease, however, seemed closely allied to eclampsia, and this view was supported by the post-mortem appearances—namely, meningeal

apoplexy, hemorrhage into the ventricles, and minute extravasations, which are similar to those found in many fatal cases of eclampsia. He had seen several such examinations. At the same time the thrombosis of the straight sinus was a peculiarity of this case, and he had neither seen nor read of such a condition being found in eclamptic cases. The congestion and hemorrhage were the most important features in the case. Dr. Macdonald's arguments showed very clearly the distinction between the two sets of symptoms.

Dr. MACDONALD, in reply, thanked the members who had spoken for the way in which the paper had been received. We owed very much to Ferrier for the way in which he had differentiated the physiology of the corpora quadrigemina, which in this case were intact, and had consequently been able to regulate the tetanic convulsions. He could not assign any cause for the plugging of the vessels.

---

*Meeting, May 12th, 1875.*

*The Development of Cysts in the Stroma of the Ovary.*

By Dr. FOULIS.

Dr. FOULIS showed that, in the normal Graafian follicles, and in the cysts of the diseased ovary, the epithelium was developed from the corpuscles of the connective tissue, of which the stroma consists. The stroma of the adult rabbit's and cat's ovary is very remarkable, in that it consists almost entirely of little fusiform fibre-cells, and it is an extremely important observation that, even in the adult ovary, the cells of the membrana granulosa may be actually traced in their development from those fibre-cells which lie immediately round the embedded eggs. Dr. Foulis has preparations of the human ovary from a woman about thirty years of age, in which the greater part of the stroma is converted into cells exactly similar to those which constitute the cells of the membrana granulosa. In the old cat's ovary, one may see the fibre-cells of the stroma very beautifully, and also trace the development of the membrana granulosa cells from these. On the surface of such an ovary the germ epithelium can still be seen, and on comparing the cells of the germ epithelium with the young developing cells of the membrana granulosa, we are at once struck with the great difference in their appearance; and it can be seen that the germ epithelium on the surface of the old cat's ovary has no possible connexion with the fibre-cells around the imbedded eggs, which give rise to the cells of the membrana granulosa. The same can be equally well seen in the adult rabbit's ovary. The observations, as illustrated in the old cat's ovary, appear to Dr. Foulis to afford the most conclusive proof that Waldeyer's view, that "the cells of the membrana granulosa are developed from the germ epithelium," is incorrect and untenable.

*Discussion.*

Dr. MACDONALD had listened with much pleasure to the paper. The preparations exhibited were very convincing of the truth of Dr. Foulis's physiological theory, and, in his opinion, he had proved his point; for they certainly seemed to show that the cells of the membrana granulosa are formed of connective tissue corpuscles, and are of different origin from the ova. His only difficulty was, that in some of the preparations of the very early ovary, he thought he had seen minute ova, with elongated cylindrical-shaped cells round them.

Dr. SIMPSON agreed with Dr. Macdonald as to the value of the paper, but thought there was not much room for discussion. He felt like a learner listening to one who had made himself a master of the subject. Dr. Foulis's demonstrations were very convincing of the fact, and he believed it to be a new one in physiology, that connective tissue cells can be developed into structures so different from them as the cells of the membrana granulosa. He would like to ask Dr. Foulis how he distinguished between the cells of the membrana granulosa and those of the germ epithelium on the exterior of the ovary; they seemed very much alike, and Waldeyer would say they are identical. Turning to the pathological part of the paper, it was certainly a very interesting fact that these connective tissue cells can take on the growth and development described.

Dr. KEILLER expressed his admiration of Dr. Foulis's paper; it carried him back to the days when he heard his old friend and fellow student Dr. Martin Barry read a paper on the same subject. He thought he had made a great step in advance at that time, and now Dr. Foulis appeared to have gone a step farther. He took it for granted that Dr. Foulis must be right in the main, from the fact that Dr. Allen Thomson, an old and excellent authority on the subject, had expressed at the Royal Society his belief in the correctness of Dr. Foulis's views.

Dr. MATTHEWS DUNCAN hoped that Dr. Foulis's work would meet with an earlier recognition than Dr. Martin Barry's had done, for it was not till after his death that the value of his work was fully recognised. The paper entered into three important subjects:—(1.) The development of the ovum: in this Dr. Foulis had gone over the same ground as Waldeyer, but had extended and improved upon the observations of that writer; and here it was he had shown most originality, and certainly his views appeared to be the true ones. Closely connected with this was (2) The development of epithelium from connective tissue, a point on which there was the greatest disagreement among observers. The side taken by Dr. Foulis was quite evident; he did not believe in the old notion of all the organs of the body being developed from three primordial layers of the embryo—and indeed these views had been quite overthrown by the observations of Dr. Thin and others. The third point was the history of the production of cysts; and here he supposed that when Dr. Foulis

spoke of cysts as being developed from collections of connective tissue corpuscles, he referred only to the cysts of a multilocular cystoma. Here too his views were original, but not so much so as in other points.

Dr. FOULIS, in reply, expressed his gratification at the way in which his paper had been received. With regard to Dr. Simpson's remarks, he stated that in the developing ovary, say of a human fetus of seven months, the corpuscles of the germ epithelium and those of the connective tissue are so different in appearance that no mistake could arise if you look at them side by side. Moreover, if we carefully examined those groups or clusters of germ epithelial corpuscles embedded in the meshes of the stroma of such an ovary, we should find that all the corpuscles swell up, and become large, globular, and vesicular bodies, none of them retaining their original columnar form and small size; nor do they undergo division to produce the cells of the membrana granulosa, as stated by Waldeyer. But it could be seen that much smaller fusiform connective tissue corpuscles of the stroma work their way in between the developing germ epithelial corpuscles, and from these the cells of the membrana granulosa are produced. All these facts could be seen in the various preparations exhibited. It is the nuclei of the connective tissue corpuscles which enlarge, swell out, and become vesicular, and when fully developed they certainly do resemble the germ epithelial corpuscles on the surface of the ovary, but not at an early stage of their development. He had been much struck with the thorough accuracy of Dr. Martin Barry's work, of which he had read a great deal lately. To Dr. Barry belongs the credit of having shown that the germinal vesicle appeared first in the stroma, and that the Graafian follicle was subsequently formed around it.

---

*A Case in which the Fetal Heart was heard Early in Pregnancy.*

By Dr. UNDERHILL.

On 6th September, 1874, I was asked by my brother, a medical student, to see a woman, Mrs. M'Gair, aged thirty, multipara, who was ill with fever, in the High Street. I found her a very spare and thin person, suffering from a moderately severe attack of typhus fever. She said she was several months pregnant—about three or four, but could not tell exactly. The abdominal wall was thin and lax, as she had had several children before. On palpation, I could not make out distinctly the shape of the enlarged uterus, but percussion revealed dulness extending for about two inches above the pubes. On placing the stethoscope on the dull part, and to the right of the middle line, I distinctly heard the double pulsation of the fetal heart, and, taking out my watch, counted it: it was beating at the rate of between 160 and 170 pulsations per minute, while the mother's pulse, which I also counted, was only about 120. My brother afterwards listened and heard the heart beats and also



counted the number of pulsations, making them about the same as I did. I recommended the removal of the patient to the Royal Infirmary, where she was taken two days later, and was dismissed convalescent in a fortnight. She was delivered of a very large and healthy child on the night of 8th March, 1875—that is to say, six calendar months and two days, or 183 days, after I heard the fetal heart, which was, taking 278 days as the average duration of pregnancy, only 96 days (13 weeks and 5 days) after the supposed successful coitus. It is right to add that the woman expected to be confined some time before the labour came off, and the very large size of the child makes it possible that pregnancy was protracted somewhat longer than usual, and that consequently an earlier date should be fixed for its commencement. But still the fact remains that the fetal heart was heard in this case six calendar months before parturition, that is, about the beginning of the fourth calendar month of pregnancy.

I have thought this case worthy of a short note, as being one of the earliest dates on record of hearing the fetal heart, and because it shows that in a favourable case the diagnosis of pregnancy may be made certain in this way much earlier than is usually supposed. We were fortunate in this case on hitting upon the spot where the heart was nearest the surface at once, but in other cases in which I have examined for it in the middle months of pregnancy, I have generally found it to require considerable and sometimes prolonged search before it could be hit on, and the extreme mobility of the fetus at this early period seldom allows of its being heard for many minutes consecutively in the same spot.

The period at which the fetal heart first becomes audible is very differently estimated by different authors.

M. Depaul,\* who investigated the subject with great care, states that in 73 cases in which he examined for the fetal heart-beat before the 20th week 11 were in the third month, and in none of them was it audible; 22 were in the fourth month, and in two of them only, each of whom had reached three and a half months, and dated from single coitus, was it perceptible; in the remainder, who were more than four calendar months pregnant, not to hear the fetal heart was the exception. He further relates that, in endeavouring to show that the uterine souffle is audible at a very early period of pregnancy, he came upon two cases which proved to him that the heart can be heard at the end of the third month. These cases may be summarized thus:—

No. 1. A. B., aged eighteen, menstruated 18th July, married 28th July, fetal heart heard at the end of October (day not given), 148 pulsations per minute; auscultation was repeated with the same result a week later. Delivery at the end of April. The fetal heart being thus heard at the end of the third month.

---

\* "Traité d'Auscultation Obstétricale." Paris. 1847. Pp. 243 *et seq.*

No. 2. C. J., mother of several children, menstruated 10-15th of April, cohabited with her husband 17-20th of the same month. Her husband then left home for a few days, and before his return she took typhoid fever. Fetal heart heard on 1st of August (140 per minute). Delivery of a fully developed mature child on 19th January of following year. The time in this case being the 15th week.

With regard to the first of these cases, Depaul says that it proves that the heart can be heard "*déjà à la fin de l'onzième semaine,*" but, as stated, it is clear that at least three calendar months must have elapsed from the date of possible impregnation. His statement therefore that he has heard it at the end of the 11th week, which is copied into most of the text-books, is certainly not borne out by the case as he relates it.

Naegele, in his *Treatise on Obstetric Auscultation*, tells us that the 18th week was the earliest period at which he heard it, and that only in primipara; it was made out in 30 out of 50 cases before the middle of pregnancy, and in almost all these examined after the 20th week.

Evory Kennedy\* says that it may in a few cases be detected before the end of the fourth month, but as a rule we must not look for it before the period of quickening.

Cogeaux makes it during the fourth or fifth month, more frequently the latter, but states that in one case he "thought he heard" it a little before the fourth month, but could not re-examine the woman until six weeks after.

Leishman's words on the subject are, "It is certain that, as a general rule, it is not until the fifth month that the pulsations can be detected; but many trustworthy authors declare that they have heard them in the course of the fourth month, and even as early in some few cases as the eleventh week" (alluding to Depaul's case mentioned above, which he looks upon with incredulity). According to him they may be heard occasionally in the first week of the fourth month, but the eighteenth week is the average period.

The question of the earliest period at which these sounds become audible is of importance from two points of view: first, from the bearing it has upon practice in relation to the diagnosis of pregnancy in the earlier months; and, secondly, it is of physiological interest as showing at how early a period of its development a fetus possesses a heart capable of producing sounds audible through the abdominal wall of the mother.

Thus, in the fourth lunar month, i.e., from the end of the twelfth to the end of the sixteenth week (and you will remember that in the case before us the fetus was supposed to be in the 14th week), the fetus, according to Schroeder, is from 10 to 17 centimetres ( $4-6\frac{1}{2}$  inches) in length, and weighs as much as 1860 grains, or about

---

\* "*Observations of Obstetric Auscultation,*" p. 101. Dublin. 1833.

$3\frac{3}{4}$  ounces; and it is during this month that the external genitals first begin to assume the sexual distinction; similarly, Caspar says, that at the end of the fourth lunar month the embryo weighs  $2\frac{1}{2}$ –3 ounces, and is from 5 to 6 inches long, the sex is just recognisable, &c. Now, it seems to me to be a point worthy of attention, if it can be proved that a being in other respects so small and imperfect, yet has a heart of such considerable power, for the size of the organ must be exceedingly small.

It is true that, as is well known, the heart of the fetus during the earlier months is very much larger relatively than it is later in intra-uterine life, and occupies at one time nearly the whole thoracic cavity, lying nearly in a vertical direction; at the 2nd month the proportion of weight of the heart to that of the whole body is stated by Meckel (I quote from Quain's "Anatomy") to be as 1 to 50, the ratio being gradually reduced to 1 in 120 at birth, and 1 to 160 in the adult on an average. At this period of development, moreover, the auricles are much larger than the ventricles, and the foramen ovale is still a widely open orifice—points which bear upon the cause of the first sound of the heart.

This communication was merely intended for a note, and I only wished to point out that this subject will bear and repay additional investigation.

Dr. RITCHIE had examined a case in which he heard the fetal heart before the 16th week of pregnancy; the female had last menstruated 12 weeks before he saw her, but from the date of delivery at full time he considered the pregnancy to be of more than 12 but not exceeding 16 weeks, when he was first consulted.

## OBSTETRICAL SOCIETY OF DUBLIN.

*Meeting, July 10th, 1875.*

LOMBE ATTHILL, M.D., *President, in the Chair.*

*On the History and Use of the Short Straight Midwifery Forceps as a Tractor, and of the Long Double Curved Forceps as a Compressor and Lever.*

By THOMAS MORE MADDEN, M.D., M.R.I.A.

Within the last few years the midwifery forceps has once more come into frequent use, after a long period of obstetric darkness, during which even the most eminent accoucheurs, being ignorant of the value of this instrument, or incapable of applying it, resorted to craniotomy whenever the natural efforts failed to accomplish delivery in cases of difficult labour. A great advance had been already made in this respect before I first became a student of midwifery, and "cold steel for the child, followed by mercury for the mother," was no longer the

rule of practice in such cases. Still, even then the aphorism inculcated in the lecture-room, and acted upon at the bedside, was that urgent necessity alone warranted any instrumental interference with labour, and the cases in which this necessity was held to exist were comparatively few and far between. The forceps was seldom applied without a consultation, and very rarely indeed until the os uteri had been for some time fully dilated. But now—*nous avons changé tout cela*—the forceps is used as freely as the catheter, and instrumental delivery promises to become soon the rule, and natural labour almost the exception.

This transition from complete neglect to habitual use is merely an additional instance of those strange reactions in opinion and in practice of which the history of our profession presents so many remarkable examples. The judicious use of the forceps, by which living children may be safely delivered from living mothers in cases which might terminate fatally to either without its assistance, has been justly described as the greatest triumph of our art. For my own part, I have long endeavoured, by my "Lectures" and other writings, to contribute to the more frequent and timely employment of the short straight forceps. But if this, or any other forceps, be resorted to, as some have recommended, in almost every case of labour, the inevitable result will again be its exclusion for another period from its proper place in midwifery practice. And therefore it especially concerns those who advocate the timely and judicious use of this instrument to deprecate its premature or unnecessary application.

These extreme practices have never been sanctioned by the Dublin School of Midwifery; but recently certain views in favour of the very early and frequent use of the forceps have been brought forward by obstetricians of high eminence and great experience, in whose hands this has undoubtedly proved very successful. However, as this practice, if largely followed by others less expert, would probably have different results, since the great majority of practitioners cannot possibly have opportunities for acquiring that special operative dexterity which alone can ever render it safe or facile, it should not be adopted as a general rule without careful consideration. Therefore, in the hope of eliciting the opinion and experience of the Dublin Obstetrical Society on a question of much practical importance, I now submit an account of my use of the forceps in a large number of cases, and of certain modifications which I consider as improvements in the long and short forceps, together with some preliminary observations on the history of these instruments and the circumstances under which they should be resorted to.

The history of the invention of the midwifery forceps, the strange desuetude into which it fell for many years, and its reintroduction into modern practice, form one of the most interesting chapters in the annals of medical discovery, and convey a lesson, the practical application of which to the present time has been too generally lost sight of, owing to the prevailing neglect of ancient medical literature.



"The mental disease of the present day," says Johnson (and the observation is surely more applicable now than when penned, one hundred and thirty years ago), "is impatience of study, contempt of the great masters of ancient wisdom, and a disposition to rely wholly on unassisted genius and natural sagacity. If no use is to be made of the labours of past ages, the world must remain always in the infancy of knowledge. The discoveries of every man must terminate in his own advantage, and the studies of every age be employed on questions which the past generation had discussed and determined."

I have elsewhere enlarged on this subject, and have shown in two recent papers\* that some of our most valued improvements in gynæcology and surgery—such, for instance, as the dilatation of the os uteri by sponge tents, the local application of nitric acid in uterine diseases, the use of the vaginal speculum, and the employment of anesthetics before surgical operations—are all instances of the revival of old and disused practices as modern discoveries and improvements. The same history applies to the midwifery forceps, and even the very discussion we are now engaged on as to its proper use has been anticipated upwards of a century ago:—

"For out of the olde feldis, as men saieth,  
Comith all this newe corne, fro yere to yere ;  
And out of old bokis, in good faith,  
Comith all this newe science, that men lere."

Most writers who have treated of the history of the forceps since 1794, when Mulders' "Historia Literaria et Critica Forcipium et Vectium" was published, appear to have taken not only their narrative, but also their quotations, at second-hand from this work, of which therefore I have not availed myself, but have compiled the following sketch of the history of this instrument, and taken my citations as far as possible from the original authorities.

The invention of the forceps is generally ascribed to the elder Chamberlen, whose family monopolized the obstetric practice of London for three-quarters of a century. Even Dr. Churchill acquiesces in the common opinion. "There can now be no doubt," he says, "of the credit of the invention being due to Dr. Paul Chamberlen, and I think I have shown that there is presumptive proof that it took place before the year 1654."† Having, however, devoted a good deal of attention to this question, it appears to me that the only merit the Chamberlens are entitled to is that of improving an old and less perfect instrument, designed for the same purpose, and described in works with which Dr. Paul Chamberlen, who lived at a time when medical literature was circulated in a language common

---

\* "On the Probable Employment of Anæsthetics in Surgical Practice in Ancient Times." By Thomas More Madden, M.D. *Dublin Medical Journal*, Dec. 1874.

† "Researches on Operative Midwifery." By Fleetwood Churchill, M.D. P. 111. Dublin. 1843.

to the learned in all countries, could hardly have failed to be conversant.

The forceps is not mentioned by any of the known Greek or Roman medical writers, whose obstetric knowledge, however, with the exception of Celsus, was extremely limited. For midwifery was then almost exclusively confined to female practitioners, the higher class of whom, the *Medicæ*, or *ιατρίναι*, were entirely distinct from the *Obstetrices*, or *μῆναι*, as the mere midwives were called, and appear to have corresponded very closely to the "lady-doctors" of the present day, and we have evidence that the forceps, or something of the same kind intended for the same purpose, was not unknown to the latter at least eighteen hundred years ago, in the discovery of a similar instrument in the house of a Roman obstetrix in the excavations at Pompeii.\*

The first known reference to the forceps is that of Avicenna, the Arabian physician of the tenth century, whose works were translated into Latin, and published at Basle in 1556, by Andrew Alpago, from whose edition I have taken the following chapter, in which the author refers distinctly to the use of the forceps for the delivery of living children in cases of difficult labour, and makes this more evident by going on to direct that, in case the midwife fails with the forceps, she must then resort to embryotomy, as in the case of a dead child :— "Cap. 26. De Regimine ejus cujus partus sit difficilis causâ magnitudinis fœtus—Oportet obstetrix bona faciat retentione hujusmodi fœtus : quare subtiliter in extractione ejus paulatim ; tunc si valeat illud in eo, bene est ; et se non liget eum cum margine panni, et trahat eum subtiliter attractione post attractionem. Quod si illud non conferet, administrentur forcipes, et extrahatur cum eis. Si vero non confert illud, extrahatur cum incisione, secundum quod facile sit, et regatur regimine fœtus mortui."†

A century after the time we find two midwifery instruments, which, in the Latin version, are mentioned as "Forcipes," were described by Albucasis—i.e., the long forceps or *Almisdach*, and the short forceps, or *Misdach* ; but these instruments, from their construction, were obviously not intended for the extraction of a living child, and hence may be dismissed without further consideration.

The directions of Avicenna as to the use of the forceps were repeated by Mercurialis, a writer of the sixteenth century, whose treatise, "De Morbis Muliebribus," was reprinted by Spachius in 1597. In this work he says :—"When the labour is rendered difficult by the size of the child Avicenna gives the following rules—'Prima est ut obstetrix tenent manibus educere. Si vero manibus no potest, fascia circumligetur fœtus corpus, atque ita paulatim educatur. Si vero hoc

\* Adams, "Translation of Paulus Ægineta," vol. i. p. 652.

† Avicennæ "Medicorum Arabum Principis, Libres Canonis de Medicinis, Cordialibus et Cantica jam olim quidem a Gerardo Carmonensi ex Arabico sermone in Latinum conversa et partia vero ab Andrea Alpago infinitis penecorrectionibus," &c., p. 724. Basilæ. 1556.

non succedat habent obstetrices quædam tenacula quibus circumligant pannos ne lædant vel offendant fœtam iisque educant.”\*

Jacobus Rueff, in his treatise, “De Conceptu et Generationis Hominis,” published at Zurich in 1524, and also reprinted in Spachius’ collection—“Gynæciorium Grecorum Arabum Latinorum, Veterum et Recentium, &c. Opera et Studio Israelis Spachii Med. D. Fol. Argentinæ, 1597”—describes and gives an engraving of a midwifery forceps—“In hoc casu si postulaverit necessitas, huic instrumento forcipem qua dentes eruuntur adhibeas, vel depictam hinc forcipem longam et tersam, qua ita utatur commode, ut si possibile sit, id quod protrahendum est, educat faciliter.”†

The earliest English reference to the use of any instrument, apparently for the same purpose as the vectis, is contained in James Cook’s “Mellificum Chirurgiæ, or Marrow of Chirurgery,” the first edition of which was published in 1647, and is quoted in Dr. Aveling’s interesting “Biographical Sketches of British Obstetricians,” in the OBSTETRICAL JOURNAL for October, 1873 :—“Being commanded by the Lady Dowager Brook to wait on her to London, to take the consult of physicians, in the way before we came to Tossiter, we met with the tidings of that fatal fire of London, which caused her Honour to resolve for Hackney. After some time of her being there I was desired by Mrs. Hatton to go visit one near her time of her first child, who was aged. She begged of me to come to her if there was need. I told her there were several men abler than myself, and fitted with instruments which I wanted, that might be had from the city (he, doubtless, here, says Dr. Aveling, refers to Peter Chamberlen). After two or three days, in the night she sent for me. I being very much indisposed, and the night tempestuous, I denied; but, being very much importuned by a gentlewoman, I went. When come, I made trial, and found the child came right, but without advantage, though pains were strong. I made use of what came next my thought, getting it a little better fitted at a smith’s shop hard by, with which I brought away the child, though with much difficulty.”

The forceps of Avicenna, like those of Jacobus Rueff, were small and imperfect instruments, the opposite blades being united by a fixed point, and therefore necessarily introduced into the vagina together, and there opened to catch hold of the head of the child, *si possibile sit!* This same malconstruction occurs in Chamberlen’s first forceps, which was exhibited by Dr. M’Clintock at the last meeting of this Society, and is merely an enlarged copy, with fenestrated blades, of the “forcipes longa et tersa,” described by Rueff in 1524. In Chamberlen’s second forceps we find that he had discovered the inconvenience of the fixed point, and I think the only credit he deserves is that of opening and enlarging the blades, and doing away

\* Mercurialis, in Spachius’ “Gynæciorium,” &c., p. 237. 1597.

† Jacobus Rueff, “De Conceptu,” &c., in Spachius’ “Gynæciorium,” p. 179.

with this articulation. Even on their own showing, none of the Chamberlens, from Dr. Paul, the supposed inventor, down to Dr. Hugh, the translator of Mauriceau's work, are entitled to any gratitude from posterity for their boasted discovery of an instrument professedly designed to save life and relieve suffering, but which they sordidly kept a close secret for their own aggrandisement. Up to the time when the last, the sixth, edition of his translation of Mauriceau's first volume was published in 1715, Dr. Hugh Chamberlen still retained his secret. "My father, brother, and myself (though none else in Europe as I know), have by God's blessing and our own industry, attained to, and long practised a way to deliver women in this case without any prejudice to them or their infant, though all others (being obliged, for want of such an expedient, to use the common way) do, and must, endanger, if not destroy, one or both with hooks."

Some years ago the late Dr. M'Keever, who has very recently passed away from amongst us, with all his faculties unimpaired by advanced age, and who was long distinguished as an obstetric writer and practitioner, presented me, amongst other papers, with the manuscript now shown, which contains a version of the history of Chamberlen's failure with the forceps in his Paris case, as related in the earliest Lectures delivered in Edinburgh on midwifery. The first Professor of this subject in that University was Dr. Gibson, who was appointed in 1736, but died before entering on his professorial duties, being succeeded by Dr. Young, of whose unpublished lectures the manuscript is now before the Society. Dr. Young's account is evidently founded on Mauriceau's, from which it only differs in saying that, "The woman died under his hands undelivered, upon which he quitted Paris without selling his secret. This afterwards turned out to be the forceps, as we learn from Chapman, the material thing in whose book is the discovery of that noble instrument, the forceps, which has saved the lives of thousands that otherwise must have been lost. The next writer is Giffard, who practised about the same time with Chapman, and it was he that introduced the frequent use of the forceps, and who perhaps had more practice with them than any of his predecessors, or even successors. . . . Chapman only delivered six, and these with one single blade of the forceps. This single blade is what is called Roonheysen's secret, and in Holland none are allowed to practise midwifery without being instructed how to use this single blade by the Professor appointed for that purpose."\*

The foregoing account of the introduction of the forceps into practice differs somewhat from that given in another manuscript, also in my possession, containing the unpublished lectures on midwifery delivered in Edinburgh in 1776 by Professor Hamilton. In the latter it is erroneously stated that before attempting to use the forceps in his celebrated Paris case, Chamberlen had obtained a thousand pounds

---

\* Dr. Young's Manuscript Lectures.



from the French Government for divulging his secret. "This sum," says Dr. Hamilton, "was readily granted, and he was called to the next laborious case that occurred, but in this he was foiled, and Mauriceau afterwards delivered the woman by opening the child's head, but the woman died, as Mauriceau mentions, from the instrument of the English operator wounding the uterus in several places. Chamberlen left Paris, and came home by Holland, and it is said there showed the forceps to Roonheysen; this, however, is disputed, but most certainly it was not known at Paris for a long time after—not, I believe, till 1734. Most certainly it was not known at the time that Palphyn came to Paris to publish his system of surgery. . . . After Chamberlen, Chapman improved them, but very little. Both his and Chamberlen's were straight, by which they could not be worked with without the handles injuring the woman very much behind. Levret introduced a curved pair. Freke armed his with a crochet at one end, and a blunt hook at the other, by which the practitioner went about armed at all points. Freke's instrument is too long; however, it is used to this day all over the Continent, with a very slight alteration. Smellie, who had a very considerable mechanical turn, improved the forceps most. He first constructed a wooden pair, but he found this so difficult of application that he soon gave it up, and had a steel pair made. Dr. Wallace Johnston next improved the forceps; he added the curve of Levret; he increased the breadth of the blades, and diminished the weight of the instrument. The London practitioners are every day inventing new ones, which are in no degree superior to this. After all that has been said about the forceps, I may now remark that a man who has been used to deliver with instruments may deliver with the shafts of a couple of spoons; yet young practitioners find considerable difficulty in delivering with the modern forceps."\*

The case in which Chamberlen failed to effect delivery with his forceps in Paris, even as narrated by Mauriceau, reflects more credit on the English than on the French accoucheur—the latter left the woman to die undelivered, the former at least attempted to assist her:—"On the 19th August, 1670," says Mauriceau, "I saw a small woman, aged thirty-eight, who had been in labour of her first child for eight days. The waters escaped on the first day without hardly any dilatation of the os. Remaining in this condition until the fourth day, I was sent for, and recommended the midwife to bleed her; and in case this did not produce the effect I hoped, to administer an infusion of senna to excite pains, which she had not; this was done the following day, and succeeded in causing pains, by which the mouth of the womb was dilated as far as possible. Nevertheless, I could not deliver, and the child had remained in the same situation, without being able to advance, for this woman was so small, and the

---

\* Dr. Hamilton's Manuscript Lectures, vol. i. p. 223.

bones (of the pelvis) so narrow and close to each other, and the sacrum so curved forwards, that it was quite impossible to introduce the hand to deliver her, although mine is small enough, . . . or to introduce the fingers sufficiently to enable me to use a crochet safely, so as to extract the child, which had been apparently dead for about four days. I declared the impossibility of delivering this woman to my assistants, who, being well persuaded of this, prayed me to perform the Cesarian operation, which I would not undertake, knowing well that it was always certainly fatal to the mother. But *after* I had left the woman in this condition, it not being possible for me to help her as I would any other of a more normal conformation of body, there came shortly afterwards an English physician named Chamberlen, who was then in Paris, and who, from father to son, made a profession of midwifery in England, in the town of London, where he thus acquired the highest reputation in that art. This physician finding the woman in the condition just stated, and learning that I had not found any possibility of delivering her, declared himself astonished that I could not do so. *Moy* (says Mauriceau, with all a Frenchman's untranslatable vanity), *qu'il disoit assuroit estre le plus habile homme de ma profession qui fort à Paris*; notwithstanding which he at once promised to deliver her most assuredly in less than half a quarter of an hour, whatever difficulty he might find. Accordingly, he immediately applied himself to the business, and in place of half a quarter of an hour, he worked for more than three entire hours without cessation, except to take breath. But having vainly exerted all his strength, as well as all his industry, and seeing that the poor woman was almost dead in his hands, he was obliged to abandon the attempt and to allow that he could not accomplish it, as I had well declared. This poor woman died undelivered twenty-four hours after the violence he had done her, and, at the examination I made in performing after her death the Cesarian operation, which I would not do before, as I have said, I found the child and everything else as I had before stated, and the womb all torn and pierced through in several places by the instruments which this physician had blindly used without the controul of his hand, which being a size larger than mine, he did not seem to have been able to introduce sufficiently far so as to preserve it." Mauriceau then goes on with great complacency to observe that the English physician, who had come six months previously to Paris in the hope of making his fortune, had circulated a report that he had a secret (*tout particulier*) for such cases, and vaunted that he could thus deliver in even the most desperate and otherwise hopeless cases in less than half a quarter of an hour, and had even proposed to the First Physician to the King that for a reward of ten thousand crowns he would disclose his pretended secret. "Mais le seule experience de ce fâcheux accouchement le dégouta tellement de ce pais-ci, qu'il s'en retourna peu de jours en suite au Angleterre; voyant bien qu'il y a Paris de plus habile gens en l'art des accouchemens que lui." But before leaving Paris, Chamberlen called

on Mauriceau, and after various compliments had passed between them, the latter thus concludes his account of the visit:—"Je recûs son compliment comme je devois lui faisant entendre qu'il s'était bien trompé en croyant trouver autant de facilité à accoucheur les femmes à Paris, comme il avoit pie trouver à Londres ou il retourna le lendemain emportant avec lui un exemplaire de mon livre ; qu'il fit imprimer après l'avoir traduit en Anglais en l'année 1672, depuis laquelle traduction il s'est acquis un si haut degré de reputation dans l'art des accouchemens dans la ville de Londres, qu'il y a gagné plus de trente mille livres de rente, qu'il possède présentement."\*

Amongst the writers who took part in the introduction into midwifery practice of instruments intended for the same purpose as the forceps, a prominent place must be assigned to M. Jean Palfyn, of Ghent. In 1708 Palfyn published at Leyden an anatomical continuation of Mauriceau's work ;† and twelve years later, being at Paris bringing out a new edition of his book, he presented to the French Academy of Sciences what he termed his *tire-tête*—a kind of extracting forceps, the handles of which did not cross, but were simply connected together by a ligature.

Although Chamberlen and, in a lesser degree, Chapman have generally been given the credit of introducing the forceps into English midwifery practice, the first who avowedly employed and recommended the use of this instrument was Mr. William Giffard, surgeon and man-midwife, who died before Chapman's book appeared. Giffard appears to have used his "Extractor," as he calls it, almost as freely as any modern accoucheur does the forceps, and, moreover, anticipated Smellie's plan of dilating the os uteri to apply this instrument, which has been again recently revived. I am indebted to the kindness of Dr. M'Clintock for the opportunity of referring to this scarce work.

The first case in which Giffard employed his "extractor," or forceps, occurred on the 8th of April, 1726, the patient being the wife of one of the Prince of Wales' servants, and, owing probably to the inexperience and timidity of the operator, was unsuccessful. Two years subsequently he relates the first published case in which the forceps was successfully used for the delivery of a living child. This occurred on the 28th of June, 1728. The woman had been for many hours in "labour which was delayed by inertia ; and having first administered a clyster and two cordial hypnotic draughts" at intervals of eight hours, he says:—"I then found the child but little advanced ; her

---

\* "Observations sur la Grossesse et l'Accouchemens des Femmes," &c. Par François Mauriceau, Ancien Prévost de la Compagnie des Maîtres Chirurgiens de la ville de Paris. Observation xxvi. p. 25. Paris. 1715.

† "Description Anatomique des Parties de la Femme qui servent la generation," &c. Lequelles ouvrages ont peut considéré comme une suite de l'Accouchement des Femmes par M. Mauriceau. Par M. Jean Palfyn, Anatomiste et Chirurgien de la Ville de Gand. Leide. 1708.



pulse was very quick and labouring, and the womb very much spread, so that I could entirely pass my fingers round the head to the ears, for it was no ways engaged, but loose ; wherefore, considering that her pulse grew languid, and that her strength decreased, I thought it advisable to attempt her delivery. I endeavoured to press the child back, that I might be able to turn and get the feet, but it was so locked at the shoulders that I was not able to move it, whereupon I passed my extractor and drew it with much difficulty forwards without the labia. . . . The child was born alive. This case proves that a child presenting right, but sticking in the passage, may be brought alive (I won't say always) without either the use of hooks, or lessening the head, contrary to the opinion of most former writers.\*

Giffard occasionally narrates the history of more than one forceps case occurring in the same day as an ordinary matter. Thus, on the 17th of May, 1731, he met with two cases "where," he says, "I thought it advisable to lend my assisting hand." The first was a case where the head was for some hours impacted in the pelvis ; and the second is a case—interesting at the present time, when the same practice is again recommended—of labour delayed by rigidity of the os, where he "was of opinion that the delivery ought to be immediately effected in respect both of the mother and of the child. . . . But as the os internum was not so fully dilated as readily to admit the passage of the head through it, I strove to stretch and widen it by putting the ends of my fingers between it and the child's head, and, by this method, made way for the more easily passing of the instrument, without bruising or tearing the parts."†

To Edward Chapman is due the credit of first making Chamberlen's secret known to the profession, as well as of improving its construction by substituting hard for soft metal, and disusing the riveted lock still retained in some French and American forceps. In his "Treatise on the Improvement of Midwifery," published in 1733, Chapman states that difficult labours, where the head lies low, can only be accomplished by either the fillet or by the forceps. "As to the forceps," he says, "which, I think, no person has yet any more than barely mentioned, it is a noble instrument, to which many now living owe their lives, as I can assert from my own knowledge and long successful practice."

The frequent use into which the forceps came as soon as it was known is evinced by numberless contemporaneous authorities ; but by none more clearly than by the author of a letter addressed to Chapman, and published in the third edition of his book. "All I can say," reiterates Chapman, "in praise of this noble instrument must necessarily fall short of what it justly demands. The following letter was sent to me by a gentleman who had been recommended to

---

\* "Cases in Midwifery, written by the late Mr. William Giffard, Surgeon and Man-midwife." Revised by Edward Hody, M.D. P. 49 London. 1734.

† *Ibid.*, p. 459.



me for information in this art, and has long practised with great success and applause :—‘ Sir, if you please to remember, about a week after I came into the country, I acquainted you that I was called to a woman in labour, where the child presented with the head far advanced in the vagina, with the os uteri extending. I delivered her with the forceps, and neither the mother nor the child received the least injury. Since that time I am come into such credit, that I am frequently called in twice or thrice a week ; and, I thank God, I have not, as yet, met with the least mishap. Our midwives here are pretty dexterous, but when the head falls so low as to require the use of the forceps, they are at a loss. I have had two cases where I was obliged to deliver feetways, the heads of the infants not offering directly right for the instrument. *All the rest I delivered with the forceps.*—Yours, &c., John Paget. Lullworth, Oct. 30th, 1734.’”\*

The years 1733 and 1734 are memorable in the history of the forceps, for not only were Giffard’s and Chapman’s works then published, but, at the same time, Mr. Alexander Butter, Surgeon in Edinburgh, communicated to a Society in that city—“ The description of a forceps for extracting children by the head, when lodged low in the pelvis of the mother.” “ The forceps,” he says, “ for taking hold of a child’s head when it has fallen so far down among the bones of the pelvis that it cannot be pushed back again into the uterus, to be extracted by the feet, and when it seems to make no advances to the birth by the throes of the mother, is scarce known in this country ; though Chapman tells us it was long made use of by Dr. Chamberlen, who kept the form of it a secret, as Mr. Chapman also does. I believe, therefore, that a sight of such an instrument—*which I had from Mr. Duse, who practises midwifery at Paris, and who believes it to be his own invention*—would not be unacceptable to you, and the publication of a picture of it may be of use to some of your own readers.”†

In 1742 the use of the long forceps, which appears to have been even then “ in general use all over Europe,” was described by Mr. (afterwards Sir) Fielding Ould, who succeeded Dr. Mosse as the second Master of the Dublin Lying-in Hospital. Ould’s work is very interesting, as it contains clear directions for the performance of version as a substitute for craniotomy in certain cases of obstructed labour, for the proposal of which the late Sir James Simpson obtained so much credit a hundred years afterwards. Mr. Ould also forestalled a suggestion made a few years ago by the late Dr. Beatty for preventing impending laceration of the perineum and recto-vaginal septum during labour by incising the perineum. But on the subject

---

\* “ A Treatise on the Improvement of Midwifery.” By Edward Chapman, Surgeon. Third Edition, p. 89. London. 1759.

† “ Medical Essays and Observations.” Published by a Society in Edinburgh. P. 321. Edinburgh. 1735.

of the forceps Ould merely repeats the directions of former writers. Speaking of labours delayed by disproportion or inertia, where the child is living, "or, rather if there be not a certainty of its death, in this case," he says, "the best adapted instrument is the long forceps, which is in general use all over Europe, wherefore it needs no particular description. . . . Being thus provided, we proceed to the operation by placing the woman on her knees, &c."\* Immediately after its publication, Ould's work was unsparingly attacked by a rival Dublin accoucheur, Dr. Southwell,† who printed two pamphlets on the subject—one in Dublin, and the other shortly afterwards in London. In the former he reproaches Ould with being "the youngest surgeon practising midwifery in this city; a man not conversant with authors; and, at best, but a novice in practice. . . . I shall only add, in general, Mr. Ould is totally ignorant of the regular use of instruments. He entirely mistakes the right use of the large forceps."‡

In 1752 Dr. Smellie's Treatise, from which a new era in midwifery practice may be dated, was published. To Smellie we owe what were until very lately the best types of the short and long forceps, as well as the clearest directions for using them "on rational and mechanical principles." Nay, on comparing his writings with those of his successors for upwards of eighty years, we find that when, in the course of time, Smellie's teachings were supplanted by those of William Hunter, Osborne, and Denman, and even down to the date of Blundell's and Collins's works, midwifery retrograded; and only within the last thirty-five years has it regained the ground lost since Smellie's time, and has progressed as much beyond his practice as he had advanced beyond that of any of his predecessors.

The mode of effecting delivery with the forceps before the full dilatation of the os uteri was distinctly described by Smellie, who warns his readers that "in stretching the os externum or internum, we ought to imitate nature; for in practice we find that when they are opened slowly and at intervals by the membranes or by the child's head, the parts are seldom inflamed or lacerated. . . . We ought, therefore, when obliged to dilate those parts, to proceed in that slow and deliberate manner; and though, upon the first trial, they feel so rigid that one would imagine they could never yield or extend, yet, by stretching with the hand, and resting at intervals, we can frequently overcome the greatest resistance.§

The prudent caution which induced Smellie for many years to

---

\* "A Treatise of Midwifery." In Three Parts. By Fielding Ould, Man-midwife. P. 156. Dublin. 1742.

† "Remarks on some of the Errours in Anatomy and Practice in a late Treatise of Midwifery." Published by Fielding Ould, Man-midwife. By Thomas Southwell, M.D. and Man-midwife. P. 41. Dublin. 1742.

‡ "A Continuation of Remarks on Mr. Ould's Midwifery." By Thomas Southwell, M.D. and Accoucheur. London. 1744.

§ Smellie's "Midwifery." P. 159.

refrain from recommending or even showing his long forceps to his class, was founded on reasons still applicable. "In order," he concludes, "to disable young practitioners from running such risks, and to free myself from the temptation to use too much force, I have always recommended the forceps so short in the handles that they cannot be used with such violence as will endanger the woman's life."\* And in his collection of cases he says:—"But if these expedients be used prematurely, when the nature of the case does not absolutely require such assistance, the mischief that will ensue will often overbalance the service for which they are intended. I did not then recommend the use of them (the long forceps), because I was afraid of encouraging young practitioners to exert too great force, and give their assistance too soon."†

Hardly was Smellie's work published than its author's scholarship and style, and, still more, his practice with the forceps, were vehemently assailed by Dr. Burton, of York, whose portrait and obstetric armament have been immortalized by Sterne in "Tristram Shandy." "Great Son of Philumnus, what can'st thou do? Thou has't come forth unarmed; thou hast left thy *tire-tête*, thy new-invented forceps, thy crochet, thy squirt, and all thy instruments of deliverance behind thee." The "new-invented forceps" referred to was an instrument somewhat like a crab's claw, recommended by Dr. Burton.

Dr. Burton's attack on Smellie, though virulent in the extreme, is evidently the work of a learned and able man. Its animus is sufficiently shown by the title—*i.e.*, "A Letter to William Smellie, M.D., containing Critical and Practical Remarks upon his Theory and practice of Midwifery. By John Burton, M.D. Wherein the various gross mistakes and dangerous methods of practice mentioned and recommended by that author are fully demonstrated and generally corrected. London: 1753."

"To confound all nature," he says, "all distinctions of sex, to make animals vegetables, and the one and the same author two different persons, and neither character agree with the true one; to palm upon us an author that never existed; to pass over in silence several material things that contradict your own practice in those authors that are genuine, and to make them say things they never dreamed of, in order to countenance it, is such a piece of history as the present day cannot boast of; yet, strange as this may appear, you have done it. And if anything can be added to shock human faith, or prejudice your character as an historian or translator, it is your having converted *Lithopodii Senonensis Icon*, which you call *Lithopedus Senonensis*, an inanimate, petrified substance, into an author, after you had been six years cooking up your book."‡

---

\* "A Treatise on the Theory and Practice of Midwifery." By William Smellie, M.D. P. 162. London. 1752.

† "A Collection of Cases and Observations in Midwifery." By William Smellie, M.D. P. 4. Sixth Edition. Dublin. 1764.

‡ "Burton's Letter to Smellie," p. i.

If Smellie's writings and practice were fiercely assailed, they were no less warmly vindicated by contemporary writers. Thus the Manuscript Lectures of Dr. Young, already noted, contain the following remarks :—"The great Dr. Smellie, ever to be held in esteem by all succeeding accoucheurs—men who ought to hold his memory in esteem have taken great liberties, and presume to find fault. But with regard to this matter I differ in opinion with the self-conceited blockheads who have not been able to produce anything equal to this good man. The second part contains one hundred pages on laborious births, where he gives full and explicit directions for using the forceps ; and, forsooth, here again they find fault by saying he recommends their too frequent use. But every man of merit is the subject of envy to the ignorant and weak (I had almost said pettyfoggers of the profession). . . . And every unprejudiced person must allow him the merit of being the first who gave us a proper idea of using that noble instrument with ease and elegance, although they were in the hands of the Chamberlens, Chapman, and Giffard long before."\*

"I knew him well," says the anonymous author of a furious diatribe against the employment of men in midwifery practice, published in 1772, speaking of Dr. Smellie ; "he was an honest man, and not only a faithful compiler of the doctrines and sentiments of other writers on the subject, but whatever he advanced as new and properly his own was founded on real facts and observation ; and, what ought still more to recommend him and enforce his authority with those of his fraternity, he was an enthusiast in his profession ; man-midwifery was the idol of his heart, and he believed in his forceps as firmly as he did in his Bible."†

A few years after its first introduction into English midwifery practice, we have evidence to show that the forceps had come into such general requisition, that its over frequent employment, or mis-application, led to that widespread prejudice against its use, from the effects of which the practice of midwifery has only very recently been emancipated.

One of the most strenuous opponents of the forceps was the anonymous writer just quoted, whose attack is worth citing as indicating the frequency with which the forceps was employed upwards of a century ago, and pointing out the commencement and causes of the prejudice with which it was regarded for so many years, and which is not undeserving of special consideration at this time :—

"This instrument (the forceps) was, for some time, in the possession of a few practitioners only, nor has it been publicly known above forty years. But as soon as it was made public, it is surprising with what avidity it was adopted, insomuch that, for the first twenty years, the whole study of the men midwives was how to new model and

---

\* Dr. Young's Manuscript Lectures, p. 18.

† "The Present State of Midwifery Considered," p. 40. London. 1772.



improve its form and make, to delineate the various methods of using it, and to demonstrate in what a variety of situations and positions of the child it might be serviceable, till they, by degrees, found out that there could hardly occur a case in midwifery but where the forceps might be used with advantage. . . . I can hardly, therefore, fancy myself exceedingly presumptuous if I declare the forceps to be quite as useless to women in labour as either the blunt hook or fillet. But I must beg leave to go still a little further upon this head, and observe that this is not only a useless but also a very pernicious instrument, for by hastening delivery before the parts are properly distended by the natural pains and strainings of the mother, such dreadful lacerations are made, both internally and externally, as must frequently prove fatal, or, at best, the source of much inconvenience and misery to the unfortunate woman who has been the subject of such practices. . . . Nor am I by any means singular in my opinion of the inutility of this instrument. The best practitioners in midwifery have given it up, and very seldom have recourse to it; and I am credibly informed that the man who has for many years been deservedly esteemed the practitioner of the greatest skill and judgment of any who profess the obstetric art in this kingdom (this evidently refers to Dr. William Hunter), declares that he has seldom or never, during the whole course of his practice, used the forceps, or met with a case where he thought it necessary to do so; unless he may be said to use them when he occasionally introduces a single blade to remove any impediment which the head of the child may accidentally meet with by pressing upon some of the bones of the pelvis, whereby its descent and delivery are retarded; but he adds that occasions for this very seldom happen; he could almost always get the better of such obstacles with the hand only.”\*

There can be no doubt that the forceps or vectis was, at this period, greatly abused in both English and foreign midwifery practice, for in the latter we read of one accoucheur boasting of 800, and another of 1200 instrumental deliveries, and of the same state of practice in England we are assured on the authority of Osborne† and Denman.‡

In contemporaneous medical literature we find constant reference to the frequent use of the forceps. Thus, this is one of the heaviest charges brought against the obstetricians by the author of the most unjust and indecent attack ever made on our profession. “Men-midwives,” says this scurrilous writer, “seldom wait for nature’s moment. Women are objected to because they are tedious. Men are extolled because they are quick. If Dr. — has one or two

---

\* “The Present Practice of Midwifery Considered,” p. 79. London. 1772.

† “Essays on the Practice of Midwifery.” By William Osborne, M.D. P. 142. London. 1792.

‡ “Introduction to the Practice of Midwifery.” By Thomas Denman, M.D. P. 275.

pregnant ladies waiting, from whom he expects handsome payments, he will take merit for hastening the birth, and if any accident happens from his impatience, his reputation is too well established to suffer in the eyes of mankind, and the misfortune is attributed to some of the common casualties attending labour, when it derived its source solely from the doctor's having brought the child forward unnaturally before the parts were predisposed, by a proper distension, for its reception and passage. I fear two ladies died lately from this very practice; the parts inflamed, the inflammation spread by sympathy, the bowels mortified. The men-midwives not only give rise to inflammation by bringing the child before the woman has felt half the number of pains which nature intended to predispose the parts, but likewise by their dilatations. Can any practice be more repugnant to common sense than that of irritating the exquisitely sensitive nervous fibres of these parts by way of preparing them for distension? The men absolutely counteract the very end they pretend to have in view by dilatation! Friction must irritate, irritation must inflame, inflammation must contract.”\*

The reaction against the forceps now set in, and, being supported by men so eminent as William Hunter, Denman, and Osborne, as well as their successors in the early part of this century, has continued to affect midwifery practice down to a very recent time.

“It is scarcely possible,” observes Denman, “to say too much against a hasty resource to the forceps, even in cases which may ultimately be relieved by using them, and neither this nor any other instrument is now used in the practice of midwifery one-twentieth part as frequently as they were fifty years ago. . . . The use of instruments of any kind ought not to be allowed in the practice of midwifery from any motives of eligibility. Whoever will give himself time to consider the possible mistakes and want of skill in younger practitioners, which I fear many of us may recollect, the instances of presumption in those who, by experience, have acquired dexterity, and the accidents which, under certain circumstances, seem scarcely to be avoided, will be strongly impressed with the propriety of this rule, as well as from the general reason of the thing.”†

Dr. Osborne says that “in the state indicating the use of the forceps, all the powers of life are exhausted, all capacity for further exertion is at an end, and the mind is as much exhausted as the body; they would both together yield under the influence of such continued and unavailing struggles.”‡

---

\* “The Danger and Immodesty of Employing Men-midwives,” &c. Anonymous. Second Edition. P. 69. London. 1772.

† “Introduction to the Practice of Midwifery.” By Thomas Denman, M.D. P. 276.

‡ “Essays on the Practice of Midwifery.” By Wm. Osborne, M.D. Essay iv. sec. i. London. 1795.

"If you must err," says Dr. Blundell, "then take my advice and err rather by the neglect or rejection of instruments, than by their too frequent use ; for the cases in which you may use instruments without need are as numerous as the cases that fall under your care, with the exception of the few—very few—in which these weapons are really required."\*

It would be superfluous to add any other quotations from the countless authorities who, down to our own time, have repeated Denman's warnings against the too-frequent employment of the forceps, or to cite any of the almost equally numerous writers who now advocate this practice. The statistics I am about to adduce will show the practical effect of these teachings better than any mere statement of opinions could do.

The cases in which I have used the forceps myself are shown in the Table on following page, which contains an abstract of 163 forceps cases in hospital and private practice.

From 1745, when the Dublin Lying-in Hospital was first opened by Dr. Mosse, down to the present time, nearly two hundred thousand patients have been delivered in this great institution. But only seven of the Masters have left any detailed record of their practice, and from these separate Reports I compiled an account of the comparative use of the forceps at different times in the hospital for my "Lectures on the Forceps," since published. These statistics may probably be new to some of my hearers, and therefore I shall now briefly refer to them in proof of the desuetude of the forceps during many years, and the saving of life and suffering which has resulted from its reintroduction and judicious use in modern practice.

During the Mastership of Dr. Joseph Clarke, from 1787 to 1794, there were 10,387 deliveries in the hospital, and the forceps was only applied in 14 of these with 6 deaths. But the more easily used perforator and crochet were resorted to in 49 cases. And in his private practice, extending over forty years, Dr. Clarke only once attempted to use the forceps. In Dr. Labatt's Mastership, from 1815 to 1822, during which time 21,867 births took place in the hospital, the forceps does not appear to have been used in any instance. From 1826 to 1833, Dr. Collins used the forceps in 24 cases out of a total of 16,654, but employed the perforator in no less than 118 cases. From 1842 to 1845, Dr. Charles Johnson used the forceps in 18, the vectis in 16, and the perforator in 54 cases, in 6702 deliveries. From 1847 to 1854, in Dr. Shekleton's Mastership, there were 13,748 deliveries in the Rotunda, and the forceps was now used in no less than 220 of these, and the perforator in 54. Dr. M'Clintock, who ruled the hospital from 1854 to 1861, brought the forceps into still more frequent requisition, and in his last three years of office employed it or the vectis in 76 cases, or once in every 60, in 3700 deliveries,

---

\* "Principles and Practice of Obstetric Medicine." By James Blundell, M.D. P. 321.





whilst the number of craniotomy cases was reduced to 5. The next Master, Dr. Denham, has not yet published his Report, from 1861 to 1868, but was (as I had an opportunity of knowing when serving as his assistant, as well as subsequently under Dr. Johnston) a constant advocate for the timely use of the forceps, as well as a most dexterous operator with it. To Dr. Johnston, the present Master, undoubtedly belongs the credit, however, of having brought the forceps into more frequent use than had ever previously been the case. Thus, from November, 1868, to November, 1874, in 7027 deliveries, the forceps has been used in no less than 639 cases, or about once in every 11 cases, with only 39 deaths, whilst the proportion of craniotomy, or cephalotripsy, cases has been reduced to 29.

The foregoing statistics show that, as the forceps is used more frequently, the mortality in the cases in which it is employed diminishes, and, secondly, also shows the happy effect of the free use of the forceps in lessening the proportion of craniotomy cases in the hospital.

In 1872 Dr. Johnston employed the forceps in thirty-five cases before the os was fully dilated, and in the following year's Report he says :—"There were 36 of the foregoing cases in which we considered it prudent to apply the forceps before the os was fully dilated ; and as there are many still who will be astonished at this apparently bold mode of practice, and mayhap question its justifiability, I beg leave to assure them that, having adopted it for the last two years, during which time we delivered 71 such cases, we are more and more convinced each day of its great advantage in saving the lives of both mother and child."\* In his last Report, for 1874, Dr. Johnston again urges the advantage of this practice, which was spoken of, during the subsequent discussion in this Society, as a novel practice, and even as one "opening a new era in the history of midwifery." This practice, was, however, described in the earliest published accounts of the forceps, and so extensively did it at one time prevail as to lead the most eminent practitioners to reprobate the premature use of instruments in terms so exaggerated and so forcible as to prevent their followers, for many years, from resorting to their use even when most urgently required.

That the application of the forceps before the full dilatation of the os uteri is necessary in certain cases, especially of complex labour, is unquestionable ; and in the Table on the following page are given some cases in which I have thus employed the long forceps.

No fact in midwifery seems better established than that the dangers of childbirth bear a certain relation to the length of the second stage of labour, and that it matters comparatively little what the period of the first stage may be, provided that the second stage, when the child's head has passed through the pelvic brim,

---

\* "Report of the Rotunda Hospital, from Nov. 8th, 1872, to Nov. 8th, 1873." By George Johnston, M.D., F.K. & Q.C.P., Master. P. 10.

*Cases in which Forceps was applied before Full Dilatation of Os Uteri.*

No. of Case.	Age.	Preg- nancy.	Cause of the Operation.	Duration of Labour.	State of Os Uteri when the Forceps was applied.	Sex and State of Child.	Result to Mother.
1	28	1st	Rigidity of os, and threatened rupture of the uterus	29 hours	Half dilated ... ..	Female, alive ...	Recovered.
2	27	1st	Rigidity and exhaustion ... ..	37½ hours	Size of crown-piece ..	Male, alive ...	Recovered.
3	36	7th	Fetal heart failing ... ..	24 hours	Three-quarters dilated	Female, alive...	Recovered.
4	25	5th	Accidental hemorrhage ... ..	15 hours	Size of crown-piece ...	Female, still-born	Recovered.
5	27	5th	Exhaustion ... ..	16 hours	Three-quarters dilated	Male, alive ...	Recovered.
6	22	1st	Hemorrhage ... ..	12 hours	Three-parts dilated ..	Male, alive ...	Recovered.
7	40	3rd	Exhaustion ... ..	36 hours	Half dilated ... ..	Male, alive ...	Recovered.
8	20	1st	Rigidity, threatened rupture ...	26 hours	Size of crown-piece ..	Male, alive ...	Died 8th day, puerperal peritonitis.
9	20	1st	Rigidity and exhaustion ... ..	51 hours	Size of crown-piece ...	Male, alive ...	Recovered.
10	20	1st	Threatened convulsions ... ..	30 hours	Three-parts expanded	Male, alive ...	Recovered.
11	27	4th	Intense suffering, no advance for 8 hours	21 hours	Three-quarters dilated	Male, alive ...	Recovered.
12	24	2nd	Exhaustion, rigidity ... ..	80 hours	Size of crown-piece ...	Male, alive ...	Died 8th day of secondary hemorrhage.
13	30	7th	Exhaustion ... ..	29 hours	Half dilated ... ..	Female, alive ...	Recovered.
14	35	1st	Convulsions ... ..	16 hours	Size of a shilling when incised to effect delivery	Male, still-born	Died of convulsions 7 hours after delivery.
15	36	3rd	Rigidity ... ..	28 hours	Three-quarters dilated	Male, alive ...	Recovered.
16	28	1st	Exhaustion, rigidity ... ..	24 hours	Half dilated ... ..	Male, still-born	Recovered.
17	31	3rd	Rigidity, no advance for 12 hours	18 hours	Size of crown-piece ...	Male, alive.	Recovered.

is not unduly prolonged. But the ordinary definition of these stages, however useful to students, may be disregarded by practitioners when it is necessary to do so. In some cases we find the child's head down in the pelvis, and the labour more advanced before the os uteri is fully dilated than in others in which it has been expanded for some time, and, under these circumstances, we may, when necessary, apply the forceps as soon as the *os tinæ* is sufficiently dilatable. This procedure should, I think, be carefully restricted to cases of absolute and unavoidable necessity. In the hands of obstetricians so experienced and so judicious as Dr. Johnston and Dr. Nichols, by whom this practice has been recommended, it is, of course, safe and feasible, but the danger is that others, less experienced or less judicious, seeing its success under exceptional circumstances, may be emboldened to resort to it under less favourable conditions.

Natural labour is necessarily a slow process, by which the uterus contracts so as to expel its contents, which, and the parts through which they pass, must gradually accommodate themselves to the immense strain thus put upon them, and this gradual and permanent uterine contraction is essential to the life of the patient, and is her only safeguard against fatal post-partum hemorrhage. If the uterine efforts be allowed to continue too long without any assistance in a case of difficult or obstructed labour, this result may follow from exhausted contractility. But, on the other hand, if the child be dragged forth before the uterus has had sufficient time to contract on its vessels, the same consequence must be inevitably produced. If, therefore, obstetric practitioners should ever come to regard it as a safe rule of practice to apply the forceps as soon as the os uteri can be sufficiently expanded to admit its introduction, which in some instances might be done long before the occurrence of any true labour pains, is it not probable that the ill results of the indiscriminate and injudicious employment of this practice will outweigh all the possible benefits of its right use?

Most obstetricians apply the same forceps in all cases. When I was in the Rotunda Hospital, Dr. Denham's straight forceps was invariably used, and, at present, Dr. Barnes's curved forceps is as constantly preferred. This exclusive reliance on one instrument is, I think, a great cause of the differences of opinion as to the uses and safety of the forceps. For, under this name, two different instruments are frequently classed together; and there can be no useful comparison between distinct mechanical powers, such as the long double-curved forceps, which is a powerful lever and compressor, but a feeble tractor, and the short straight forceps, which is a tractor of great force in proportion to its size, though a weak lever or compressor.

In operative midwifery, as in any mechanical problem, it is obvious that there should be a due proportion between the power used and the resistance to be overcome, and that the force employed should

be the minimum necessary to accomplish the desired effect. Thus, a steam hammer, capable of fracturing the strongest bar of iron, can be so deftly managed by a trained mechanic as to crack a walnut without breaking it; and we have recently seen that a skilled natator can use his life-preserving apparatus to traverse a wide and angry sea in safety; but yet, without in any way under-estimating the value of either invention for their proper purposes, none doubt that the shell might be cracked or the Straits of Dover crossed with greater certainty and greater ease by less heroic means. So it is with the long and short forceps; and though under exceptionally favourable circumstances the former may be used as a substitute for the latter, under ordinary conditions and in ordinary hands the latter is unquestionably far safer, as well as in most cases more applicable.

I have endeavoured to carry out these views in the two instruments now shown to the Society, and which have been tested by extensive use during the last few years. The first is a very short straight forceps. This weighs only 8 ounces, and is 10 inches in length, of which 6 inches are occupied by the blades, the curvature of which is very gradual. They are fenestrated throughout, so that, when applied, the child's scalp may protrude and cover the rims, thus protecting the maternal passages from any contact with the instrument during extraction. Immediately above the lock is a ring for the finger of the operator. The greatest space between the blades, when closed, is  $2\frac{7}{8}$  inches, and between the points  $1\frac{1}{4}$  inch. This instrument is most portable, is easily applied, and fits the child's head better than the ordinary forceps. It possesses little power as a lever or compressor, but is a very efficient extractor, and therefore may be used in nine-tenths of the cases in which any instrumental assistance is required during labour. The most common cause of delay in the second stage of labour is inertia of the uterus, requiring but a little aid to supplement the inefficiency of the natural *vis a tergo*, and it is for such cases that the short forceps is specially adapted.

It is unnecessary to enlarge on the expediency of affording timely and judicious assistance whenever the second stage of labour is unduly prolonged, or to speak of the ill effects of such delay, the protracted sufferings of the patient, and the subsequent danger of inflammation of the soft parts, of exhaustion or of post-partum hemorrhage, as well as the possible risk to the child, which may result from leaving a woman for many consecutive hours in pain and anxiety on the very verge of delivery, when this might be easily and safely accomplished with the assistance of the short forceps. No other motives, however, should ever induce us to interfere with the course of labour, nor should any question of our own convenience be suffered to influence our judgment. No rule as to the time which a patient should be suffered to remain in labour before instrumental assistance is resorted to is of the least value; for one woman may suffer more from an hour's delay in the second stage than another would from six hours. The rule should, therefore, be to effect delivery by



art whenever any danger to either the mother or to the child is likely to result from further protraction of the labour. And even then, unless the danger is urgent, the forceps should not be applied until a trial has been given to other means likely to stimulate the natural efforts to effect delivery, such as friction over the uterus, stimulating enemata, and a dose of ergot.

If such caution is useful with regard to the use of even the short straight forceps—the application of which, in the second stage of labour, when the os has been some time fully dilated, and the head is low down in the pelvis, is, with due care, a simple, safe, and easy operation—how much more necessary is it with regard to the instrument I now place before you ! This is a double-curved long forceps, somewhat formidable-looking, but of great power, and intended to effect the delivery of living children in cases in which this could not be accomplished by any other forceps.

This instrument, which I exhibited at a meeting of the British Medical Association in London two years ago, being designed to obviate the use of the perforator or cephalotribe, is necessarily of great strength and size. It weighs about 26 ounces, and is 18 inches in length, the blades 10 inches long, the fenestrated portion being 17, and the shanks 3 inches. The widest space between the blades, when closed, is  $2\frac{3}{4}$  inches, and between the points  $1\frac{1}{4}$  inch. The handles being moveable, the instrument may be applied in the ordinary obstetric position ; or when a greater degree of compressing power and leverage is required, the handles may be adjusted, and then it can only be employed by placing the patient on her back. To these handles may be affixed a screw, somewhat like that of the cephalotribe, by which the amount of compression exerted on the child's head can be exactly regulated. Strong shoulders are also affixed below the loops to increase the traction power of the instrument. The blades are very gradually curved, and thus, when applied, the pressure is more equally distributed over the child's head, so that the instrument is less liable to slip than other forceps.

This instrument is fortunately seldom required, for the class for which it is specially designed are happily comparatively rare, as it is intended mainly for the purpose of preventing the use of embryotomic instruments, and also for certain cases of complex labour, in which version cannot be readily accomplished before the natural termination of the first stage of labour, but in which immediate delivery is essential for the safety of the mother or of the child.

It need hardly be observed that an instrument of such great power, however useful when necessarily and judiciously used, cannot be improperly or needlessly resorted to without grave risk.

There can be no doubt of the compressing power of the forceps. Nor is it necessary to refer to the experiments of Baudelocque to demonstrate what every case of natural labour proves—*i.e.*, the extraordinary plasticity of the fetal head ; and it is unquestionable that it is possible by art to assist the natural moulding process by which the

child's head is forced through the pelvis. This assistance, when absolutely necessary, may be given by the instrument under consideration, and even a very considerable degree of disproportion may be overcome by the compressing power of this forceps—provided always that it be most gently and gradually applied in careful imitation of natural labour. I have thus, or by version, in several instances safely extracted living children from women who had, in their previous confinements, been delivered by craniotomy or cephalotripsy on account of some pelvic deformity.

As these forceps differ somewhat from those in general use, I may here reiterate a few suggestions as to the manner of applying them. The rectum and bladder being first emptied, the operator should make an examination to ascertain the exact position of the child's head. Then, if the short forceps is used, placing the patient on her left side, with her hips projecting over the edge of the bed, he should sit down opposite the perineum, and taking the upper or pubic blade, previously warmed and oiled, in his left hand, he should gently insinuate it between the two first fingers of his right hand and the child's head, until the fenestrum is well over the ear, and the lock rests against the perineum. In like manner the sacral or lower blade is now to be introduced, the operator merely reversing the previous position of his hands. As soon as the locking is effected, which with this instrument is peculiarly easy, and requires no force, the operator introduces his right index finger into the ring already described, and very gently draws the head in the axis of the pelvis, at first downwards and backwards, and then downwards and forwards, until the vertex protrudes through the vulva, when the blades are unlocked and withdrawn, so as to avoid any possibility of lacerating the perineum, and the child is helped out by manual pressure from the coccyx forwards over the perineum.

The long double-curved forceps is to be used nearly in the same manner, except that, whenever practicable, it should be applied with the patient lying in the supine position, and drawn down to the end of the bed, with her legs flexed on the body, as though she were about to undergo lithotomy. As very few patients in this country will submit to be so placed, however, this instrument may be applied as the short forceps, bearing in mind that whatever situation the child's head may be in, the position of the blades of the long forceps must correspond with that of the transverse diameter of the brim of the pelvis. Another point of difference is, that this being a compressing instrument, the handles must not be suddenly or forcibly closed. In the case of a normal fetal head at full term, they should remain a full inch apart. When further compression is absolutely necessary to accomplish delivery, this may be cautiously and gradually applied by the screw already described, turn by turn, until the blades are sufficiently approximated to allow of their being slowly drawn down through the pelvis. In this operation it must never be lost sight of that these blades include in their iron grasp the fragile head

of a living child, to which any hidden, violent, or excessive compression would prove destructive, but which may, within certain limits, be safely assisted in that gradual moulding and elongation necessary to effect its passage through the pelvis.

---

## Obstetric Summary.

### *Injuries to the Fetus in Breech Presentations.*

Dr. Carl Ruge (in *Bull. Gén. de Thérapeutique*, August, 1875) has collected a number of instances where various accidents occurred, both as the result of turning and delivery by the breech.

He gives forty-four instances of lesions produced in extraction after the version, comprising fractures of the humerus, femur, clavicle, parietal, occipital, and lower maxilla; rupture between the cervical and dorsal *vertebræ*, of various muscles, as the sterno, mastoid, pectoral, &c., and also of the longitudinal sinus; effusion of blood into the abdomen, muscles of the chest, &c.; separation of epiphyses, as between the occipital and basilar process, clavicle, humerus, maxilla, tibia, &c.

In the 2nd table, lesions produced in breech presentations; twenty-nine instances are given where fracture of the frontal bone, femur, clavicle, and other accidents similar to those observed in the former group occurred. The author points out the importance of combining the three methods—expression of the head by external pressure above, proper position, and moderate traction on the trunk.

---

### *On the Obstetric Forceps as a Time-Saver.*

Dr. G. F. Francis read a Paper on this subject before the Massachusetts Medical Society, showing that lapse of time in the second stage of labour brings increasing danger; that the forceps are not necessarily a dangerous instrument; and, finally, that the experience of those who have used the forceps freely, to save time, and avoid danger, is by no means unsatisfactory.

---

## Gynecic Summary.

### *On Complete Atresia of the Female Genital Organs, or Unilateral Hematometra.*

Dr. Albert Vulch (in *Ann. de Gynékol.*, August, 1875) brings his observations to a conclusion, relating in detail the particulars of twelve cases.

Twenty-eight cases collected from various sources are tabulated, eight of them proving fatal following operation, chiefly from peritonitis.

Success, according to the author, depends less upon the mode of operation than upon the condition of the patient at the time of operation. In eleven instances, the obstruction was at one of the uterine orifices, and in twenty-three cases in the vagina.

In two cases, the uterus was bilocular, or partitioned; the others were examples of bicorned uterus in different degrees.

The phenomena of retention began sometimes before, during, or later than the appearance of menstruation, and consisted in pains in the back and sacrum, pressure internally, and in the appearance of a tumour occupying one or other side of the hypogastrium and extending into the pelvis, interfering with locomotion, and causing dysuria, and even retention of urine.

The tumour was elastic, more or less voluminous, and painful on pressure, moderately resistant, sometimes clearly fluctuating, limited above, inclining to the side, and losing itself below in the pelvis proper. Impulse was communicated by pressure above the pubis to the finger inserted per vaginam, and vice versâ.

Where the diagnosis was doubtful, an exploratory puncture cleared it up.

The modes of termination are identical with those of simple atresia—spontaneous rupture of the obstruction, perforation of the intermediate partition between the two horns, passage of the blood through the Fallopian tube into the peritoneal cavity; on account of the mortality—eight deaths in twenty-six operations—great precautions are necessary. About the end of the week following menstruation is the most favourable time, a free incision being preferred to simple puncture; no pressure upon the tumour or prolonged examination subsequently should be encouraged for fear of peritonitis ensuing.

#### *On the Treatment of Fissure of the Anus by Chloral.*

Dr. Crequy, in a letter to the *Bull. Gén. de Thérapeutique*, mentions two cases in which he had employed it with advantage. A solution of chloral (one in fifty) was inserted on charpie, just within the anus, daily, for a fortnight, when convalescence was complete. The painful defecation gradually abated after the first few days, and the patients were enabled to continue their work.

#### *Chlorosis.*

Dr. E. Frankel writes upon the combination of chlorosis with aplasia of the female genital organs (*Archiv f. Gynæk.*, Bd. viii. Heft 3). He gives the following as his conclusions:—1. That chlorosis and defective development of the female genital organs is



frequently combined, though not without exceptions, with a dwarfish condition of the heart and aorta. 2. The influence which the sexual organs exercise upon the body may also determine the character of the blood: that is to say, that sexual aplasia may be the prime mover of chlorosis. 3. That form of chlorosis which is not associated with defective development of the vascular system is permanently curable. 4. The so-called menorrhagic form of chlorosis occurs as well in defective as in excessive development of the sexual organs.

---

## *Pediatric Summary.*

### *Pulmonary Apoplexy in an Infant.*

Monsieur Budin (*Ann. de Gynékol.*, August, 1875) relates an instance where an infant, born naturally, on the third day showed symptoms of edema of the legs, which were quite cyanose; nothing abnormal was discovered on examining the chest. The infant succumbed on the fifth day. The two lungs were found to be the seat of multiple hemorrhagic infiltrations—no other lesion of any organ was detected—no cause could be assigned.

---

### *Absorption of Medicaments by the Milk.*

Dr. Lewald (in *Lyon Médical*, June, 1875) relates some experiments on animals, showing that iron, bismuth, iodine and its compounds, arsenic, lead, zinc, antimony, and mercury, are eliminated by the milk, and can thus influence the offspring. The time required before the detection of the various substances differed.

“It is not demonstrated that alcohol and the narcotics are eliminated by the milk” will hardly be endorsed by those who have had much to do with the diseases of infancy.

---

### *Rare Case of Vaccination.*

Dr. A. Muller reports a case (*Gazette Obstétricale*, Sept. 1875) of a child, aged thirteen months, vaccinated on the 9th July. At the time it was perfectly healthy. Two days later measles declared itself. All trace of the vaccination totally disappeared. On July 17th, eight days after the date of vaccination, when the symptoms of measles were disappearing, an inflammatory areola was discerned round the points where vaccination had been performed, and from this date the progress of the vaccination was normal.

---

*On the Development of the Graafian Follicles in the Ovaries of  
Newly-Born Infants.*

M. de Sinety has communicated to the Biological Society of Paris some interesting researches, showing that the Graafian follicles of infants often take on a considerable development, and that in young girls before puberty cicatrices resulting from ruptured follicles are found.

In infants the follicles are visible to the naked eye, the most developed ones being always situated in the region of the hilus.

*On the Treatment of Chorea by Ether Spray and Ice to the  
Vertebral Column.*

Dr. Fabry, in his Inaugural Thesis, relates the experience of Dr. Perrond, who successfully tried this method. The spray was applied for an interval of from four to eight minutes, thrice daily at first, then only twice, along the spine. The ice was continued for five minutes.

## News.

### SHEFFIELD HOSPITAL FOR WOMEN.

At a meeting of the Building Committee of the Sheffield Hospital for Women, held to receive the various tenders for the erection of the new premises, the expense of which is to be generously borne by Mr. Thomas Jessop, twelve tenders were presented. Mr. Jessop intimated his desire that the tender of Messrs. John Chambers and Sons, Commercial Street, for 16,250*l.*, should be accepted, and the Committee, of course, agreed to do so. This sum does not include the expense of heating, furnishing, &c., which the donor of the building also intends to bear. The total cost of the works will be about 21,000*l.*

The new building will be erected on the site at the corner of Leavygreave and Gell Street, Brookhill, known as Brooklyn House. Its architectural features, which are Gothic in character, will be imposing and effective. It will face on the west towards Gell Street, on the south towards Leavygreave Road, and on the east towards Victoria Street. The principal entrance will be in Leavygreave Road, and will be surmounted by a square tower, with slated roof. The hospital will be in two distinct blocks under one roof. The department for midwifery will face towards Victoria Street; the out-patients' department will front to Gell Street. On entering the hospital, out-patients will pass into a waiting-room, the dimensions of which are

24 feet by 24 feet 6 inches; they will then be received in the consulting-room (14 feet 9 inches by 14 feet 6 inches), and after procuring their medicine at the surgery will leave the building by another way, specially intended for the exit of this class of patients. The total number of beds to be provided is 35, or three times the utmost capacity of the present building in Figtree Lane. On the first floor are two large wards, which will hold five beds each, and two smaller wards to hold two beds each, with separate bath, closet, and lavatory. In the midwifery department are two wards, with separate baths and lavatory. The larger wards will be 24 feet by 24 feet 6 inches, and the smaller wards 21 feet by 14 feet. The second floor is a counterpart of the first. Every possible provision is made for the health of the inmates, 2000 cubic feet of air being calculated for each patient. The boardroom, which is on the right of the principal entrance, is 25 feet by 19 feet; and all the arrangements for kitchen, larder, scullery, and servants' accommodation are on the most liberal scale. The width of the main staircase will be 16 feet, and of the principal corridor 10 feet 6 inches. The edifice will be erected in brick, with stone dressings; and the work will be commenced forthwith.

---

#### BOOKS, PAMPHLETS, AND PAPERS RECEIVED.

"Cyclopædia of the Practice of Medicine." Edited by Dr. H. Von Ziemssen. Vol. X. "Diseases of the Female Sexual Organs." By Prof. Carl Schroeder. London. Sampson, Low and Co. 1875. Pp. 575.

"Lectures on Nursing." By William Robert Smith. London. J. and A. Churchill. 1875. Pp. 228.

"Clinical and Physiological Researches on the Nervous System." No. I. "On the Localization of Movements in the Brain." By J. Hughlings Jackson, M.D. J. and A. Churchill. 1875. Pp. 37.

Communications have been received from Dr. J. Barnsfather, Cincinnati; Dr. Lloyd Roberts, Dr. Grigg, Dr. Bantock, Dr. John Williams, Dr. Edis, Dr. Henry, Dr. Galabin, &c.

---

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.

THE  
OBSTETRICAL JOURNAL  
OF  
GREAT BRITAIN AND IRELAND.

~~~~~  
No. XXXIII.—DECEMBER, 1875.  
~~~~~

Original Communications.

---

ON THE DIAGNOSIS AND TREATMENT OF  
THE CURABLE FORMS OF FIBROID  
TUMOURS OF THE UTERUS.\*

By ALFRED MEADOWS, M.D. Lond., F.R.C.P.

Physician-Accoucheur to, and Lecturer on Midwifery at St. Mary's Hospital.

THERE is a trite saying in very common use, which refers to a practice of almost universal application, and yet which, when we come to apply it practically, no two probably of those now present would, in any given number of cases, apply it in exactly the same manner. I refer to the practice involved in the saying that "we must draw the line somewhere." Let us take an illustration. We are now pretty well agreed, I suppose, that there is great convenience, both as regards the public and the Profession, in the division of the practice of medicine into the three great departments of Medicine, Obstetric Medicine, and Surgery; and yet when we come to "draw the line" between these three great departments, we experience in a very striking manner the difficulty to which I have referred, and the attempt to do so would probably illustrate, in a very forcible way, the state-

---

\* Read at the Harveian Society, November, 1875.



ment I just now made—viz., that no two of those now present would probably “draw the line” in exactly the same way. Upon this fact, however, if it be a fact, I rely to a great extent as justifying my hope that the subject which I have chosen for our consideration to-night may be, if not equally or universally, at least very generally interesting, because I find in practice that wherever physicians, obstetric physicians, and surgeons may “draw the line,” theoretically as to the limit of their respective departments of practice, they at least seem *not* to draw it so as to exclude the uterus from their domain. I do not of course complain of this as an unwarrantable intrusion on the part of my friends the physicians and surgeons. On the contrary, I am proud to think, that, what the common people regard as my *spécialité*, has so many charms for my brethren of the other departments—that they derive special pleasure, and I hope profit, in poaching upon this manor; and I trust that one result of this will be an increase to-night to the knowledge which I have of the subject under consideration, seeing that all departments of practice are here so well represented, and that we may have a vastly more interesting and varied discussion than if only obstetricians were present.

The subject of my paper is “The Diagnosis and Treatment of the Curable Forms of Fibroid Tumours of the Uterus.” I do not intend—for the time would fail me if I made the attempt—to discuss all the modes of treatment applicable to the many varieties of uterine fibroids. A large number of these are, I believe, hopelessly beyond the reach either of medicine or surgery; many of them are, I am equally persuaded, quite amenable to one or other method of treatment, and I propose to-night to give a *résumé* of my experience in this department, not in an exhaustive manner, nor illustrated by any array of statistics—this is neither the time nor the place for that—but I wish to state frankly my opinion, in such a manner as will, I think, best serve to elicit a discussion on the many most interesting questions connected with this subject.

Now, it is impossible to consider this question in a practical manner unless we first define accurately the basis of our

discussion, and for this purpose we must at the outset recognise the several varieties of uterine fibroids *in their anatomical relation to the uterus itself*. This is the very essence of the discussion, for to talk generally about the treatment of a fibroid tumour of the uterus, without any reference to its seat, would be the wildest and most profitless of subjects. Hence, to be practical, we must not only specify in our discussion the exact situation of the tumour which is being, or to be treated, but we must also for the purpose in question be able accurately to diagnose this relationship. It is to this point, I think, that the attention of clinical observers is especially needed. We want carefully observed and recorded histories, together with accurate post-mortem details as to the situation and connexion of these growths. My own reading on this branch of the subject leads to the conclusion that only the vaguest ideas prevail, even in the minds of great authorities. Yet there is very much that can be stated with tolerable certainty, and in many cases I am sure that an accurate diagnosis on the question of situation can be secured. It is the more important to be precise on this point, because in my opinion the whole question of treatment, both medical and surgical, depends in great part, if not entirely, upon it. There are, for instance, some tumours which are so placed that nothing whatever can, in my judgment, be done for them, either medically or surgically, at least with a view to *cure*, and it is in its *curative* aspect especially that I am considering the subject to-night. Others, again, are so placed that their removal by operation or otherwise, is, comparatively speaking, easy, and little if at all dangerous. Then, again, there is a middle group, where removal of the tumour can probably be effected, though certainly not without risk; but I believe future experience will show that these tumours are far more amenable to treatment than has hitherto been supposed.

It is, then, of the very utmost importance that we should be able to diagnose accurately the situation of any given tumour in regard to the uterus itself. And upon this point, in giving you the result of my own observation, I ask for any contribution in the way of enlightenment which the

experience of those present can afford. I am satisfied that fuller and more trustworthy knowledge on this point must be obtained before we can hope to make much advance in the treatment of this disease.

Now, in studying the clinical history of fibroid tumours of the uterus, the first point which strikes us is the great dissimilarity which exists in regard to two of their most leading features—viz., hemorrhage and pain. In some cases, there is almost no pain whatever experienced; these patients express themselves as having absolutely no suffering at all; and if it were not that they lose, either periodically or at no regularly stated intervals, large quantities of blood, together with, during the intervals of the hemorrhagic attacks, considerable amounts of mucous discharge, they would have really nothing to complain of. Yet, on examination instituted because of those losses, we find them the subjects of large uterine fibroid tumours.

On the other hand, patients sometimes come to us making no complaint of any undue loss of blood, or of any irregular discharge, mucous or otherwise, and showing no evidence of such a drain in a whitened face or pallid complexion; but complaining bitterly of much local distress, of great abdominal and pelvic pain, and possibly even of considerable abdominal enlargement. Here, also, we find on examination instituted for the purpose of discovering the cause of this suffering, that the patient is the subject of a large uterine fibroid. The great difference, however, in the clinical history of these two cases indicates, and is the result of, a radical difference, not in the nature, but in the seat of the tumour in the two cases. For in the one case the tumour proves to be extra-uterine; in the other it is intra-uterine; in the one it has originated on, and grown beyond, the peritoneal or serous surface, while in the other, it began near and pushed forward the mucous surface, developing in, and considerably encroaching upon the uterine cavity; hence, in the latter case, blood has been attracted to, and been freely poured out upon, the mucous surface of the organ, together with a good deal of mucus and muco-sanguinolent discharge. In the former case there has been no such cause and no such result; but the peritoneal

surface has been considerably stretched, other neighbouring viscera have been hardly pressed upon, sensitive parts have been mutually interfered with, and pain and suffering have been the characteristic results.

These are the respective phenomena which I have clinically associated with these two classes of cases. Taking typical cases, I have found that these two symptoms, hemorrhage and pain, which are *the symptoms par excellence* of fibroid tumours of the uterus, are inversely proportioned the one to the other, and bear a fair relation to, and may therefore be regarded as pathognomonic of the situation of the tumour in regard to its peritoneal and mucous surfaces. That is to say, the more peritoneal or subserous the tumour is, the more is *pain* the predominating symptom; and, on the other hand, the more submucous or intra-uterine the growth is, the greater is the hemorrhage and the mucous, or mucosanguineous discharge. Hence, apart altogether from any physical examination, internal or external, the clinical history of the case suggests as it were a preliminary diagnosis, which further examination will only tend to confirm. Of course there are cases where, either from constitutional peculiarity or from some hematic condition, or from ovarian influence, or from past history as regards the generative function—frequent childbearing, and the like—the menstruation is excessive, even though there were no growths; and this would certainly influence that function, in regard to the amount of discharge, when a growth such as a fibroid tumour exists in the uterus. But, apart from these conditions, the broad general rule which I have here laid down will in the main be found correct, and may serve as a useful guide in establishing the diagnosis; not, perhaps, so much as to the existence of such a tumour, as to the location of it upon one or other of the surfaces of the uterus—the mucous or serous.

Let us now suppose that suspicion has been aroused as to the existence of a uterine fibroid in any given case, and that careful examination has established the fact that this suspicion is well grounded. The next point which we shall have to consider in practice is the proper mode of treatment



to be adopted in that particular case, especially with reference to its curability, by removal or otherwise. This, it need hardly be said, is a question of the utmost importance, and my experience has taught me certain rules which are my guide in cases of this sort ; it is these which I now proceed to describe, and which I may add are the result of my own personal experience. Time would not permit me, nor is it necessary for the elucidation of the particular branch of the subject which I have chosen for our consideration this evening, to discuss in detail all the points upon which rest the diagnosis of a fibroid tumour of the uterus, or its differentiation from the many other morbid growths of the pelvic or abdominal viscera. This is in itself a most interesting and difficult subject, as the record of the many mistakes which have occurred in the practice of the most experienced and most skilful testifies—mistakes, to the number of which I have myself contributed, and which those who have had the largest experience will be the most willing to condone. Our attention this evening is, however, limited not to the diagnosis of uterine fibroids generally, but to the curable forms of these growths in particular, and to the treatment by which that cure is to be effected.

Now the curability of a fibroid tumour of the uterus will in the main depend upon its position, *quoad* the uterine walls ; for instance, if the growth has originated in the wall of the uterus immediately under its mucous surface, and has grown into the uterine cavity, either in a sessile or pedunculate form, then we say, and say truly, that it is distinctly of the curable class ; if, on the other hand, the growth began near the peritoneal surface, and as it grew projected more and more into the abdominal or peritoneal cavity, then we say, though not perhaps with equal truth, that it is of the incurable class—I say, not perhaps with equal truth, because undoubtedly some of these cases are curable, though only by the terribly formidable operation of abdominal section, and only then if the tumour be of the more or less pedunculate variety of the subperitoneal class. I shall have a few words to say presently of the possible diagnosis of this form of growth. So then, the diagnosis of the curable forms of

uterine fibroid tumours resolves itself into the diagnosis of the exact situation of the tumour in regard to the uterine wall ; and in endeavouring to elucidate this by vaginal examination, special attention should be paid to the following points. First, the degree and direction of cervical displacement revealed by digital examination. Secondly, changes in the os and cervix—as regards the patency of the former, and as regards size and consistence of the latter. Thirdly, in the use of the uterine sound, note the exact dimensions, and, as far as possible, the shape of the uterine cavity, especially as to the apparent existence of any foreign body in it, and generally as to the direction which the sound takes ; lastly, if the tumour be sufficiently large to be felt externally, observe while the sound is in utero whether, and to what extent, movement of the tumour by external palpation affects the handle of the sound which is beyond the external parts.

I attach importance to these several points for the following reasons :—

First, with regard to the degree and direction of cervical displacement, it will be found that displacement in any given direction indicates a growth in the opposite direction. Thus, a tumour in the posterior wall displaces the cervix forwards, and so on with regard to other situations, the degree being governed by the size of the growth ; so that the situation of the cervix is a key to the position of the tumour, and it is a point which should first arrest attention, because then the subsequent examination will be directed as it were to the true seat of disease. A tumour which is entirely within the uterus, being central, does not displace the cervix in any direction ; hence, if the cervix be normally placed, the tumour is probably an intra-uterine polypus.

Secondly, as regards the patency of the os, and the size and consistence of the cervix, most valuable information as to the curability of the case will be obtained by attention to these points ; a closed os, with a small, perhaps normal-sized cervix, and with no abnormal change of its consistence, are, in my experience, almost fatal indications of incurability. I am not of course now speaking of intra-uterine polypi, for I have

often met with cases of this sort, the tumour being entirely intra-uterine, possibly attached to the fundus, and the os and cervix being scarcely, if at all, changed ; such cases may be dilated, and the tumour removed without any difficulty. But the case is far otherwise where a uterine fibroid exists with the os and cervix such as I have described, and the larger the growth and the less the changes in the os and cervix, the more hopeless, in my opinion, the case is. On the other hand, if the os be patent, that is a good sign in favour of easy and successful removal, for it indicates growth towards the uterine cavity, and this point will be brought out all the stronger the more the tumour invades the lower segment of the uterus, and of course the lower the tumour the easier will be its removal ; the incurable forms of uterine fibroids seldom or never expand the os. This is pre-eminently seen in the case of purely sub-peritoneal growths ; so that a dilated os is *pro tanto* a favourable indication of a curable case, and the converse is, as a rule, almost equally true.

As regards the size and consistence of the cervix, I should say that the larger the cervix the more favourable is the case ; and, conversely, the smaller the cervix, the more difficult, if not incurable, the case is—a small healthy cervix almost defies interference, while a large one invites it. In reference to this point, special attention should also be paid to the exact situation of the morbid growth. In the case of uterine fibroid, the *entire* cervix is seldom or never invaded ; it is usually one or other lip, and, in my opinion, the posterior is vastly more commonly affected than the anterior. Then, as to consistence, the softer, more fleshy, almost tense and elastic, the cervix is, the more likely is the tumour removable ; while a hard, almost cartilaginous cervix indicates more a state of fibroid infiltration or degeneration than an actual fibroid tumour existing as a distinct growth, enclosed in its own cellular capsule ; such a condition is, in my judgment, incurable, and had far better be left alone. Sometimes, by a very minute and careful examination, we can feel quite distinctly through a more or less thin layer of proper cervical tissue the firm fibroid tumour imbedded in the uterine sub-

stance. This is always a good sign. I have met with it several times, and the cases all proved satisfactory in regard to the facility with which the tumour could be enucleated. Great care is necessary to distinguish cases of this sort from those where the tumour grows from the posterior wall of the uterus, so as to occupy Douglas's pouch and to press upon the posterior cul-de-sac. Here the sensation of feeling the tumour through the tense posterior vaginal wall very much resembles the feeling already described.

The difficulty of diagnosing the exact situation of the tumour when it is in the anterior wall is certainly greater than when in the posterior wall; but, happily, the former are much less frequently met with than the latter, and they are undoubtedly, at least according to my experience, more dangerous, and far more likely to be followed by inflammation after any attempt at surgical interference. I believe this is due to the existence of the cellular tissue between the cervix and the bladder, and therefore to the greater liability to pelvic cellulitis.

Thirdly, as to the use of the sound. No one who has had much experience can doubt its value in diagnosing the exact situation of a uterine fibroid. The first point to note in its use is the length of the uterine cavity which it measures; attention to this point alone will, if the tumour be of any size, go far probably to determine the question as to curability or otherwise by operative interference; for, whereas a sub-peritoneal tumour need not, and very commonly does not, add anything to the length of the uterine cavity, even though the tumour attain very considerable size, an intra-mural, and still more an intra-uterine, growth will increase the length of the uterus in exact proportion to the size of the tumour; so that increased length of the organ not only indicates the presence of a tumour, but to some extent also declares its size and the possibility of its removal. Of course we may get elongation of the uterine cavity, and to almost any extent, without a tumour of that organ, as in hypertrophic elongation of the cervix, sub-involution, &c.; but here the symptoms which call for treatment will be very dissimilar, and the other physical signs will vary also.



Uterine enlargement is not, however, the only fact which the sound reveals in cases of this sort. Alterations in the shape of the uterine cavity can also be made out by careful examination, and where the tumour encroaches upon the uterine cavity, we can often note the fact by observing that the direction which the sound takes indicates its passage over a rounded body. And further, in the use of the sound we should regard it as a kind of elongated index finger, and work it in conjunction with that of the other hand ; in this way, with the sound in utero, and one finger in the anterior part of the vagina, we can observe the distance between the finger and sound, and thus diagnose whether the tumour be in the anterior or posterior wall ; for in the former case, if the finger be in the anterior part of the vagina, the thickness will be considerable ; in the latter, there will be only the uterine wall separating the finger from the sound. Lastly, as a corroborative means of diagnosis, if the tumour be sufficiently large to be affected by external palpation, then while the sound is in utero we should observe the extent to which movement of the tumour externally affects the handle of the sound *per vaginam*, for the more the tumour is *in* the uterus—wall or cavity—the more readily and freely will the sound move by any movement of the tumour ; whereas in the case of a sub-peritoneal growth we can often move the tumour about freely without much effect upon the uterus, and therefore without influencing the sound while in its cavity.

These, then, are, in the main, the several points upon which I rely, not only for the diagnosis of a uterine fibroid, but also for its location in a particular part of the uterus, and therefore for determining the question whether or no in any given case the tumour is removable. We may now consider the several modes of effecting this removal—in other words, the treatment of the curable forms of fibroid tumours of the uterus—and the first question that necessarily occurs is, what is the value of medicines for this purpose ? I may say at once with reference to the whole class of so-called absorbent drugs—such as iodine, bromine, and their compounds, mercury, &c.—whether applied externally or administered internally that I believe them to be not only utterly inert

for the purpose in question, but I regard them often as positively hurtful to the general health : remedies of this kind may possibly be helpful in promoting the absorption of inflammatory products, and as they act through the absorbent system they may also help in the removal of some glandular enlargements ; but they are, in my judgment, perfectly inert in regard to the absorption of distinctly new growths.

There are, however, some drugs which experience has taught me possess a very definite value in the curative treatment of these tumours. They are the whole class of so-called oxytoxic agents, of which of course ergot is the chief, though not by any means the sole representative. I speak of them as a class, premising that I regard ergot as *par excellence* the drug which is capable of exerting the greatest influence in this direction. Quinine, cinnamon, borax, &c. are all agents of the same kind, and they act no doubt partly by exciting contraction through the vaso-motor nerves of the minute involuntary or unstriated muscular fibres in the arterial vessels—thus not only limiting the supply of blood to the tumour, and so preventing its growth, but also, by cutting off the supply, they reduce the growth by the slow process of starvation ; no doubt, *cæteris paribus*, this action is more likely to be efficacious the smaller the growth is. So also, tumours which being intra-mural are well surrounded by uterine tissue, are more amenable to this treatment, because in like manner these drugs act upon the involuntary unstriated muscular fibres of which the uterus itself is composed ; and when these fibres completely surround the growth, a kind of steady, persistent contraction is maintained, which is productive, I am sure, of the happiest results—considerable experience has satisfied me on this head. I cannot say that I have actually seen tumours of this sort entirely disappear, but I have certainly seen them reduced considerably in size, and become practically inert. For the success of this treatment, however, much will undoubtedly depend on the situation and character of the growth ; the most satisfactory results which I have obtained have been in cases where the tumour was small, where it was of the softer, laxer, and, as it were, semi-edematous kind, and where it was so placed as to be well acted upon by the

uterine fibres. If, then, success is to be achieved from the administration of ergot, there ought, in accordance with the rules I have laid down for the diagnosis of the situation of these growths, to be, in the first place, plenty of hemorrhage or other discharge; and secondly, some increase in the size of the uterus, with elongation of its cavity, in accordance with and in direct proportion to the size of the tumour.

As to the mode of exhibition, I have tried ergot both subcutaneously; by the stomach; and locally, *per vaginam*, in the form of pessary—in all of which I have had good results as regards the production of uterine action; but I am inclined to think that the subcutaneous administration of ergotine is most effective for the purpose in question. Unfortunately, however, it very commonly happens that so much local irritation is caused by the injection, that lymphatic inflammation, diffuse cellulitis, and even abscess, have resulted. On the other hand, as regards the local application by ergotine pessary, there are obvious objections to this as a matter of sentiment and personal dislike, besides the fact that the results are certainly feebler; but it may be cited in favour of this method of treatment that it avoids the evils of the subcutaneous method, and it does not occasion the nausea and vomiting which too frequently follow the exhibition of ergot by the stomach. For prolonged treatment, however, and where the drug is favourably received, the latter is undoubtedly the mode which will be found most convenient; and as regards the kind of preparation, my experience has taught me that there is nothing equal to the old powder recently ground and made into a decoction. It is, I admit, very nauseous, and therefore not always tolerated; in such cases the liquid extract is the best. The dose is fifteen to thirty grains, two or three times a day, when the stomach is empty; and this I have given for many months without any evil effects. I believe it is useless to expect any improvement, short of continuous treatment, for three, six, nine, and even twelve months.

There is one other drug about which a few words must be said—viz., the chloride of calcium, in the form of liquor calcii chloridi of the Dublin Pharmacopœia. Dr. Matthews

Duncan appears to believe that this drug possesses the power of completely removing these fibroid tumours. I must confess I have never been fortunate enough to secure such a result, even after the lapse of twelve months' steady, persevering treatment. My experience of this drug, however, leads to this conclusion, that if the tumour does not disappear, its growth is, occasionally at least, arrested; that still more frequently it seems to effect some change in the tumour or its surroundings, by which the hemorrhage or other discharge is controlled, and even prevented. These are no doubt important results, which fully justify the use of this drug; but perseverance with it becomes a fault if other and more important and heroic measures which would effect a cure are thereby excluded.

It is not my present purpose to treat of the merely palliative or symptomatic treatment of these cases; I have, therefore, nothing further to say with regard to the medicinal or drug treatment of uterine fibroids, because I have no confidence in any other agents than those already referred to.

We come now to consider the curative treatment of these tumours by surgical interference. It may, I suppose, be regarded as an axiom in the management of these cases, that the risk of interference is in exact proportion to its difficulties, and success will in the main depend upon facility of operation. A tumour which can be easily reached, should be easily and therefore successfully removed, provided it be a fit case for such interference; and those cases only are so which, in the first place, give rise to distressing symptoms, and, in the next, can be diagnosed with tolerable accuracy as regards their exact situation. How this latter is to be done I have endeavoured to show. As regards removal, two distinct and separate stages have to be considered—first, getting on to the tumour; second, taking it away. The former is generally simple enough; the latter may be so, or very much the reverse.

I shall dismiss with few words the curative treatment of the sub-peritoneal forms of these tumours. There is but one method which is applicable to them—viz., gastrotomy, and no one, I suppose, would be hardy enough to attempt this



unless he were pretty confident either that the tumour was attached to the uterus by a very narrow peduncle, or else that the tumour was so placed upon the fundus uteri that the body of the uterus itself could be used for the purpose of a pedicle. No doubt there are many cases in which this operation is perfectly justifiable, and I believe the cases will become much more common as time goes on, and as abdominal surgery advances to perfection. The cases truly are often distressing enough to warrant almost any interference, nor are the difficulties of the operation great in properly selected cases. I should lay it down as a guide in determining the fitness of any case for this operation, that the tumour should be well out of the pelvis, so as hardly, if at all, to be felt there per vaginam, and certainly that the cervix with a good part of the body of the uterus should be free from the disease; also that the tumour should be freely moveable in the abdomen, and that free movement of it should be possible without very much affecting the cervix. If with these conditions, and the tumour being a good large one, the uterine sound measured but a slight increase in the length of the uterus, or if elongated, the body of the uterus was not much enlarged, then I should regard the case as on these grounds very favourable for interference, and any departure therefrom would *pro tanto* lead to a contrary opinion.

Turning to the other aspect of the uterus—viz., its mucous surface, we have not generally much difficulty in deciding how to treat tumours in this situation. I exclude, of course, ordinary polypi, as I do not regard them as fibroid tumours, and moreover there is no difference of opinion as to their proper treatment. But fibroid tumours of the uterus, which are sessile upon the mucous surface, and are also imbedded in the substance of the organ, require very different management. And first as to the mode of getting on to the tumour. This we may do, either by dilatation, or by division, of the cervix. I resort to the one or the other according to the condition of the cervix itself; if that be thin, small, free from disease, and quite uninvaded by the tumour, then dilatation is unquestionably the best mode of proceeding. The

risk of inflammation—cellular or peritonitic—is less, while the amount of opening attainable is greater. On the other hand, if the cervix is large and thick, and still more if the tumour itself has invaded it, then dilatation will in all probability be useless; the cervix will not yield to the influence of any tent, and even if it did, I am satisfied from clinical observation that the dangers from liability to inflammation are far greater. When once we have reached the tumour itself, the capsule which envelopes it must be either cut through with a knife, or broken-down with the finger. I greatly prefer the latter—it is easier of application, quite as effectual for the purpose, and less liable to be followed by hemorrhage. As a general rule, in all cases of this sort, when once the capsule is pierced, the detachment of the tumour is very easy, and, if within reach, the whole growth may be shelled out, as it were, from its cellular bed. If this cannot be entirely effected, we may safely trust to ergot to accomplish our purpose.

Now, with regard to uterine fibroids which are imbedded in the substance of the uterus itself, the interstitial or intramural variety, much difference of opinion prevails as to the best mode of treating these. Some regard them as incurable, because of the risk attending their removal. Others, and I am certainly of the number, consider them very fit subjects for interference. As I have said before, the whole question depends upon our knowledge of the exact situation of the growth; but I must assume for the purpose of illustration that the tumour is upon its peritoneal side also invested with proper uterine tissue, of sufficient thickness to make removal possible without going too near the peritoneum. The difficulty of getting on to, and subsequently removing, such a tumour, will be in exact proportion to its nearness to the cervix; if the tumour is situate in the posterior wall, and is as large, say, as a fetal head at term, it will probably have invaded some part at least of the cervix; possibly it may have encroached as far as the os, and the cervical tissue will be found expanded over it, so that the tumour occupies the whole posterior half of the pelvis. Suppose now that removal has been decided upon,

we may either make an incision into the most depending part of the tumour from the os directly backwards, leaving an opening large enough to admit one or two fingers easily ; or we may thrust into the tumour an olive-shaped actual cautery, as has been suggested by Dr. Greenhalgh, and practised both by him and myself with complete success. The depth to which the cautery should penetrate must depend upon the distance of the tumour from the vaginal roof and enveloping uterine tissue, care being taken at least to penetrate through the capsule of the tumour. If hemorrhage occurs in either case, we should plug with cotton wool, wet with tannin and carbolic acid.

After either of these proceedings, we may wait for a few days, or we may at once proceed to the process of enucleation. We have now got upon the tumour, that is the first step ; its subsequent removal must be partly a work of time. But in this we can help very materially, though we trust to the natural effort not a little. Now in the very first touch at the process of enucleation, we can tell pretty plainly whether or not removal is possible. In a large number of cases the tumour lies in its uterine bed, with which it is connected merely by loose cellular tissue, and from which it can be dislodged with the utmost facility. Care must, of course, be taken that the finger is really between the tumour and the uterus, for I have heard of cases where a mistake on this point has been made, the finger being at work in Douglas's pouch outside the uterus altogether, a fatal result of course ensuing.

In some cases the process of detachment is by no means so easy ; it may indeed be found almost impossible, so firmly is the tumour united to the uterine tissue that they seem mutually blended the one into the other. In such case the attempt should be abandoned, as disaster is sure to follow any determined attempt at forcible separation in circumstances such as this. The extent to which separation is necessary varies a good deal in different cases. In some a very little suffices ; the detached portion becomes a kind of foreign body, very much like a dead ovum ; and the uterus accordingly proceeds to expel it ; distinct uterine contractions occur, and pains of a forcing,

bearing-down character are experienced ; soon it is observed that the opening we have made, either by incision or cautery, is enlarging, under the expanding influence of the *vis a tergo*, and through it the mass is beginning to protrude. This process we may greatly facilitate by daily or occasionally passing the finger through the opening, and sweeping it round between the tumour and uterus, detaching it more and more, and so completing its final separation. In all which process the natural effort of the uterus, acting upon the mass as upon a foreign body, will render most efficient aid. The more the tumour is detached, the more "foreign" it becomes, and the greater will be the effort of the uterus to effect its expulsion. I have often been surprised at the marvellous assistance which the uterus gives in this process of cure. Sometimes the whole thing is completed in a few days, so violent are the expulsive efforts, and I have observed that these efforts never occur unless the tumour is of the removable kind. No such action occurs in cases where the tumour and uterus are blended together, and where detachment is next to impossible.

In my earlier experience I was in the habit of using the *écraseur* so soon as a sufficient portion of the tumour was detached to admit of its application. I was then satisfied with making the tumour into a kind of polypus, and cutting through its midst as through a pedicle. Further experience has taught me, however, that there is no pedicle to these growths ; they are more or less rounded masses imbedded in a capsule, having no special seat of attachment, and they are as easily separated at one point as at another ; consequently, their detachment all round is the point to be aimed at, and the *écraseur* need only be used, if at all, in cases where the mass is so large that its entire removal in one operation is thereby made needlessly difficult. In the majority of cases the whole mass may be turned out and removed by a pair of strong forceps.

I have spoken chiefly of these interstitial tumours as they occur in the posterior wall. In my experience, they are much more common in that locality than elsewhere, and happily so, because, as a matter of fact, I find that the ante-



rior wall of the uterus is far less tolerant of operative interference than the posterior, and that cellulitis, with cystitis, occurs much more frequently when the anterior wall of the uterus is meddled with than the posterior. I attribute this entirely to the greater amount of cellular tissue which exists between the uterus and the bladder, than between that organ and the rectum. Nevertheless, the mode of procedure will be the same in both cases.

In the after treatment, care must be taken against three possible consequences. First, hemorrhage; this must be prevented by plugging with cotton wool, with or without an astringent—of which matico is, I think, the best—and always with a disinfectant, of which carbolic acid is decidedly to be preferred. Secondly, inflammation; against which opium must be constantly exhibited in small, frequently repeated doses, and linseed-meal poultices, with laudanum, be constantly applied to the abdomen, with a firm bandage, so as to maintain some steady pressure over the uterus. Lastly, septicemia, which can be effectually guarded against by frequent injections of tepid carbolized water, and by applying constantly a vaginal pessary containing one grain of carbolic acid. Rest, it need hardly be said, must be absolutely enjoined.

Such, then, is the plan of treatment which I have practised, and now recommend for adoption in the curative treatment of fibroid tumours of the uterus. It will be observed that I have said nothing specially about the treatment of interstitial fibroid tumours when limited to the fundus and body of the uterus, and not invading the cervix uteri. This silence is intentional, and has reference to the title of my paper, which deals only with the *curable* forms of these growths, for I doubt very much whether interstitial fundal tumours can be so regarded. I always look upon the fundus uteri as vastly more important than the cervix—higher in physiological dignity, and certainly superior in pathological importance. Hence I am convinced, and this not theoretically merely, that the attempt to enucleate a tumour from the fundus uteri will be not only a work of extreme difficulty, but will surely be accompanied by very great peril. It is

different when the tumour is simply sessile upon the mucous surface projecting entirely into the uterine cavity, and requires only to be peeled off, as it were, or partly so, and finally to be removed by the *écraseur*. This can be safely done, as I have seen again and again ; but the others I regard as practically incurable.

I must apologize for the great length to which this paper has run, but the subject is a very large one, and I feel now that it has been handled in the most sketchy and imperfect manner, which requires from me an appeal to your kind indulgence, while thanking you, as I do most heartily, for your long and patient attention.

---

## FOUR CASES OF OVARIOTOMY.

By C. H. ROUTH, M.D.

Physician to the Samaritan Free Hospital for Women, &c.

HARRIET F., aged fifty-two. Married. Residing in Fulham. Admitted under my care June 25th, '69. Occupation, needle-work. Married eleven years ; a widow fifteen. Has had five children ; two died—one, the first-born, destroyed at birth. Has never been to say strong, but seldom in a doctor's hands. Her belly began to enlarge fifteen years ago, after her husband's death, but very slowly, until one day, when on lifting up a pail of water, she felt something give way, which produced great abdominal pain. From this period belly enlarged rapidly. Growth began originally in left side, but the impression of something giving way was on right side. Since then the pain has been all round the belly ; sometimes it is very severe. To-day, 29th, is very painful. Catamenia began at thirteen. Had a good deal of trouble at that time. Occurs every four weeks, lasting five days, with a great deal of dysmenorrhea.

*Examination.*—Belly very large, measuring round umbilicus 49 inches ; below it, 46. From ensiform cartilage to pelvis measuring 19½ inches. Dull on percussion up to just below ensiform cartilage. Clear in left lumbar region ; not so clear

except low down on right side. Fluctuation irregular, varying in distinctness at different parts. The parts of tumour vary in consistence. Uterus high up, anteverted. Normal length, moveable.

Sir W. Fergusson and Dr. Greenhalgh saw this case in consultation with me. As the tumour was clearly multilocular, and probably some of the cysts contained gelatinous matter, tapping was deemed inadmissible, and ovariectomy recommended at once.

Ovariectomy was performed by me July 1, in the presence of Sir W. Fergusson, Drs. Greenhalgh, Rogers, Williams, and Day, in the usual manner, under chloroform. Incision in middle line. Tapped and removed through the opening entire. There were no adhesions; it was multilocular. The pedicle was secured by a clamp.

*July 2nd.*—A good deal of pain since operation; no sickness. Pulse 108. Pain was relieved by twenty drops of tr. opii every three hours, and a larger one of thirty drops at night.

*3rd, P.M.*—Was a little weak last night, feeling suddenly faint after taking some brandy. No pain since last injection. Pulse 96. *Evening:* More sick, but no pain except when retching. Pulse 108; weak. Cannot keep down even iced champagne. 10 P.M.: Sickness persists, resisting effervescent draughts—ice. To have beef-tea injections every three hours, with a little laudanum if in pain, not otherwise.

*4th.*—Very sick this morning (9 A.M.), with a good deal of flatus and hiccough. Wound appears united everywhere, except where the clamp is placed; lower portion somewhat tightened. No pain. Slept well last night; sweating. Pulse under 100. One or two drops of oil of amber were given to check the sickness, on a lump of sugar, but failed. In the course of the day, however, she informed me that when a child a piece of *rotten cheese* always turned sickness with her; this I therefore ordered to be given to her. I saw her at 3 and at 10 P.M. The rotten cheese *had* turned the sickness altogether. No pain. Sweating. Pulse 90. Feels very comfortable. Ordered a little arrowroot at night.

From this day this patient progressed very favourably.

She ate a chop on the 7th day after operation. Clamp came away on the 8th day. Ligatures removed, and parts cicatrized rapidly. A carbolic acid lotion only was used externally till the 25th, when chlorate of potash lotion was substituted. Left on 28th, cured.

Mrs. T., aged thirty-two, was admitted under my care March 15, '71, complaining of large abdomen ever since her last child was born, in August last. She has had two children. Has had more or less pricking pain on left side, which was that which enlarged first, until the belly assumed present dimensions. No pain, and only inconvenience when walking.

The abdomen was pyriform, large. Both lumbar regions clear, and upper part of abdomen at epigastrium. Fluctuation very evident. The vaginal examination gives first the idea of a large retroverted uterus, but the sound proves it to be in normal position and of normal length, the swelling behind being part of the tumour. The tumour is freely mobile. This case I saw in consultation with Mr. Wells, who advised only a preliminary tapping, as the case was unilocular. This was done on the 28th March, when thirteen pints of a dark-brown fluid were withdrawn, containing albumen; sp. gr. 1010.

On the 23rd a careful examination of the belly revealed some body, rather solid, lying around the uterus, but extending chiefly to left side, where more substance was to be felt. No fluctuation. Left the hospital 25th.

Readmitted May 10th, '72, for operation. Four months ago was confined, but at present the belly is very large.

This patient was now seen in consultation with Sir W. Fergusson, Drs. Greenhalgh and Rogers, who all recommended an immediate operation.

*May 15th.*—Operated in usual way; only that a Baker-Brown clamp and actual cautery were employed to pedicle. 29 oz. fluid were removed. Tumour weighing 2lbs. 9 oz. Wound closed by silver wires. 6 P.M.: No pain; feels weak. Pulse 84. A little brandy and water to be given. 12 P.M.: Pulse 140. Temp. 101°. Very sick. Breathing trouble-



some. Coughing. Evidently labouring under an incipient attack of bronchitis. Mustard poultices to chest. To be kept very warm.

16th. *Morning*.—Breathing very rapid. Sickness persists. To have acidi hydrocyanici dil.  $\mathfrak{m}$  v; sod. carb. gr. xv; tr. camph. co.  $\mathfrak{z}$ j; aquæ  $\mathfrak{z}$ j, every three hours. At 3 P.M. breathing still very rapid. Pulse 144. Temp.  $103^{\circ}$ . Sickness not so bad. Ordered tr. aconiti,  $\mathfrak{m}$ viii; liq. ammon. acet.  $\mathfrak{z}$ j. At 4 P.M. temperature was  $102^{\circ}$ . Breathing still rapid. At 5, breathing less rapid. Has had a little quiet sleep. Brings up a good deal of phlegm. No inconvenience about wound. No abdominal pain. Belly soft. Sickness better. At 7 P.M. pulse 110. Temp.  $101^{\circ}$ .

17th.—9 A.M. Greatly better. Pulse 140. Temp.  $100^{\circ}$ . Slept pretty fairly during the night. Cough much less violent. Has brought up a good deal of phlegm. No abdominal pain.

18th.—The cough is still rather troublesome. Pulse 108. Temperature  $100^{\circ}$ . This morning there was a good deal of backache, which preceded occurrence of catamenia this afternoon. Add to mixture tr. senegæ  $\mathfrak{z}$ j. Sing. dos.

19th.—Wound dressed. All healed except superiorly, where there is a little discharge from a suture. Temperature at 12 and 2 P.M. was  $100^{\circ}$ ; at 5,  $101^{\circ}$ ; at 10,  $99^{\circ}$ . Pulse 96. Cough greatly better. Sleeps well; eats well.

During the five following days nothing remarkable occurred. The following were the variations of temperature and pulse:—

Date.	Hour.	Temp.	Pulse.	Date.	Hour.	Temp.	Pulse.
20th.	6 A.M.	97.4	104	22nd.	9 A.M.	97.8	
	3 P.M.	96.6	96		1 P.M.	99.1	98
	6 "	96	96.8		9 "	98	96
21st.	9 A.M.		96.4	23rd.	9 A.M.	98	
	1 P.M.		97.4		9. P.M.	101	120
	5 P.M.	99.8		24th.	9 A.M.	99.8	108
	9 "	99			1 P.M.	98	
				25th.	9 A.M.	98.1	88
					9 P.M.	97.6	

The catamenia ceased on the 22nd. On the 23rd was ordered—Acidi nitro-muriat. dil. ℥j; tr. lavand. ℥x; eth. chlor. ℥x, p. r. n. if thirsty. Bowels were opened normally on seventh day after operation (22nd), and ate a chop the same day.

Left hospital early part of June, cured.

Hannah L., aged fifty, was admitted under my care, July 13th, 1872.

Had been suffering from an abdominal tumour two years. First appeared in right side. Great uneasiness. Pain all over belly. Catamenia stopped two years ago. Has had nine children. Youngest eleven. Tumour is very inconvenient, especially when she walks. Circumference measures at umbilicus  $39\frac{1}{2}$  inches. From sternum to pubis 22 inches.

*Examination.*—Both lumbar regions clear, especially on left side. Clear also superiorly below sternum. Dull inferiorly. Fluctuation very evident. Tumour is irregular and lobulated to the feel, especially in two places. Os is patent. Uterus three inches long. Tumour filling all Douglas's space.

This case was seen by Drs. Greenhalgh, Savage, Rogers, and Williams, who all diagnosed multilocular ovarian disease, and advised an immediate operation.

This, however, had to be delayed, owing to a slight attack of bronchitis.

On July 20th, Drs. Greenhalgh, Rogers, Williams, and Boulton assisting, an incision was made in the usual way in the median line till the tumour was reached. This was then found to be adherent everywhere in front, but the adhesions readily gave way except one superiorly, which proved rather tough. The tumour was now tapped. The opening in tumour subsequently enlarged; the hand introduced within, and other growths broken-down and emptied, till reduced in size. The cyst was extracted; the pedicle was removed by the actual cautery, upon a Baker-Brown clamp. A portion of adherent omentum was also removed with the actual cautery on a Henry-Smith clamp. The wound was now closed with

silver-wire sutures, and dressed in the usual way, and patient put to bed.

*July 20th*, 5 P.M.—A little sickness; not much. Temp.  $98^{\circ}6$ . Feels comfortable. Pulse under 100. Ice to be given, and patient kept perspiring. At 7, temperature was  $101^{\circ}2$ ; at 9,  $100^{\circ}6$ ; at 12 P.M.  $100^{\circ}$ . Pulse 88; no sickness.

*21st*.—Has slept fairly during the night, perspiring freely. No pain. Feels very comfortable. Variations of pulse and temperature as follows:—2 A.M.  $100^{\circ}4$ ; 4 A.M.  $100^{\circ}$ ; 6 A.M.  $99^{\circ}6$ ; 1 P.M.  $100^{\circ}4$ ; 4 P.M.  $100^{\circ}$ ; 7 P.M.  $100^{\circ}$ ; 10 P.M.  $99^{\circ}6$ . Pulse all the day, 84.

*22nd*.—Feels very comfortable. No sickness or pain. At 4 A.M. temp. was  $99^{\circ}8$ ; at 7 A.M.  $99^{\circ}6$ . Pulse 84. At 11 A.M., 2 P.M., and 6 P.M. temp.  $99^{\circ}8$ . Pulse 88. At 10 P.M. temp.  $98^{\circ}6$ . Pulse 84. There was a little sickness, for which an ammonia draught in lemon juice was given.

*23rd*.—Has been a little sick, but this she ascribes to the medicine, which she dislikes greatly, otherwise she feels very comfortable. Wound, which has healed by first intention, dressed. There is not quite so much perspiration. Pulse 80 all the day. Temperature was noted as follows:—

5 A.M. $99^{\circ}4$	4.30 P.M. $100^{\circ}4$
9.30 „ $99^{\circ}$	6.30 „ $102^{\circ}$
1 P.M. $99^{\circ}8$	9 „ $100^{\circ}2$

To have a little fish for dinner.

*24th*.—Progressing favourably. No sickness or pain. Pulse 88. Feels very comfortable. Temperature noted as follows:—

4 A.M. $99^{\circ}2$	2 P.M. $100^{\circ}$
7 „ $99^{\circ}2$	5 „ $100^{\circ}$
10 „ $99^{\circ}6$	8 „ $100^{\circ}2$
11 „ $100^{\circ}$	

To have chicken for dinner.

*25th*.—Feels very well. No pain. Had a little uneasiness last night from flatulence. Pulse 88. Temperature noted:—

9 A.M.	99°·8	2.30 P.M.	99°·2
10 "	99°·2	6 "	99°·4
12 "	99°	9 "	99°·2

To have a chop.

26th.—Convalescent. Wound quite healed (6th day after operation). Pulse 84. Temp. noted 8 A.M. 99°·2. 8.30 P.M. 99°·2. To have roast mutton.

27th.—Temp. 9 A.M. 98°. 9 P.M. 98°. Pulse 84.

28th.—Pulse 84°. Bowels freely opened naturally, assisted by an injection (8th day after operation). Temp. 9 A.M. 99°·2. 9 P.M. 99°·4.

29th.—Eats as usual. Bowels regularly open daily. Temp. 9 A.M. 98°·2. 9 P.M. 98°·4.

31st.—Is sitting up. A few strips of plaster only are placed every third day over abdomen. Temp. 9 A.M. 98°·2 9 P.M. 99°.

For the few following days temperature was noted only :—

Aug. 1st	9 A.M.	Temp.	99°	9 P.M.	97°·1
"	2nd	"	98°·1	"	98°
"	3rd	"	97°·4	"	97°·3
"	4th	"	"	"	"
"	5th	"	"	"	"

All the wires were removed on the 4th. Discharged cured, August 7th.

Jane W., aged fifty-seven, a housemaid, admitted under my care May 27th, 1875, at the request of Dr. Warwick, lately of Southend.

I saw this patient originally January 4th, 1875. She complained of feeling full. There was no abdominal pain. Had not noticed any enlargement there till September last, and then found herself short-breathed and growing stouter, although the belly was larger sometimes than at others. Has had no catamenia for last seven or eight years. She has occasionally a yellow discharge, and thick, but not bloody at any time. Occasionally, though not invariably, suffers from dyspnea on walking. The belly was found on



examination to be very large, with obscure fluctuation superiorly, more obvious, however, inferiorly. Both lumbar and epigastric regions clear. No cardiac sounds transmitted on auscultation of belly, only a few intestinal noises. Near the umbilicus, and to the right side, a part of the tumour feels more consolidated. Percussion sounds emit same note and pitch all over tumour. Uterus is high up. Os felt beneath pubis. Vagina is supremely full of folds. Os closed. Sound not admissible. The diagnosis then made was unilocular ovarian tumour. Fluid semi-liquid. Only a little ascites.

She returned to Southend, and was tapped by Dr. Deeping, of that town, who on inquiry kindly wrote me as follows: "The fluid removed from Miss W., in March last, was of a brown gelatinous nature. When the fluid had ceased to flow, a dull, solid mass was felt on the right inguinal region, about the size of a small fetal head. She suffered much after the tapping, when the lump began to expand. She had rheumatic fever some months ago, but her heart sustained no valvular lesion, as far as I can remember, although later, after she was tapped, the heart's action was very much disturbed, coincident with that of the lungs, producing a very slow intermittent pulse, with severe paroxysms of apnea."

On admission into hospital, on 27th May, I thought she was not so large as when I had seen her before. She now informed me that in the early days of her present disease she had suffered a good deal from sickness, but not at present. That she was satisfied she had caught cold on returning to Southend in January last, and that the amount withdrawn by the tapping on 11th March was  $14\frac{1}{2}$  pints of a variously coloured, semi-solid fluid.

On examination I found general fluctuation over belly, but clear percussion at epigastrium and low down in both lumbar regions. The right side was the least clear. She measured  $51\frac{1}{2}$  inches round the belly. There was also a good deal of ascites present. The veins over the belly were very large. She was quite unable to walk, and had to be

carried. Could not lie except in semi-erect position. Legs were very large and edematous.

The case was seen by Drs. Greenhalgh and Savage and Dr. Rogers, and at a later period by Sir W. Fergusson.

Dr. Greenhalgh wrote me the following opinion: "The more I think of J. W's. case, the more I am impressed with the notion that she is the subject of malignant disease, probably involving the pelvic organs, as well as extending over the abdomen. The edematous condition of the thighs, legs, and abdominal parietes, with the constitutional condition, bear a relation to the size of the fluctuating tumour in the abdomen, which is most probably the ovary. Then, again, why should the uterus be so completely out of reach? —a rare occurrence in ovarian disease, and when existing always a strong ground against ovariectomy. I would suggest, as her condition is a desperate one, you should tap in the ordinary way, or make an exploratory incision to a small extent, and then be guided by what you find as to any further proceedings."

Dr. Savage's opinion was also unfavourable as to an operation, but he was of opinion that an exploratory incision might be made, but that the result, most probably, would prove unfavourable. Dr. Rogers concurred, and Sir W. Fergusson, who saw the case on the day of operation, also sanctioned an exploratory incision as the best course under the circumstances.

On being apprized of the unfavourable view taken of her case, the patient at first objected to the operation: but a second examination having been made, the belly was found so much more full of fluid, and so tense, and respiration so short, all power of progression so completely lost, lower limbs so dropsical, and belly so tender in parts, that it was feared she would die *in transitu* to Southend. Of this she herself became convinced, and agreed to have the operation performed.

Consequently, in the presence of Sir W. Fergusson, Drs. Greenhalgh, Rogers, Percy Boulton, Bantock, Williams, Godson, and Mr. Thornton, I proceeded to operate on June 4th,

The abdomen being opened in the usual way a large quantity of serum escaped, subsequently a very large tumour of jelly-like consistence came into view, but this appeared to have given way posteriorly and expelled some of its contents into the peritoneal cavity, portions of this jelly coming away with the ascitic fluid. The tumour was next tapped, but the matters would not flow through the canula; the tumour was therefore cut open, and its interior scooped out with the hand, till so diminished in size that the tumour could be extracted. The pedicle was then separated on a Baker-Brown clamp, and burned off with the actual cautery. It was the right ovary which was affected. About (2 folds) 8 inches of the small intestine were found red and inflamed, affected with peritonitis. There was very little bleeding. The abdominal cavity was carefully cleared of all gelatinous remnants, and sponged, the wound closed by wire sutures, and the patient bandaged and put to bed. Ten drops of tr. opii were injected per anum.

Patient soon began to perspire, and continued pretty well all night, complaining only of a little abdominal pain. Temp. in axilla,  $99^{\circ}$ .

*January 5th.*—Feels pretty well, but complains of some abdominal pain. Repeat 20 drops of laudanum. 10 P.M. Pulse 100; temp.  $100^{\circ}$ . A little sick; mostly complains of flatus in abdomen.

*6th.*—Slept but little during the night; is very sick. Still a good deal of abdominal uneasiness. Temp. at 3 was  $100^{\circ}4$ . At 7 and 9,  $100^{\circ}$ ; pulse 108. Ice has been given freely to no effect. Continue the opium. 9.30: Sickness persists. Ol. succini  $\mathfrak{m}\mathfrak{v}$ , ex saccharo, 3<sup>tis</sup> horis. At 12 temp. was  $100^{\circ}$ . At 2 and 3  $101^{\circ}2$ ; pulse 108. At 3, as sickness persisted, was ordered a mixture of citrate of potash, which checked the sickness. Ordered raw beef juice injections.

*7th.*—5 o'clock. Pulse 112. Perspiring freely. Temp.  $101^{\circ}8$ . Chief complaint is from pain in abdomen, which is swollen and full of flatus. Has slept very little. Go on with laudanum. Apply turpentine stupes. 10 P.M. Pulse very weak—112; weakest on right arm. No sickness, but

abdomen very tender. Perspiring freely. Temperature axilla  $102^{\circ}8$ ; in vagina  $104^{\circ}8$ .

The patient gradually became weaker, and died next morning at 9 A.M. After death temperature in axilla was  $102^{\circ}4$ ; in vagina  $106^{\circ}2$ .

Post-mortem 24 hours after death. General peritonitis. A very few colloid jelly lumps in belly. A little muco-pus here and there. Large clot in right cavity of heart.

Of these four cases the last was a desperate one, and gave very little hope of recovery; but it was her only chance. A ruptured colloid cyst and peritonitis before operation could not but be most unfavourable complications.

Of the other three cases the first is a most favourable one, as exemplifying the use of Mr. Wells's clamp. The second and third are equally good cases, illustrating the advantage of Mr. Baker-Brown's clamp and actual cautery.

---

## THE TREATMENT OF HOOPING-COUGH BY THE IODIDE OF SILVER.

By ROBERT BELL, M.D., F.F.P.S.G., &c.

Physician to the Glasgow Ophthalmic Institution.

IT must be apparent to all engaged in the study and practice of medicine how unequally the science of it has progressed. This perhaps is more especially observable in the class of diseases which we call "epidemic." Whilst we are able to trace to their source and know how to prevent some of these affections—*e.g.*, typhus, typhoid, small-pox, &c., we must confess that others are as obscure to us as they were to our forefathers a century ago. Doubtless we can conjecture a good deal, and hazard a few very plausible theories, as to the origin of nearly every disease; but can we positively assert what is the potent entity at work which causes hooping-cough, scarlatina, or measles? and whence does it come? Those who are afflicted with sewage mania will affirm that these diseases, like plotting ogres, ascend the



drainage-pipes which ramify underneath our great cities and towns, and of necessity have communication with all our houses, and that hence we derive disease of this class ; but the proof is wanting. It is not my intention here to enter on sanitary controversies ; we have had these *ad nauseam* elsewhere. The object of this paper is to give an experience of the treatment of hooping-cough by means of the iodide of silver. It has fallen to my lot to treat over 100 cases with this substance, and with uniform success. It is now more than three years since I read of its being useful in this disease, and since then I have used no other remedy, except occasionally ten or fifteen grains of bromide of potassium at bedtime, which helps very much in procuring a good night's rest. In almost every case in which iodide of silver has been used by me, the cough has lost the hoop by the end of four weeks, and been quite well in six weeks, and I may add the usual complications of the disease have been exceedingly rare. I have twice employed the remedy in families where six patients were ill at the same time, and in both of these instances the disease was practically cured by the end of the fourth week. I have several times treated more than one child in a family at the same time, and with the like excellent results. The superiority of iodide of silver over the bromide of ammonium is most marked. A curious coincidence occurred which demonstrated this. It happened that one of my professional friends was attending a family, the children of which were suffering from hooping-cough, and who resided in one of our West-end terraces ; and at the same time I was attending another family in the said terrace, the children of which were also ill with this disease. My little patients were put upon one-eighth-of-a-grain doses of iodide of silver thrice a day, and this was all the medical treatment they required. My friend at this time was anxious to go away for a holiday, and he asked me to look after his patients during his absence. I found the children before mentioned in a state of great prostration, and most sadly afflicted with the cough ; one of them, in fact, narrowly escaped with its life. These little patients were being treated by the bromide of ammonium, and were ordered to be kept in one room, and I

did not feel justified in changing the treatment. I, however, ordered them to have a large pailful of boiling water brought into the room, into which about a tablespoonful of carbolic acid and glycerine was added, so that they might inhale the carbolic vapour. (This was two years ago.) It was three months before they were able to leave their nursery, and as many more before they were quite well ; while my patients, who were about the same ages respectively as the other children, and who had been taking the iodide of silver, and had been going out every fine day, had not a trace of the disease remaining at the end of a month. I do not attempt to explain how the iodide acts in this affection, but it seems to me that whooping-cough is a disease of the gastric periphery of the pneumogastric nerve, and that the silver salt acts as a sedative to this morbidly sensitive nerve, preventing reflex irritation being conveyed to the pulmonary ramifications of the nerve. I would urge all to give this preparation a fair and lengthy trial in the treatment of this disease, as I am convinced that in it we have a most valuable therapeutic agent.

---

## Reports of Hospital Practice.

---

### ST. BARTHOLOMEW'S HOSPITAL.

---

#### CASES SHOWING THE IMPORTANCE OF MAKING A CAREFUL PHYSICAL EXAMINATION.

By CLEMENT GODSON, M.D.

Assistant Obstetric Physician to the Hospital.

CASE I.—On August 14th, 1875, C. N., single, aged seventeen, of excessively anemic appearance, applied at the outpatient room, stating that she had been losing large quantities of blood from the vagina since Christmas ; had been regular from fourteen years of age up to that time ; had never missed a period. Cannot say that she has been free a day since Christmas ; is sometimes deluged with blood, and passes large clots. Previously never suffered from menorrhagia or leucorrhea.

The under-garments, even the petticoats, were found saturated with blood. The abdomen showed no signs of enlarged uterus. The cervix uteri was ascertained by vaginal examination to be short, the os patulous, the uterus retroverted, but freely moveable.

The patient was admitted into the hospital.

For two or three weeks, complete rest, with the cold douche, and the administration, first of ergot and subsequently of *cannabis indica* in large doses, also the replacement of the uterus by means of a pessary, were tried, but with little or no improvement. On September 6th some cotton wool saturated with *tr. ferri perchlor.* was passed into the uterus. It seemed to grate over a roughened surface towards the fundus. This only served to increase the hemorrhage.

On September 14th, the os uteri having previously been well dilated by tents, I passed my finger into the uterus, and detached from the fundus a quantity of soft pulpy tissue, having very much the appearance of decidua. From this day the hemorrhage ceased entirely, and the patient left the hospital on October 12th perfectly recovered.

CASE II.—On October 7th, 1875, S. E., aged forty-seven, was led into the out-patient room, supported by two friends, walking in a doubled-up position, the face expressive of great suffering. She stated that she had been married twenty-one years, and had had eight children and five miscarriages, the last a child, seven years ago. Catamenia commenced at thirteen years of age, and continued with regularity up to three years ago, excessive in quantity during the last ten years. No appearance for the last three years. Has been subject to leucorrhea ever since marriage. Three weeks ago was seized with violent pain in lower back and abdomen, which has never abated, and prevents her from sleeping. This is accompanied by a discharge from the vagina, of a brownish colour, sometimes sanious, and very fetid.

No abdominal swelling was detectable. A vaginal examination showed the cervix to be very short, smooth, and indurated, the uterine walls to be felt bulging from it on all

sides, the os patent, and a quantity of sloughing tissue protruding from it. Uterus quite moveable.

The patient was admitted into the hospital, and in the evening a sponge tent was inserted.

On the following day I introduced my finger into the uterus, the os being well dilated, and with the aid of the ovum forceps I succeeded in removing all the sloughing mass which I could feel attached to the posterior wall. The odour from this was very fetid, and I washed out the uterus with a weak solution of iodine. A few days subsequently a slight attack of pelvic cellulitis occurred, and some deposit was detected posterior to the cervix. This, however, soon cleared up. The uterus is now quite moveable, the patient is free from discharge and pain, and her general health is rapidly being restored.

CASE III.—On October 6th, 1875, E. B., aged thirty-two, applied at the out-patient room, her face very sallow and expressive of pain.

Has been twice married, thirteen years ago to first husband, by whom she had two children; eight months since to present husband. Catamenia regular till five months ago, up to which time was quite well. Since then there has been a continual discharge from the vagina, which is either like pure blood, or is of a brownish colour, and is very fetid; sometimes passes large clots.

*Physical Examination.*—Roof of vagina transformed into an irregular mass; the os uteri very patulous; in the centre of it the tissue in some parts very dense, in others friable, breaking down and bleeding when touched. The hand on abdomen comes upon a well-defined swelling, reaching almost to the umbilicus, pressure upon which is directly conveyed to finger in vagina. On abdominal auscultation the fetal heart can be plainly detected. *Mammæ* very attenuated; turbid serum can readily be squeezed from nipples.

The patient was admitted into the hospital.

On visiting the ward on October 11th, I was informed that during the night a large quantity of watery fluid had suddenly passed from the vagina, and the patient had been



in intense agony ever since. She was rolling in the bed with pain ; her pulse almost imperceptible, and so rapid it could hardly be counted. I found the os uteri dilated as widely as seemed possible without the tissues being rent, and the fetal head was presenting. The patient was at once placed under the influence of ether, and I perforated the head, and subsequently the chest, and succeeded in extracting a fetus (as far as I could judge, of nearly six months' development) through quite a small aperture, without having exerted any undue force in traction upon it. The placenta soon followed, and there was hardly any loss of blood.

She gradually sank, and died on the 13th from exhaustion. The autopsy showed no signs of a rent in the uterine structure.

*Remarks.*—These cases show the importance of a careful physical examination. In the first there was no history of pregnancy, or even coitus, ever having taken place ; a vaginal examination was insufficient to discover the cause of the hemorrhage, and it became necessary to explore the cavity of the uterus before the origin was ascertained. In the second case the symptoms were rather those of carcinoma, and the vaginal examination alone revealed the real nature of the disease, a submucous fibroid sloughing.

In the third case carcinoma of the cervix was believed to exist, and the patient had been treated for such by her medical attendant, but no idea of pregnancy existed. There was no history to lead to such supposition. It was the examination of the abdomen alone which led to the discovery. It shows therefore the importance of extending the investigation further than a mere vaginal examination.

---

---

# THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

DECEMBER, 1875.

---

## THE DISPOSAL OF STILL-BORN CHILDREN.

PART of the oath administered to midwives by the bishop, when he granted them licences to practise, ran as follows :—

“If any child be dead-born, you yourself shall see it buried in such secret place as neither hog nor dog, nor any other beast may come unto it ; and in such sort done as it be not found nor perceived, as much as you may ; and that you shall not suffer any such child to be cast into the jaques (privy), or any other inconvenient place.” This was certainly an economical, decent, and sanitary arrangement, contrasting favourably with the method recently adopted by the Southampton undertaker, who deposited the bodies of thirteen infants under the floor of his shop, until the effluvia arising from them led to their discovery. In spite of recent legislation, the regulations concerning the burial of dead-born children remain vague and unsatisfactory. It has been enacted that a person shall be guilty of a misdemeanor who endeavours to conceal the birth of or secretly bury a still-born child. It has also been made illegal to bury the body of a still-born child in a burial-ground without a certificate or declaration from a medical practitioner or coroner ; and making, giving, or using a false certificate of still-birth is also punishable. Notwithstanding these provisions, it seems to be doubtful whether an undertaker at present incurs any penalty, however “inconvenient” or repulsive may be the manner he chooses to adopt in disposing of the bodies of those who have never breathed. To the rich, the disposal of a still-born child is a matter of no difficulty : the accident is announced in the birth-list of the leading journal ; a neat little coffin is provided, and this with its contents is deposited

in a cemetery, with the necessary certificate and customary fees. But when a child is born dead in a garret, matters assume a very different aspect. There is no spare room in which the little corpse can be placed, and no spare money with which it can be buried. The immediate and paramount question in this case is, How can the body be most cheaply and rapidly removed, without breaking the law? It is doubtless advisable that the birth of every viable child should be registered. The difficulties, however, incident to the carrying out of such a scheme are very great. We are met on the threshold with the perplexing task of drawing lines. What are to be the anatomical characters which shall be taken as representing the development upon one side of which the expelled fetus shall be considered an abortion, and on the other a still-birth; or on one side viable, and on the other not; or on one side legal to conceal its birth, and on the other not? Again, what difference shall be insisted upon in the mode of burial of a child which has died during parturition, and one which has died directly after? What a fine line divides the case of a child (breech presentation) whose respiratory muscles have been observed acting abortively, and a child whose whole life has been represented by one feeble gasp. Yet now the former can be buried at half or quarter the expense of the latter, and hence arises an undesirable temptation for parents to bury as still-born, children who have lived for days. There can be no reasonable cause for such a difference: wood and workmanship for encasing either of the bodies must be the same, and the cost of conveying each to the cemetery must be equal. The coffin required for the still-born child would in some cases have to be the larger, but on an average the expenditure of time and material would in both cases be very similar. There can therefore be no valid reason why a still-born child should cost more to bury than an infant which has died after a brief existence, without being baptized. It is a good principle to make it as easy as possible to do the right thing. No obstacles should be placed in the way of the early and inexpensive disposal of still-born and semi-still-born children. Concealment of birth and burial by stealth will be best checked by the State

facilitating the registration and burial of infants. We are well aware that the whole of this subject is beset with innumerable difficulties, and not the least are those which refer to the prevention of crime—criminal abortion, concealment of birth and burial, and infanticide. Many wise heads are, however, industriously working at these problems, and every one must wish them good speed. The case of the Southampton undertaker, like that of his fellow-tradesman in Bethnal Green, has laid bare fresh complications, and these will doubtless lead to new and more perfect regulations.

---

### Notices and Reviews of Books.

---

*Cyclopedia of the Practice of Medicine.* Edited by Dr. H. VON ZIEMSEN. Vol. X. *Diseases of the Female Sexual Organs.* By Prof. CARL SCHROEDER. English Translation, edited by Dr. BUCK, New York. London : Sampson Low and Co., 1875. Pp. 575.

A CYCLOPEDIA of Gynecic Medicine in one volume, consisting of 575 pages of large print, is at the present time an impossibility. In 1843, when the subject was infinitely smaller, the volume of the "*Bibliothèque du Médecin praticien*," devoted to the diseases of women, contained 693 pages of small print, in double columns. Taking into consideration, however, the space at Prof. Schroeder's disposal, he has done all that could be reasonably expected. The volume is carefully written, and its materials are presented to the reader in an orderly and masterly manner. The portion of it devoted to pathology will for some time be referred to and quoted by English reading medical men, for it is particularly well done. Altogether the work must be acknowledged to be a valuable addition to our works on Gynecology. Its weakest point is its operative instruction. The author differs from the best authorities in many cases as to the selection of instruments and the mode of using them. The directions necessary for successfully carrying out some ope-



rations are insufficient. As we have stated, however, the size of the book, although large, prevented the author from entering into many subjects as fully as probably he would have desired. He has naturally devoted most space to the subjects with which he is most conversant, and we thank him heartily for his labour. The translation is excellent, inasmuch as it everywhere makes intelligible the meaning of the author. Words have, however, been used which in this country are not considered elegant. The book would have been rendered more acceptable to the taste of English readers had one of our countrymen been employed to cut these out, and supply in their places more agreeable synonyms.

---

*A System of Midwifery, including the Diseases of Pregnancy and the Puerperal State.* By WILLIAM LEISHMAN, M.D. Second edition. Glasgow: James Maclehose, 1876. Pp. 848.

It is with very great pleasure we have to announce a second edition of Dr. Leishman's excellent work. It is gratifying, more especially for two reasons. First, because omissions have been remedied and opinions corrected, and second, although the present volume contains several pages of additional matter, its price has been reduced so as to bring it more within the reach of the student. The last edition was sold for thirty shillings, the present is offered at a guinea.

---

## *Abstracts of Societies' Proceedings.*

---

### OBSTETRICAL SOCIETY OF LONDON.

*Wednesday, November 3rd, 1875.*

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P.,  
*President, in the Chair.*

The following gentlemen were elected Fellows of the Society:—  
John Ford Anderson, M.D.; R. J. Fancourt Barnes, M.B.; John

Branson, L.S.A. (Rotherham); and William Stewart, L.R.C.P. Ed. (Barnsley).

The PRESIDENT announced that Dr. Farre had accepted the Honorary Presidency of the Society.

---

*Hydrocephalic Head impeding Delivery.*

Dr. EDIS exhibited an infant which presented by the breech. Traction upon the trunk failing to accomplish delivery of the head, Dr. Edis was sent for. Examination of the abdomen showed that no second child was present, so that the question of twins with locked heads was negatived. The patient was a pluripara, and vaginal examination failed to detect any deformity of the pelvis. The child being dead and the head evidently much enlarged, perforation behind the right ear was effected—a gush of clear fluid confirming the supposition that had been made that the case was one of hydrocephalus. Delivery was then readily accomplished. The head when distended with fluid measured 17 inches in circumference. The patient convalesced normally.

The placenta had been examined microscopically by Dr. Coupland, who failed to detect any change in its structure throwing light upon the case.

---

*General Anasarca in a Fetus.*

Dr. PROTHEROE SMITH exhibited a fetus of six months and a half old, the subject of general dropsy. The mother, aged thirty-five, had previously had three living children born, three miscarriages, and three premature labours at the twenty-sixth week following each other, the last child was now shown. In the two last pregnancies she had suffered from albuminuria and liver derangement. Before the eighth labour the patient was kept lying up and well fed; before the last she got about and was thin. On both occasions there was incompressibility of the pulse and flushing of the face, and the labour was attended by considerable hemorrhage both before and after.

The body of the fetus was anasarcaous, there was a large quantity of clear brownish-yellow fluid in the peritoneal cavity, and also some fluid in the pericardium and pleural sacs. The kidneys were healthy. The placenta was large, and consisted of only the fetal portion.

The PRESIDENT inquired whether there was any trace of syphilis in the mother, and whether the edema existed during the life of the foetus, or was only a post-mortem appearance, the result of decomposition.

Dr. SMITH replied that the fetal heart had been heard the day before birth.

Dr. BRAXTON HICKS inquired whether there was any symptom of obstruction to the funis, to which Dr. Smith replied in the negative.

Dr. SNOW BECK stated that the fetus when born was in such a fresh, healthy condition as to preclude all idea of decomposition.

*Case of General Dropsy in a Fetus.*

Mr. LAWSON TAIT related the particulars of a case where the skin was so tense that it was not possible to bend the limbs without risk of bursting the skin. The scalp was so distended that the bones could nowhere be felt, and in fact the head had been mistaken for the breech during labour. The abdomen was enormously distended by highly albuminous fluid, as also the *pleuræ* and pericardium. The placenta was large and very edematous. The cause of the general dropsy seemed to be the premature closure of the channel between the auricles—there being no direct opening between the auricles through the foramen ovale.

Dr. PROTHEROE SMITH considered this to be evidently a disease primarily and altogether of the ovum, whereas in his own case the disease had originated from the disordered health of the mother. He would like to know whether any of the Fellows had met with similar instances—hyperemia with liver derangements. There was in his own case great peculiarity of the pulse, which was extremely incompressible—the mother's lips were of a crimson tint, and she had had several abortions. In one case who had aborted seven times he had bled her once and she went to full time. In his case he had tried abundance of fruit and wine in one pregnancy and exactly the reverse in the latter—the result being the same in both. He had witnessed in a few cases frequent consecutive abortions at about the fifth and sixth month arrested by venesection and followed in succeeding pregnancies by living children.

Dr. MADGE said he could hardly accept Dr. Smith's explanation as to the cause of the death of the fetus. He had given considerable attention to the subject, but he had never understood that a common disease or derangement of the liver in the mother could affect the child in utero in a somewhat similar way. It was well known that the mother may be morbidly affected in many ways—even with albuminuria resulting in convulsions, and still give birth to a perfectly healthy child. A syphilitic history, if such existed, might afford an explanation. The fetal liver receiving the tainted blood direct from the mother became affected with syphilitic inflammation and deposit leading to obstruction of the portal system and ascites.

Dr. ROUTH inquired if there had been any specific examination of the foramen ovale of the first child.

Dr. HEYWOOD SMITH replied that the heart on examination was found to be perfectly normal. The last three times pregnancy had ceased at the twenty-sixth week, showing that there was some disease of the ovum. The liver was also disordered in former confinements. He would be glad if any Fellow of the Society could throw any light upon the intermittence of the albuminuria during pregnancy.

Dr. SNOW BECK remarked that he had the opportunity of examining both Dr. Smith's cases, and had also seen two other very similar specimens. The clinical histories of each had been very much alike—

healthy living children at first, then premature expulsion of dead fetuses about the sixth month, presenting evident signs of previous and serious disease. Effusion of fluid into the peritoneal cavity existed in all—far too large in quantity to be the result of post-mortem change. The organs, excepting the liver, were healthy—this was paler than normal, soft, readily torn with the finger, the liver cells being much broken down. The parents were singularly free from even the suspicion of a syphilitic taint. The effused fluid was too large and too generally diffused to be caused by any mechanical disturbance of the circulation. The only probable explanation of this condition of the fetus appeared to be from some deficient or altered state of the nutrition—whether fetal or maternal it appeared impossible to determine from the facts at present known.

The PRESIDENT said that in considering the pathology of these cases it was well to bear in mind that the subcutaneous cellular tissue in children after birth was prone to become edematous from the infiltration of serum in certain depressed conditions of health. It was no uncommon occurrence in hospitals to find children who had been ill-fed and exposed to adverse influences, the subjects of a general edema, which might proceed to an extreme degree, without such malformation or disease as to account for it. The villi of the placenta in Dr. P. Smith's case had the appearance of being atrophied, and he might suggest that possibly the edema in the child was due to its imperfect nutrition in utero, and to the passive congestion this induced.

---

*On a New Form of Blunt Hook and Sling for assisting Delivery in Cases of Breech Presentation.*

Dr. J. G. SWAYNE (of Bristol) brought forward a communication on this subject. The instrument consisted of an ordinary blunt hook with the end curved back upon itself, so that a loop could be hitched upon it. When the hook was withdrawn after being passed over the groin, the sling consisting of a piece of strong silk cord with a loop at each end was left *in situ*. Traction was then made in the usual way, and as the sling was padded there was less risk of injuring the soft parts of the child than when the ordinary blunt hook was employed.

Dr. J. BRAXTON HICKS thought that in the use of the blunt hook sufficient attention had not been given to the assistance afforded by external pressure applied to the fundus uteri in breech cases, and the delivery of the aftercoming head through the brim. He thought half the strain was taken off the thigh, and that without undue crushing of the uterine wall by the hand pressure. Text books generally omitted to notice the great help given, and although he had no doubt some practitioners employed extreme pressure, yet very frequently it was never thought of, but all the force was put upon the hook.

Dr. MADGE had had reason to be dissatisfied with the blunt hook.



He was afraid that in any case when much force was required some injury must be inflicted, whatever the plan adopted. He had suggested covering the hook with india-rubber, compressed sponge, or wash-leather. If this could be properly carried out it would probably be a safer instrument than Dr. Swayne's, and could be much more quickly and easily applied.

Dr. AVELING remarked that the life of india-rubber was only about three years, it then becomes brittle, and if put aside would then be found useless. He feared, therefore, the suggestion, though ingenious, was not very practical.

Dr. C. GODSON agreed that whenever a foot could be brought down in impacted breech it was the best plan to adopt; but where this was impossible the hook must be employed. He had recently tried that known as Lazarewitch's, but found that the bulb at the extremity of the hook was the point which receives the force applied, and pressed severely upon the tissues. He regarded it as a dangerous instrument. In one instance the skin had been cut through and much bruising of the surrounding soft tissues produced by its employment.

Dr. LAWRENCE had seen a case where the saphena vein had evidently been lacerated from the use of a blunt hook. The sling was far less likely to cause injury.

---

### *On Prolapse of the Funis during Labour.*

Dr. GEORGE ROPER read a paper tending to show that this accident was rarely primary, but mostly the result of some other abnormality. After alluding to the various conditions predisposing to this complication of labour, and mentioning the different methods of accomplishing reposition, he stated that the real difficulty consisted in keeping the cord within the uterus after it had been returned. The best means of doing this seemed to be in securing the firm adaptation of the presenting part to the os uteri. The question of turning in place of forceps was then considered, the author preferring the latter method, considering that there was less shock to the mother. Reports of six cases were given. The author concluding by stating that prolapse of the cord is for the most part associated with some other complication of labour, and that it behoved us to make observations as to what that complication might be, as the management of the cord would in a great measure depend upon the nature of the complication; in some cases the funis could be saved from pressure, in others not.

Dr. BRAXTON HICKS had employed cephalic version in transverse presentations with prolapse of the cord, using one hand externally to press down the presenting part into the os uteri, the funis being pushed up simultaneously by the internal hand.

The PRESIDENT inquired if Dr. Hicks combined the postural method with reposition of the cord.

Dr. HICKS replied that he had not found it necessary. He had

experienced no difficulty in pressing up the head and carrying the cord up by the same hand as quickly as possible, and then the outside hand pressed the head into the os. In prolapse with head presentation, pressing the breech to the fundus is not needed as in transverse presentations.

Dr. BRUNTON thought Dr. Roper had not given sufficient prominence to the postural method. He (Dr. Brunton) had found it the most satisfactory of all methods of treatment of prolapse of the cord, and considered it applicable in all cases. He questioned whether Dr. Hicks would be able with the middle finger to reduce a long loop of cord protruding from the vagina; by the postural method this could be done.

Dr. EDIS called attention to the correct postural position being the genu-pectoral or knee-shoulder, not knee-elbow position, as had been spoken of by many.

The PRESIDENT referred to a paper read by Dr. Thomas, of New York, some eighteen years ago.

Dr. ROPER asked what was the rationale of the postural position. Gravitation would not account for it.

---

*Note on a Diseased Placenta.*

By LAWSON TAIT, F.R.C.S.

The author narrated briefly the outlines of a case. The patient, aged twenty-nine, miscarried three months after marriage. She bore a child at full term, but which died of convulsions three days after. She next miscarried between the sixth and seventh month, and again about the seven and a half month, the child being alive. When six months advanced with her next, Mr. Tait put her upon chlorate of potass and perchloride of mercury, and she went her full time and was delivered of a healthy living child. The chief point of interest was in the appearance of the placenta. It was disproportionately small to the size of the child, and fully two-thirds of its surface had undergone a peculiar change. This the author had examined most minutely and described most thoroughly, the communication being one of much scientific interest.

---

OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, Wednesday 23rd June, 1875.*

Dr. MATTHEWS DUNCAN, *President, in the Chair.*

*Case of Spina Bifida.*

Dr. MATTHEWS DUNCAN related a case of spina bifida which he had treated by excision, and in which the patient had died of bronchitis some weeks after the operation; and showed the specimen. The

child was born with a large tumour (about the size of a seventh-month fetal head) on the middle of the lumbar spine. The tumour was sessile, and its attachment was about  $1\frac{1}{2}$  inches in diameter, and corresponded to a portion of the vertebral column, where the *laminae* and *spinæ* were absent. It was firm and elastic, and the mother thought it increasing in size. The skin on it was in some parts natural, in others thin, translucent, and bluish or livid. There was no fluctuation or distinct feeling of fluid in it. The child was healthy and about three months old. Dr. Duncan first tried to evacuate it by tapping, but only a little clear transparent fluid ran out. More was squeezed out. Subsequently it was incised and freely squeezed, much nearly limpid fluid being expressed. The openings made soon healed, and the tumour became as large as ever. It was found to be composed of a myxomatous or edematous tissue, which was only slightly vascularized. Subsequently Dr. Duncan cut out a large wedge-shaped slice equal to a half of the whole. The incisions went down to the dura mater, to be afterwards described. This effected a proportional and permanent diminution of the tumour, and the child soon recovered from the operation; the wound being completely healed and the child well, Dr. Duncan intended to complete the excision. But the child, while in good health, twenty-seven days after the operation, was taken out late at night, caught severe bronchitis, and died after two days' illness. The dura mater was, on dissection, found to project from the regular contour of the spinal cavity to the size of about half a walnut, the great mass of the tumour being composed of myxomatous tissue enclosed between this membrane and the distended skin. There was no evidence of inflammation of the membranes of the cord, and during life there had never been any cerebral or spinal symptoms.

## OBSTETRICAL SOCIETY OF DUBLIN.

*Meeting, Saturday, June 12th, 1875.*

LOMBE ATTHILL, M.D., *President, in the Chair.*

*Further Observations on Fibrous Tumours of the Uterus.*

By GEORGE H. KIDD, M.D.

On former occasions I brought before the Obstetrical Society some observations on fibrous tumours of the uterus, founded on cases that had occurred in my own practice, and I now propose to lay before you some further observations on the same subject, suggested by cases that have since come under my notice.

Patients sometimes present themselves with large pendulous tumours protruding from the vulva, which are in reality tumours that were originally intra-uterine, but had been expelled from the uterine

cavity, and, after lying for some time in the vagina, passed out through the vulva.

A woman presented herself at the Coombe Hospital on the 20th of June, 1874, with a tumour of this kind, but she was able to return it almost completely into the vagina, and when she applied at the hospital only a portion of it protruded through the vulva, and this portion was ulcerated, and resembled, on a superficial examination, an old prolapsed uterus. She was able to expel the entire mass by voluntary efforts, but found it very difficult to return it, owing to its size. The tumour was observed to lie outside the vulva, as also did the long pedicle by which it was attached to the inner surface of the anterior wall of the uterus. She stated that she was forty years old, twenty-three years married, and had had five children and three miscarriages. Her youngest child was five-and-a-half years old, and she had not become pregnant since its birth. Her health had been good until about a year before she applied at the hospital, when she observed a number of clots coming away from the vagina, and, about the same time, a tumour outside the vulva. She said she suffered no further inconvenience till January, 1874, when she again had hemorrhage. After this menstruation was normal till the following April, when a more or less continuous dark discharge occurred, and she had frequently to have the urine removed by a catheter.

On the 22nd June I removed the tumour with the *écraseur*. Having got her to expel it from the vagina, and having got the drawing made of it, I passed the chain of an *écraseur* round it, and divided the pedicle close to its point of attachment.

The tumour weighed  $3\frac{1}{4}$  lbs., and measured  $7\frac{1}{2}$  inches in its longest axis, and  $5\frac{1}{4}$  in its shortest.

The woman left the hospital on the 7th July, quite well, and has remained in good health since.

In another case I had an opportunity of observing the whole progress of one of these tumours; and, as it had many other features of interest, I will relate it at length.

I was asked by Dr. Gordon to see a lady suffering from retention of urine, and on examination I found all the symptoms of a retroverted gravid uterus. The uterus was as large as at the twelfth or fourteenth week of pregnancy. The fundus lay under the promontory of the sacrum, and the cervix pressed against the neck of the bladder, so as to prevent the escape of urine.

My first impression was that the case was one of pregnancy, but the patient, who had had several children, stated menstruation was very regular and rather profuse, and denied the possibility of this. The opinion expressed was that if she were not pregnant, there was a tumour growing in the uterus.

I had an opportunity of seeing this lady from time to time after this. Menstruation became exceedingly profuse, and it became very evident the case was not one of pregnancy. The tumour could be



felt in the uterus; it increased in size; the hemorrhage became very profuse, and on several occasions she was reduced to such a state that her life was in danger, but she would not consent to have the tumour removed. At length the hemorrhage lessened very considerably, and her general health improved, but in July profuse bleeding again occurred, accompanied by expulsive pains, and when I visited her I found the tumour lying in the vagina, but attached to the inner surface of the uterus by a long, broad pedicle. Assisted by Dr. Gordon and Dr. Joseph Johnston, I drew it down out of the vagina, and removed it with the *écraseur*. When removed it weighed 2 lbs. 13 oz. The lady has since been restored to perfect health.

These are examples of the so-called sarcomatous tumours. They are fibroids of a loose texture, or fibro-cystic tumours.

The occurrence of retention of urine from retroversion of the uterus, as in this latter case, is rather a rare accident. It occurred several times, till the tumour grew so large as to prevent its falling under the promontory of the sacrum. This form of retention is, of course, to be distinguished from the form I have given instances of in some of my other communications, where the tumour lay in the true pelvis, and pressed directly against the urethra or neck of the bladder. After passing a catheter and emptying the bladder, the uterus was easily put into its proper position.

The next case I have to lay before you is one illustrating the recurrence of a fibroid growth. I have in this jar a tumour I exhibited before. It grew from the posterior wall of the uterus, and you can see the broad, rough surface by which it was attached. After the tumour was removed the patient remained in good health for nearly a year, and then the hemorrhage returned, and the uterus enlarged. Two years ago she again entered the hospital, and I removed the mass you see in this other jar. You observe it consists of eight portions; it was originally one tumour, but in removing it I could only get it away piece by piece, the operation occupying nearly two hours. The patient recovered without a bad symptom, and has remained quite well since. In a case I brought before the Society many years ago I had to operate four times, at intervals more or less prolonged, and removed twenty-nine small polypoid tumours, but these were new tumours—not a recurrence or re-growth of old ones, as seemed to be the case in the present instance. I may here mention that the young woman from whom these twenty-nine tumours were removed is now in good health, and though the uterus is large, menstruation is normal, and there is no hemorrhage or other discharge, and she is actively engaged in her occupation as a visiting governess.

This method of taking away the tumour piece by piece, where it cannot all be included in the loop of the *écraseur*, is one I have adopted in several cases, especially where there was not sufficient dilatation of the os and cervix to afford room for the necessary manipulation, and I think it worthy of a passing notice.

The next case I have to describe illustrates the practicability and advantage of introducing a large number of sea-tangle tents.

Mrs. H., admitted into the Coombe Hospital 17th June, 1874, aged fifty-four; married twenty-four years; never pregnant. For the last three years menstruation has been very profuse, accompanied by bearing-down pains, and lasting upwards of ten days, the discharge coming away in clots. During the intervals of menstruation she has a constant serous discharge; she is much weakened, has an anemic appearance, and is unable to attend to her ordinary duties.

On examination a tumour could be felt deep in the hypogastrium, and rising an inch above the pubes. On bimanual examination this proved to be the uterus, with its body enlarged and anteverted. The os was small, but soft and dilatable, and the sound passed in easily. The cavity measured  $3\frac{1}{2}$  to 4 inches. There was no bulging-out of the anterior wall.

On the 24th June twelve pieces of sea-tangle were introduced, each about four inches long and of the size of a No. 6 catheter. The following day these were removed, and a tumour was at once seen lying in the cavity of the uterus, and on passing in a finger this tumour was found to be growing from the anterior wall close to the fundus. The loop of a steel wire attached to an *écraseur* was passed round the neck of the tumour, but on tightening the screw the wire broke. Another loop was now applied and the tumour was removed. It weighed  $4\frac{1}{2}$  ozs., and measured in its longest circumference  $8\frac{1}{8}$  inches, and in its shortest six inches. On the 15th July the cavity of the uterus measured 3 inches, and there was no discharge of any kind. On the 18th July she left the hospital, and on the 1st October I received a letter from Dr. M'Donnell, of Randalstown, who had sent her up to have the tumour removed, in which he says—"Mrs. H. continues well. She has now her third menstrual period over, and has not been so well for many years. The discharge on the last occasion was about natural, and, better still, free from pain, and you have heard from herself how she suffered at these times, before the operation."

Now, as to the operation by which these tumours were removed. It was described to me in a paper read before this Society in 1868, and I have laid some copies of that paper on the table, as a diagram is given in it of the operation, and I wish on this occasion to allude specially to two points in regard to it. I believe that the operation is one that was first devised and described by myself. The hint was taken from Simpson and Marion Sims, but they do not describe the operation as practised by me. Sims suggests in his book the use of the sea-tangle originally proposed by Dr. Sloan, but he does not describe the operation, nor does he seem to have practised it. The operation consists of two stages, to both of which I wish to draw particular attention. In the first place, it is necessary to dilate the os uteri, and for this purpose I have been in the habit of using sea-tangle tents. One object of my paper in 1868 was to show the

advantage of sea-tangle over sponge, which was originally suggested by Sir James Simpson. My method is to introduce a number of pieces of sea-tangle, and I believe it is of the utmost importance, where the tumour is large, to introduce a sufficient number of sea-tangles, in order to get enough room to get at the tumour.

In the case I have just read it is stated I introduced twelve pieces of sea-tangle, about the size of a No. 6 catheter. When this can be done at a first sitting it is of great importance, but when it cannot be done without force it is desirable to introduce as many as possible, and subsequently to remove these and introduce a further number. In some cases where a certain amount of dilatation has been made, I have endeavoured to increase it by the india-rubber bags of Dr. Barnes, but I am quite sure one of the most important points in the operation is to secure plenty of room, and the most efficient mode is to introduce a sufficiently large number of the sea-tangle tents. It has been suggested by Dr. Joseph Johnston, of the Hospital for Soldiers' Wives, to introduce medicated sponge tents, after the sea-tangle has prepared the way, and it may be an improvement. I have not tried it, but I think if you can produce a certain dilatation by sea-tangle in the first instance, and then follow it up by sponge tents, it may be a great improvement. Having obtained complete dilatation of the uterus, the next point is to get the *écraseur* round the tumour, as shown in the plate in my paper of 1868.\* The tumour shown is a small one, but the same method applies to larger ones. The *écraseur* is shown about to be pushed home to the root of the pedicle. The anterior lip is to be seized, and the whole body of the uterus drawn down. Then the tumour is seized with a second *vulsellum*. I think it is of great importance to use the second *vulsellum* merely as a guide to the tumour, and to steady it. If you make great traction with the second *vulsellum*, I believe you run the risk of inverting the uterus, and, perhaps, of removing a portion of it with the tumour. You then pass the wire of the *écraseur* over the handle of the second *vulsellum*. This is a distinct part of the operation. If you have sufficient room, you may, with great ease if the tumour project into the uterus, pass the wire round the tumour, and once you can get the wire round it, you need not be very anxious to get it up to the base of the tumour, for if the eye of the *écraseur* be well applied to the neck of the tumour, the wire will slip into its place as it is tightened. Once you have the wire secured round the tumour the other step of the operation is easy. If the tumour is large, the wire may break, but you must adapt another, and persevere. The great point is to have sufficient room to manipulate. When I read my paper in 1868 I mentioned that I was in the habit of using a soft iron wire in this operation. Subsequent experience has induced me to use instead a steel wire, a piano wire being the best suited for the purpose. It was a paper of Dr. Barnes that

---

\* See *Dublin Quarterly Journal of Medical Science*, February, 1869.



suggested this to me; a paper in which he described the operation of dividing the head of the fetus in a narrow pelvis, and his reasons are equally applicable to this operation. The wire is compressed as you introduce it, but the moment it gets into the dilated part of the uterus the elasticity of the steel causes it to expand, and thus it is more easily applied.

As to the results of the operation, I may state as a fact, that in no case in which the whole tumour has been removed have I seen any unpleasant effect from the operation. In several cases in which only a portion of the tumour was removed the results have been fatal. Whether that was the result of excessive manipulation in trying to get out the tumour, or any other cause, I cannot say. In the case already mentioned, of which the preparation is shown in this jar, I was two hours in removing the tumour, and eight pieces were removed. The patient recovered without a bad symptom. In all these cases I have been in the habit of touching the inner surface of the uterus with strong nitric acid, with the idea of checking the regrowth of the tumour, and of preventing pyemic symptoms setting in, and I still do so, applying it freely to the whole surface, and, of course, to the portion from which the tumour was cut. I have never seen any injurious results from doing this, and I believe it causes the uterus to contract, prevents the absorption of unhealthy discharges, and is very useful. I believe the first time nitric acid was ever applied to the interior of the uterus was in the case operated on by me on the 11th July, 1868. This is the case already referred to, where twenty-nine tumours were removed. It was used at the suggestion of my colleague, Dr. Ringland.

The PRESIDENT.—Dr. Kidd deserves much credit for introducing this operation. He was the first to introduce the system of dilating the uterus by introducing a number of sea-tangle tents at one time. Experience has proved to us that it is not the number of tents used that renders the operation dangerous, but the undue prolongation of the manipulations. My own experience coincides with Dr. Kidd's, that where you are able to finish the operation in a short time all has gone well. In three cases in which I failed to remove intra-uterine tumours death followed, yet in none of my cases did the process of dilating the uterus extend over more than twenty-four hours. If at the end of twelve hours sufficient dilatation is not produced, I remove the lengths already in the uterus, and introduce a larger number of fresh pieces of sea-tangle in their places. With respect to the operation itself, it was described by Dr. Kidd seven years ago, and I believe his operation has not been improved on since; nor do I believe it can be improved. The steel wire possesses a great advantage over the chain or wire cable; to the latter I object entirely. If a strand breaks, as is frequently the case, it is liable to inflict injury, or to render the instrument useless. A soft iron wire is more easily manipulated than the steel wire, but on the whole the latter is



decidedly preferable. I dislike sponge tents; they produce a very offensive discharge, and not unfrequently injure the mucous membrane of the cervix, which sinks into the pores of the sponge as it swells, which is consequently lacerated as the tent is withdrawn. It is no novelty for the Society to hear of the application of nitric acid to the interior of the uterus. Dr. Kidd was the first person who so applied it, and the results were so satisfactory, that I adopted the practice, and have ever since advocated its use.

Dr. THOMAS MORE MADDEN said he had seen a case which strikingly confirmed Dr. Kidd's view, that the danger in these cases arose not so much from manipulation as from leaving a portion of the tumour behind. He had seen a considerable number of cases in which uterine polypi were removed, but the only one that proved fatal was one in which there was a large fibroid tumour, and which he saw along with Dr. Byrne and Dr. M'Clintock. They cut the tumour across, and removed about half, but did not succeed in removing the entire tumour, and the woman died. The operation was not nearly as long, and the woman did not suffer so much manipulation as in other cases, where the tumour was completely removed and the patients made a good recovery; so that in these cases he thought they should not be afraid of handling the uterus even roughly, but the operation should be completed if once undertaken.

Dr. J. A. BYRNE could fully corroborate Dr. Madden's statement. The case referred to was the only one in which he had seen bad results follow from an attempt to extract an uterine tumour. There were three operations in that case; the great difficulty was to fix the noose over the tumour, and on the last occasion the patient was so much prostrated that it was thought necessary to postpone further operation, and the lady died. He could fully endorse Dr. Kidd's statement, that the patient will bear a surprising amount of dilatation and chloroformization if the tumour be entirely removed, but if any portion were left behind, it gave rise to symptoms that caused unfavourable results.

Dr. DENHAM had seen a great number of these tumours, and he thought the result of the operation depended as much on the state of health or peculiar constitution of the patient as upon the handling by the operator or the nature of the tumour. When he went into the Lying-in Hospital as master he saw a case which had been there previously under the care of Dr. M'Clintock. There was a large fibrous tumour occupying the entire uterus. During Dr. M'Clintock's mastership he had removed very large portions of this tumour, and with decided advantage to the patient. She was occupied in a millinery establishment, where she had to stand during a great portion of the day; the tumour grew again, and during the first year of his mastership she presented herself at the hospital suffering from extreme exhaustion. The tumour was very large, and he removed it by means of the vulsellum and common scalpel, removing slice after slice as if it were a turnip. No constitutional disturbance followed this operation. On

the contrary, her health improved, and she went out better. She came again under his observation, suffering from a severe rheumatic affection; the tumour was enlarged, and she appeared like a woman in the first stage of labour. He made a crucial incision through the os, with the object of enabling him to draw the tumour down. That simple operation was followed by pyemia, and she died with all the symptoms of diffuse inflammation. On the former occasions she underwent severe manipulation, and no bad consequences ensued. So that they must be careful to distinguish between the health of the individual at the time, and the actual amount of injury inflicted on the patient by manipulation. At one time an individual would bear an amount of manipulation which at another would prove fatal. In another case which came under his observation, the wife of a clergyman was, by repeated hemorrhages, brought to the last stage of exhaustion. The os was dilated with sea-tangle for a considerable time, and a portion of the tumour removed, but she was so exhausted that it was not considered justifiable to continue the operation. Nitric acid was then applied to the uterus. After some time she rallied, and the remarkable fact was that the tumour seemed to have entirely disappeared. These cases were of deep importance to the obstetrician, and they had yet a good deal to learn respecting them. There was one point on which he was inclined to differ from Dr. Kidd. He said that in seizing the tumour with the second vulsellum the object was to steady the tumour, and that by making traction there was a danger of inverting the uterus. Now, he thought that by drawing down the tumour with the second vulsellum they brought the tumour into a better position for getting the wire over it, and brought the base of the tumour nearer to the fingers. Having got the wire over it, they should then allow the tumour to resume its natural position, and then tighten the wire. This, however, was a mere practical point of detail. In the President's recent valuable work he had made allusion to this operation, and gave a diagram of it, which was precisely similar to that given by Dr. Kidd, who he believed deserved the credit of having been the first to introduce the operation, but that did not clearly appear in the President's work. He (Dr. Denham) had seen many of those cases, and he approached them every day with more and more diffidence as to the result of the operation. Where a young female had been broken-down by disease, or where there was a constitutional tendency to disease, they should be guarded as to prognosis. He saw a patient not long ago where this operation was attempted, and a portion of the tumour removed, but the patient gradually sank, and died in twenty-four hours, to the great distress of the family, for the result was not anticipated.

The PRESIDENT said.—I wish to say a few words relative to the description of the operation for removing intra-uterine tumour given in my work on the Diseases of Women referred to by Dr. Denham. Nothing could be further from my intention than to claim the operation as mine, and I am surprised that such could possibly be inferred.

I speak of dilating the cervix, which is the first stage of the operation, by means of several pieces of sea-tangle, introduced at the same time, as Dr. Kidd's method. Describing the second stage of the operation, I attribute to Dr. Kidd the introduction of the use of the steel wire, and then refer to him as being the person who first applied nitric acid to the interior of the uterus, at the conclusion of it, thus identifying him with every stage of the operation. If I have not been sufficiently explicit, I regret it; and should my work reach a fourth edition, I shall take care to remove the possibility of doubt or of being misunderstood. As to the removal of the vulsellum, which holds the lip of the uterus after the tumour itself had been seized, I differ from Dr. Kidd to this extent, that if the vagina be very narrow, as it was in the case described in the lecture referred to, I believe it to be a wise proceeding, but if the vagina be relaxed, or the tumour very soft, it may be unnecessary or even unadvisable to do so.

Dr. J. A. BYRNE said he met with a remarkable circumstance that day which he thought worth mentioning. Some few years ago he removed a fibroid tumour from the uterus of a young woman, and that day she came to him labouring under a very large fibroid enchondromatous tumour of the jaw. It commenced to grow after the removal of the uterine tumour. This connexion between enchondromata and fibroid tumour of the uterus was not a solitary instance; it had been observed before, and he should like to know from Dr. Kidd and other members of the Society, who had had large experience, whether they have noticed any such cases.

Dr. JOSEPH JOHNSTON said he had introduced the use of the medicated sponge tents with the view of effecting more rapid dilatation. He first introduced the sea-tangle, and then when a certain amount of dilatation had been effected, he replaced the tangle with a sponge tent, and by that means got a considerable amount of dilatation in a short time. There was, however, certainly a great disadvantage in using the sponge tent. He never was so much struck with this as when using the sponge in an operation for vesico-vaginal fistula. Every time the sponge was put in, for the purpose of wiping the surface, the woman would cry out—"It is the furze you are using." He never used anything but carbolized sponge tents.

Dr. GEORGE JOHNSTON.—With regard to the sea-tangle tents I should like to ask Dr. Kidd has he ever found any feverish consequences resulting from the introduction of sea tangle; for in our practice in the Rotunda Hospital we have had serious consequences resulting from the introduction of a No. 6 tangle tent. I never can forget one case that left an indelible impression on me. A fine healthy young woman came into hospital suffering from a fibroid tumour of the uterus. I thought it necessary to introduce the sea-tangle tents. I do not think we introduced more than two tents of No. 6 in size. In six hours she complained of pain, and they were withdrawn, and in six or seven hours metritis set in, and she died. That was not the only case in which we have found bad symptoms



arising from the sea-tangle tents, and I flatter myself we introduce them as carefully as other practitioners. A short time ago I had a private case, where the lady could not bear the tangle tent for an hour. As to the removal of the tumour in one sitting, we had a case where it was necessary to have three sittings before we could remove it. On each occasion we removed two or three ounces, and the operations occupied more than a week, yet the patient went on well, and perfectly recovered. During the operation we used Condyl's fluid, to prevent any danger of septicemia.

Dr. FINUCANE said when he was attached to the Coombe Hospital a poor woman was admitted, who came from Manchester, under the impression that she had disease of the womb, but who really had a large stone in the bladder. The method of dilatation adopted was this. Some beeswax was melted, and the base of the sponge placed in it. The sponge was then introduced, and on the melting of the wax the sponge expanded, and in this way the dilatation was accomplished so completely that Dr. Sawyer was enabled to take away a stone as large as a turkey's egg.

Dr. GEORGE JOHNSTON.—With respect to the removal of a fibroid tumour of the uterus being followed by enchondroma of the jaw, I may mention that I had a case in which I removed a polypus from the fundus of the uterus, and the woman went on very well until six or eight months ago, when she got a tumour in the breast, and is now in Stevens's Hospital for the purpose of undergoing an operation.

Dr. KIDD.—With regard to the supposed connexion between fibroids of the uterus and fibroid tumours growing in the parotid gland or in the breast, I think before we can be in a position to discuss that question we should have some histological evidence of the nature of the tumours, for the prevailing opinion in the present day is that uterine fibroids are developments of the non-striated fibres that compose the uterus, whereas we are not aware of any non-striated fibres in either the parotid or the mammary gland. We should not confuse our ideas of pathology by analogies which are not fully worked out. Dr. George Johnston has spoken of irritation arising from the use of sea-tangle. I have seen it, and I have also seen irritation arising from sponge tents. I have seen two fatal results from sea-tangle—one in my own practice, and the other in the practice of a friend—and I always regard the dilatation of the uterus by sea-tangle as the most serious portion of the operation, and the portion that is to be most anxiously watched. I never introduce sea-tangle that I do not watch my patient most closely, and I am prepared to withdraw it on the least symptom of irritation. But few weeks pass by that I do not introduce sea-tangle tents, most frequently for the purpose of getting at a diseased mucous membrane, in order to treat it, and frequently for the purpose of ascertaining if a tumour is in the uterus; but the fatal results I have seen are limited to the two cases I have mentioned—one in my own and the other in the practice of a friend. In



the latter case the uterus had been so fully dilated some months previously that we were enabled to get our fingers into it, and it was then decided to postpone the operation, and some months were allowed to go over before we attempted it again. Then a few tents were introduced, and symptoms soon set in which induced us to withdraw them, but the patient died. That very instance is an example of what Dr. Denham has spoken of, that it is the peculiar constitution of the patient that causes this irritation to arise. It is not the amount of dilatation, but the unhealthy condition of the patient at the time, that causes the unfavourable result. I think that that stage of the operation, however, ought to be got over as quickly as we can, and the suggestion of Dr. Joseph Johnston as to the use of a medicated sponge appears to me a valuable one, and very probably on the first suitable occasion that offers I shall adopt it. An objection to the use of the sponge only is that you have to introduce it time after time before you get the uterus dilated, and you have the os externum dilated a long time before you can effect dilatation of the os internum; but by first introducing the tangle you can afterwards introduce the sponge with good effect, and when the sponge is medicated, either by carbolic acid or some other preparation, you do away with another great objection to it—that is the fetid discharge. As to the slipping of the vulsellum, if one be used such as I have described, it will not slip. Dr. Denham has spoken of inverting the uterus. I believe there is not much difference between us; he suggests the drawing down of the tumour so as to get the wire round it. I do not object to that, but I do object to dragging down the uterus and keeping it down by means of the vulsellum attached to the tumour, as I believe you thereby run the risk of partially inverting the uterus, and even of removing a portion of it. The *écraseur*, as Marion Sims says, is a very greedy instrument; it has a great tendency to drag tissues into its grasp that ought not to be in its grasp, and Dr. Sims describes very graphically a case in which, in amputating the cervix of the uterus, the cavity of the peritoneum was opened, but he at once closed it by sutures, and the patient recovered. I believe that when you are operating on a tumour of the uterus, or on the cervix, the less you draw the uterus down the better. I cannot pass over the remarks the President has made. I am quite sure, sir, there was no intention on your part to deprive me of any of the credit of this operation; the operation, as far as I know, is entirely my own. It is described in your book, and the description given there is nearly the same as in my own paper, but by an unfortunate oversight it is so described that many have thought it originated with yourself. You have, sir, too frequently, in your book, and in many other instances, spoken favourably of me, to permit me to entertain the idea that you wished to deprive me of the credit of the operation. At the same time I am glad the matter has been spoken of, for if there is any credit or discredit due for the operation, I wish myself to bear the responsibility of it.

Dr. WARREN, House-Surgeon in Steevens's Hospital, said that the case alluded to by Dr. George Johnston of a tumour of the breast had been operated on and the tumour found to be of a decidedly malignant nature.

---

Dr. M'CLINTOCK: It is known that we are about to lose from this city a most distinguished physician, and one of the most eminent members of the obstetric profession. I need hardly say that I allude to Dr. Churchill, an original member of this Society, and one who has always shown himself its steady friend and supporter. He is known to us all personally, and I can say nothing that could increase the estimation in which he is held amongst us. We have all had occasion, at one time or another, to seek his aid and assistance in difficult and anxious cases, and I believe every one will be ready to say that in his relations with his professional brethren and the public Dr. Churchill has always acted up to the highest standard of professional conduct. Had he done nothing else to win our respect, this feature in his character would alone entitle him to it. I am sure I only express the feeling of every one present when I say I sincerely regret Dr. Churchill has resolved on giving up practice, and going to reside at a distance in the country. I think we should be wanting in respect for ourselves were we to allow such a distinguished member of our body to go away without offering him some mark of the high esteem in which he is held by those who had the best opportunity of knowing him, and I therefore move that the Council of the Society be requested to draw up a suitable address to Dr. Churchill on his retirement from practice, and his departure from Dublin.

The motion was seconded by Dr. George Johnston, and adopted amid applause.

---

*Saturday, 10th July, 1875.*

*Address to Dr. CHURCHILL.*

Dr. KIDD said.—I have been entrusted with the duty of moving a resolution carrying out the wishes of the Society, expressed at its last meeting. On that occasion it was moved by Dr. M'Clintock and seconded by Dr. Johnston that the Council be requested to prepare an address to be presented to Dr. Churchill on the occasion of his retiring from practice. The Council held a special meeting for that purpose, and the following is the address that was agreed to on that occasion:—

“ *To FLEETWOOD CHURCHILL, Esq., M.D., Ex-President and Member of Council, Dublin Obstetrical Society.*

“ *DEAR SIR,—We, the Council and Members of the Dublin Obstetrical Society, having heard with much regret of your retirement from professional life and departure from Dublin, desire to express*

our sense of the great loss we all shall sustain by your removal from amongst us.

"We feel that this loss is not merely confined to the sphere of our Society, with which you have been intimately connected since its formation, but extends also to that of private practice, where your wise counsel, your great experience, and, above all, your high standard of medical ethics, were universally recognised and appreciated.

"Most sincerely do we hope that you may long be spared to your social circle and numerous friends, and may now enjoy that repose and uninterrupted domestic happiness which you have so well earned by a lengthened career, not less distinguished for the conscientious discharge of every professional obligation, than for the successful cultivation of all the branches of obstetric science, and for the promotion of every useful and philanthropic work that came within your reach.

"We beg to assure you that, wherever you may go, you shall carry with you the respect and warm regards of every member of our Society, and our best wishes for your health and happiness."

The resolution I have been asked to move is that the address now read be adopted, and that the Secretary be instructed to have it properly engrossed and signed on behalf of the Society by the President, the Vice-Presidents, and the Secretary, and forwarded to Dr. Churchill with as little delay as possible. It is quite unnecessary that I should say anything in support of this resolution. I feel that the address which has been drawn up by the Council expresses most truly the feelings of every member of this Society. For my own part, I must say I endorse its every word most cordially, and I am quite sure that every member of this Society feels that in the departure of Dr. Churchill we have not only lost a valued friend, personally and socially, but one whom we all looked up to and esteemed as a great professional light; one who has conferred great benefits on our branch of the profession, and whose name is known all the world over, and will be known as long as medical science is cultivated.

Dr. DARBY.—I have been unexpectedly called on to second the resolution, and I do so with all my heart. There could not be a more proper address prepared than that which has been read to you; and I wish to express my entire concurrence in everything which has been said about Dr. Churchill. Having known him in his earlier years, I may mention that his indefatigable industry and research in the promotion of obstetric science was one of the most remarkable features of his professional career.

The PRESIDENT said he should not put the question to the vote, but ask the meeting to pass it by acclamation. There was not a member present who did not cordially re-echo every word in that address. Dr. Churchill, irrespective of his great professional merits,

was not only respected and esteemed by his professional brethren, but he might also say beloved by them, and every member of that Society felt the greatest regret at his retirement from amongst them.

The PRESIDENT then put the resolution, which was carried by acclamation.

---

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

*Meeting, Tuesday, October 26th, 1875.*

SIR JAMES PAGET, Bart., D.C.L., LL.D., F.R.S., *President,*  
*in the Chair.*

*On the Use of the Actual Cautery in the Enucleation of  
Fibroid Tumours of the Uterus.*

By ROBERT GREENHALGH, M.D.

Dr. GREENHALGH, at the commencement of his paper "On the Use of the Actual Cautery in the Enucleation of Fibroid Tumours of the Uterus," alluded to the infrequent use of the actual cautery as compared with its application on the Continent, and passed on to enumerate the cases in which, for the last twelve years, he had used it with more or less success. These cases include chronic enlargements, with induration of the cervix uteri, due to inflammatory or fibroid diseases; epithelioma and cancer of the neck of the uterus where the organ is moveable; some cases of vascular tumour of the meatus urinarius; slight cases of recto- and vesico-vaginal fistula; in incontinence of urine due to dilated urethral canal, and in certain cases of interstitial and intra-uterine fibroid growths. It was to its use in the two latter classes of cases, that he limited his present observations. Full notes of a series of five cases in which this operation was performed were then given. The following is a brief outline of this part of the paper.

CASE I.—Forty-three years of age, married thirteen years, with six children; came under treatment for retention of urine in 1866. For three years there had been much vaginal discharge, and, latterly, menorrhagia. Examination showed a large elastic growth from the posterior wall and fundus of the uterus, reaching to within an inch of the umbilicus, and in the pelvis compressing the rectum. A trocar was introduced into the mass in the vagina, which was followed by jetting of arterial blood, only checked by means of the actual cautery. A profuse serous discharge followed, and four days later the cautery was again applied, and by degrees the tumour shelled out from its capsule through the aperture thus made. The tumour was of the size of the fetal head at term.

CASE II.—Forty-seven years of age, married twelve years, never pregnant. Came under treatment for difficulty in micturition and defecation. A large tumour, flattening the rectum, filled up the



pelvis. It was opened in the most dependent part by the actual cautery. Death occurred from peritonitis, due to perforation of the intestine, and the tumour was found to have contracted adhesions with the cecum and rectum, and to be ulcerating into these cavities.

CASE III.—Thirty-eight years of age, married thirteen years, four miscarriages; a large tumour from the posterior lip of the os uteri obstructed labour. The growth was treated by the actual cautery. The tumour gradually enucleated, and the patient made a good recovery.

[With reference to this case, Dr. Greenhalgh read a letter received from Mr. Marriott, of Leicester, stating that last month the patient was delivered safely of a full-grown child at term. Before the operation was undertaken, the posterior lip of the uterus used to enlarge during pregnancy, and on the last occasion premature labour had to be induced.]

CASE IV.—Aged forty-seven, married, never pregnant; suffered much from hemorrhage, constipation, and difficult micturition. A large tumour occupied the hypogastric region; the os uteri was stretched over the growth. The cautery was used in all four times, and the tumour removed piecemeal, gradually disintegrating. The patient recovered.

CASE V.—Aged thirty; came under treatment in August, 1874; had suffered from floodings since January. An enormous fibroid tumour occupied a large part of the abdomen, and below it projected into the vagina from the posterior lip of the uterus. The case was under treatment for several months. The cautery was frequently applied, and the mass gradually disintegrated. There was much putrid discharge, and constitutional disturbance. The patient died in February, 1875, from embolism, after the growth had been apparently, in great part, if not wholly removed. The tumour weighed in all 7 lb. 1 oz. Thus of these five cases three were intra-mural, one intra-uterine, and one extra-mural. The author expressed an opinion that diffused fibroid deposits of the uterus in the early stage were more amenable to treatment than was generally supposed. Further, there were cases in which through the large losses of blood to which they gave rise, or the mechanical effects they produced, surgical interference was demanded. Dilatation or enucleation by the knife was sometimes employed, but this had been followed by ill effects, while the advantages of the actual cautery might be summed up as affording facility of application, causing but little pain, being rapid in its action, giving rise to no bleeding, and therefore obviating plugging; in the charred opening being unfavourable to absorption; in there being no offensive discharge from the charred surface; in the ready dilatation of the opening without bleeding; in permitting of manipulation through the opening immediately after its use. Lastly, by the use of the cautery, portions of the tumour may be rapidly destroyed, its size reduced, and its lower segments rendered conical, thereby facilitating dilatation of

the opening, and the subsequent detachment, expulsion, or removal of the morbid growth. Spontaneous expulsive efforts shortly followed its use, and the density of the tumour appeared to be more or less reduced. In conclusion, the author drew especial attention to the three following points:—Firstly, the advisability of the gradual detachment of the growth from its surrounding capsule, especially in cases where the tumour is of large size, or where the patient has been much reduced by previous hemorrhage, by which further losses of blood are avoided, and more perfect contraction of the investing tissues is secured, and the chance of pent-up offensive discharge is almost certainly prevented. Secondly, the removal of only so much of the tumour at each operation as is external to the opening, by which the opening is kept dilated, and all chance of its closure upon the remainder of the growth avoided. Thirdly, the speedy destruction by the cautery, or removal by the *écraseur* or hand of the tumour, should sloughing ensue.

Dr. MEADOWS, while acknowledging that the method proposed was a valuable addition to the modes of treatment of fibroid tumours, pointed out the difficulties arising from its application, owing to the variable amount of union between such tumours and the uterine wall, for whereas some were distinctly encapsuled, and could be enucleated with ease, others were intimately blended with the muscular fibres. It was difficult in practice to distinguish these cases. Then the cases in which the operation was applicable were those in which the tumour was strictly intra-mural, so that the uterine tissue might aid in the process of enucleation. This or any other operative measure was contra-indicated if the growth were close to the peritoneal or mucous surface. The difficulty in diagnosing the exact site of the growth was then a great hindrance to operative measures. The cautery was no doubt preferable to the knife, but as it frequently required the introduction of the finger to break down the attachments of the tumour, another element was introduced. The cautery *alone* would be almost free from danger.

Dr. BARNES concurred with Dr. Meadows in the difficulty of diagnosis of the exact nature and site of many fibroid tumours. Those springing from the cervix might fairly be treated by this method, but there was great risk in dealing with those arising from the fundus. One danger he especially insisted on was the tendency which these growths showed—after operations had been practised on them—to undergo a process of low necrotic inflammation, terminating in septicæmia; a course of events followed in one of Dr. Greenhalgh's cases. The actual cautery could not guard against this result, and he almost doubted if it were perfectly secure against the supervention of hemorrhage. He was not then at all prepared to accept the use of the cautery, which much resembled the "gouging" process; and it could only be reserved for extreme cases.

Dr. GREENHALGH, in reply, pointed out that in his cases the cervix was involved in the growth. He would reserve the cautery for suitable cases, and would not employ it in sub-peritoneal tumours.

He alluded to the soft semi-fluctuating character of some of the tumours, and the escape of serum that followed. If operative measures be undertaken, the cautery was the best means, for even dilatation was dangerous. Moreover, by the use of the cautery, there was no interference with the cavity of the uterus—*e.g.*, Case III.

## THE ROYAL SOCIETY.

*Meeting, May 27th, 1875.*

### *Note on the Discharge of Ova, and its Relation in Point of Time to Menstruation.*

By JOHN WILLIAMS, M.D. Lond.

Assistant Obstetric Physician to University College Hospital.

It is a recognised fact in physiology that ova are discharged in connexion with the menstrual function, but it is uncertain at what time in the course of the month the separation takes place. It is generally understood to occur towards the end of the discharge, or immediately after its cessation. I have, however, reason to believe, from observations made in several subjects, that such is not the case, but that it takes place before the appearance of the monthly flow with which it is connected. The cases which have come under my observation fall into four series, as follows :—

*A. Cases, six in number, in which a Graafian follicle had been matured and actually ruptured.*

(1) The first of these was a young girl who died through the effects of a fall, three or four days before the expected return of the catamenia. In the left ovary was a recently ruptured Graafian follicle. The cavity of the follicle was about  $\frac{3}{4}$  inch in diameter, and contained a recent clot, which projected slightly through the rupture; the clot was of a fresh red colour, nowhere adherent to the parts around, for on making a section through the follicle it fell out. The wall of the vesicle was of a pale yellowish colour, and slightly wrinkled. The rupture had evidently taken place a short time only before death.

(2) The second subject was a woman who died suddenly through a fall, probably a fortnight after the cessation of the last menstrual flow. On examination a considerable quantity of blood was found in the cavity of the peritoneum, and the liver was torn. In the left ovary was a ruptured follicle, with corrugated and collapsed walls; its cavity contained no blood, but there was a slight effusion between its lining membrane and its outer coat. The depth of the follicle from the rupture to the furthest point of the opposite wall measured nearly  $\frac{3}{4}$  inch. It is not impossible that this follicle was ruptured somewhat prematurely by the shock of the fall.

(3) The next example was observed during life. Mr. Christopher Heath performed ovariectomy on a patient on the fourteenth day after the cessation of the last catamenial discharge. Menstruation lasted



usually three days, and the patient had always been regular every four weeks. In the ordinary course of things the next flow would have appeared in eleven days. When the diseased ovary had been removed, the remaining one was raised up, with a view to see if it were healthy, and it was observed that it contained an enlarged Graafian follicle, which became ruptured when being held in the hand. I ought to add that the flow returned three days after the operation, and eight days before it was due.

(4) The next case was a young woman who died of pleurisy on the fifth day of menstruation. On the surface of the left ovary was a rough, brownish-coloured, star-like cicatrix. On section there was seen under the cicatrix, a corpus luteum, dilated in the middle and narrow at both ends, nearly  $\frac{3}{4}$  inch in length and  $\frac{1}{2}$  inch in width; its walls were in some parts of a pinkish and in others of a yellowish colour, slightly if at all thicker than those previously mentioned, and had small prominences on its inner surface. In the centre was a partially decolorized clot, which was but slightly adherent to the surrounding walls. From these characters it is evident that the rupture of the follicle had taken place several days before.

(5) The fifth member of this series was a patient who died on the fourth day of menstruation, and about the ninth of typhoid fever. One ovary contained a corpus luteum similar to the one just described.

(6) The last example occurred in a young girl, who died of pneumonia six days after the cessation of the catamenia. On the surface of the right ovary was a small cicatrix, beneath which was a corpus luteum with the following characters:—It was of an irregular, elongated shape, nearly  $\frac{1}{2}$  inch in length and  $\frac{1}{4}$  in width; had thick, yellow, convoluted walls, and enclosed a small whitish mass, in which were two dark-coloured spots, which were evidently the remains of a clot. This ovary contained also a Graafian follicle of the size of a small pea. The determination of the age of effused blood is always difficult. In the Graafian follicle which becomes ruptured without impregnation taking place it is known that certain definite changes occur; the wall of the vesicle becomes thick, yellow, and convoluted; the blood which flowed into it and filled it becomes decolorized and absorbed. The exact length of time in which these changes in the follicle are brought about is not accurately determined; but it is known that the corpus luteum of one menstruation has become considerably atrophied by the return of the next.

It appears to me that the yellow body in the last example of this group was considerably older than the two preceding ones, and that it was more than a fortnight old, and that the two preceding ones were from eight to ten days.

*B. Cases, four in number, in which a Graafian follicle had been matured, and hemorrhage had taken place into its cavity, but no actual rupture had occurred.*

(1) The first case was a patient who died of pyemia in the third week after the cessation of the last catamenial flow. The left ovary



contained a follicle  $\frac{3}{4}$  inch in diameter, distended by a recent non-adherent, softish coagulum, uniform in consistence and colour. This follicle was prominent above the adjacent surface of the ovary; and its superficial wall was thick, and presented no tendency to point or rupture. There was no recent rupture to be seen on the surface of either ovary.

(2) The second example was a woman who had undergone an operation for fistula in ano. The monthly flow made its appearance a week before the expected time for its return, and she died five days after. One ovary contained a follicle measuring  $\frac{5}{8}$  inch by  $\frac{1}{2}$  inch; this follicle contained a bright red, fresh, loose clot, and its walls were thin and not corrugated. From these characters it appears that the hemorrhage into the follicle had taken place but a short time before death.

(3) The next was a patient who had undergone an operation for the removal of an ovarian tumour. She died a fortnight after the operation, when she had menstruated for one day. At the inner extremity of the left ovary was a large, dark-coloured, softish mass, which, on section, proved to be a Graafian follicle containing a brick-red-coloured clot, which appeared to be of a spongy texture. It could with difficulty be turned out of the sac. After its removal it was seen that the wall of the sac was formed by a thin yellowish substance.

(4) The last example in this group was a person who suffered with fibroid tumour of the uterus. She died on the third or fourth day of the menstrual flow. Both ovaries were bound to the surrounding structures by tough and firm false membranes. The left contained a follicle nearly an inch in length, in which was found a softish, dark-coloured clot, having a spongy texture, which appeared to be several days old.

In the first and third members of this group hemorrhage had taken place into the follicle unquestionably before the appearance of the catamenial discharge.

In the second, hemorrhage had occurred before the flow had become due; but the latter, owing to surgical interference, having returned a week before its time, the hemorrhage took place while the discharge was in progress.

In the fourth, the condition of the clot makes it almost certain that the hemorrhage had taken place before the appearance of the catamenia.

*C. One case, in which a Graafian follicle had matured, but where neither rupture nor hemorrhage had actually occurred.*

This was a patient who died of typhoid fever just before the appearance of the catamenia. In one ovary there was an enlarged Graafian follicle, which was highly vascular, and projected like a nipple beyond the surrounding surface. It was evidently on the point of bursting, and it is doubtful whether rupture of the follicle or the appearance of the discharge would have taken first place.

*D. Cases, three in number, in which no Graafian follicle had become enlarged to the size exhibited by it at maturity.*

The first was a patient in whom the menstrual flow had almost ceased. There was no rupture in either ovary, but the right contained a Graafian follicle about the size of a small pea.

The next was a young suicide, who died three days after the cessation of the catamenial discharge. There was no recent rupture in either ovary, but the left contained a follicle similar to the one seen in the preceding case.

The last member of the series was a girl who died of peritonitis, caused by the rupture of an abscess on the right ovary. In the left was a Graafian follicle about the size of a small pea, but no recent rupture. The state of the lining membrane of the uterus showed that in this case menstruation was imminent.

Besides the appearances described, there were in all the preceding cases numerous Graafian follicles, varying in size from a millet-seed downwards, together with some superficial pits and atrophied corpora lutea.

These cases appear to me to bear out the opinion stated at the beginning of the paper, that, in the great majority of subjects, the discharge of ova takes place before the appearance of the menstrual flow with which it is connected; for in ten out of the fourteen rupture of a follicle, or hemorrhage into its cavity, had occurred before the return of the catamenia; in one it was doubtful whether rupture of a follicle, or the appearance of the discharge would have taken place first; in two a menstrual period had passed without maturation of a follicle; and in one a periodical discharge was imminent, though the ovaries contained no matured Graafian follicle. It is not improbable that the follicles which were found in the three last cases, and which were enlarged to the size of a small pea, would have become mature by the next return of the flow.

I have carefully considered the cases recorded by Cruikshank, Jones, Paterson, Lee, Girdwood, Negrier, Coste and others, and find that, though they do not contribute materially to the solution of the question discussed in this paper, yet, in so far as they go, they favour the view put forward here—a view which derives support from the custom imposed by the Levitical law, and observed to this day by the stricter sect of the Hebrew community.

#### POSTSCRIPT.

Since writing the above, I have had opportunities to examine two subjects in whom the date of the last menstruation was known.

The first was a girl, aged seventeen years, who died on the fifth day after admission to the Middlesex Hospital of traumatic tetanus. She was said to have ceased to menstruate just before admission; and the condition of the inner surface of the uterus confirmed that statement. The uterus and ovaries were small and imperfectly developed. On the surface of the right ovary was found a patch  $\frac{1}{2}$  inch in diameter,

slightly injected, and presenting a punctated appearance. In its centre was a cicatrix, appearing as a white spot, beneath which was situated a yellow body, elongated and irregularly flattened in shape. This appeared to be due to pressure from several Graafian follicles growing in close proximity to it, the largest of which was as large as a small pea. The yellow body measured nearly  $\frac{1}{2}$  inch in length; it had folded walls, and in its centre was a thin elongated clot, the middle of which was of a dark colour.

The second subject was aged twenty-six years; she died of Bright's disease. The last menstruation began May 13th, ceased May 19th, and death occurred May 28th, fifteen days after the appearance of the flow. Hemorrhage had taken place into the superficial tissue of the ovaries, probably by reason of the condition of the blood.

In the right was a small superficial prominence formed by a yellow body, which measured about  $\frac{3}{8}$  inch in diameter; it was throughout of a yellowish colour, and contained no trace of the colouring-matter of blood. On comparing these organs with one another and with those previously described, I am led to infer that in the first 12 to 14 days, and in the second about 20 days had elapsed since rupture of the follicle occurred.

Reichert has examined twenty-three organs in which signs of menstruation were recognisable. In four cases a Graafian follicle had matured but not ruptured, nor had hemorrhage taken place, though the decidua menstrualis was in a state of greater or less development; in eighteen cases a Graafian follicle had ruptured, and hemorrhage had taken place into the decidua; in one case only, in which bleeding had not begun, had a Graafian follicle been ruptured. The latter statement appears opposed to the conclusions at which I have arrived; but this is only apparent; for in one case a follicle had ruptured, in four a Graafian follicle had matured before hemorrhage began, and in one of these rupture was on the eve of taking place; in eighteen a follicle had ruptured, and hemorrhage had taken place into the decidua menstrualis. Put in this form, Reichert's cases are not opposed to the conclusions arrived at in the preceding note; and as his cases have not been described, it is not possible to say what their actual bearing may be. The conclusion arrived at by Reichert, after examination of the twenty-three specimens, however, is that rupture of the Graafian follicle takes place at an early stage of the menstrual flow.

## *Obstetric Summary.*

### *Adherent Twins.*

Professor Valtuta relates an interesting case of labour with conjoined twins, fatal to both mother and children. The following is an abridgment of his narrative:—

The subject was a woman, aged twenty-five, who had already had



three normal pregnancies. In her fourth, the only thing which troubled her was the great distension of the abdomen. On June 8th, she being at about full term, labour pains set in, and a midwife was sent for, who, on arrival three hours after the commencement of labour, found the waters escaped, the os uteri fully dilated, and a head presenting. After a time, as the pains were very slow, she gave ergot, under the influence of which the head was expelled; but, although the midwife endeavoured to assist labour by hooking her finger into the axilla, no further progress was made beyond bringing down an arm. A medical man was then sent for, who, on his arrival, made fruitless attempts by means of traction with his hand and the blunt hook to bring down the body. An obstetric physician was next summoned, who, finding a third arm, diagnosed the presence of twins, either separate or united. All attempts at delivery being unsuccessful, and the patient being much exhausted by her suffering and by a sanguineous discharge which had continued from the commencement of labour, she was sent, accompanied by a medical man, to the hospital, where she arrived after a journey of three hours, nearly nineteen hours after the commencement of labour.

On admission, she was pale and cold, her voice feeble, and her pulse almost imperceptible. There was a sanguineous oozing from the vulva; and from it projected the head, arms, and nearly the whole chest, of a fetus. The head was extremely cyanotic; the left arm was attached only by a small portion of skin and cellular tissue; the right axilla was torn through as far as the joint; the soft parts of the chest were also lacerated from one axilla to the other, and several ribs were broken. The remaining parts of the fetus were immovable in the vagina and uterus. A third arm was felt in the posterior part of the vagina. The abdomen was voluminous and hard; and uterine contractions took place at long intervals. Neither fetal movements nor cardiac pulsations could be perceived. Dr. Valtuta diagnosed adherent twins; setting aside the idea of an ascitic fetus in consequence of not being able to detect fluctuation through the woman's abdomen. Having consulted his colleagues in the hospital, Dr. Valtuta searched for and brought down a foot, on which he made traction; his endeavours, however, were in vain. He then proceeded to break up the presenting portions of the fetus; and, having done this, he brought down another foot. Traction being then made on both the feet, delivery was completed an hour after the woman's arrival at the hospital. The placenta was single; and there was one umbilical cord. The woman died of exhaustion five hours afterwards. At the post-mortem examination, a laceration of the anterior part of the neck, about an inch and a half long, was found.

The fetuses were adherent anteriorly from the genital organs to the lower part of the thorax. One was of normal conformation, but had a supernumerary finger on the right hand; the other was hemicephalic, and had spina bifida. At first sight there was no appearance of anus or of genital organs. But, on drawing aside the right leg of



the well-formed fetus from the left of the hemicephalic one, there were seen female genital organs, an anus, and, a few finger-breadths above, an umbilicus, from which proceeded the single cord. On separating the limbs in like manner on the other side, no trace of similar parts could be found; but, at the point where the umbilicus should have been, there was a funnel-shaped depression which admitted the finger, and through which a probe could be passed till it came out above the umbilicus on the other side. The fetuses were well nourished, and, with the exception of the hemicephalia and spina bifida, of equal proportions. The absence of all signs of putrefaction, and the cyanized condition of the head already mentioned, indicated that their death had not taken place until labour had commenced.—*Giornale Veneto di Scienze Mediche*, Agosto, 1875.

---

*On Habitual Death of the Ovum and its Artificial Expulsion, with Remarks on the Pathology of the Placenta and the Umbilical Cord.*

Dr. Leopold (in *Arch. für Gynäkol.*, Band viii. Part II.) contributes a valuable article on this subject. His conclusions are, briefly:—The habitual death of the ovum may be due to various causes. *a.* Syphilis in the parents. *b.* Anemia, or blood changes in the mother. *c.* Chronic metritis and uterine irritation. *d.* General individual irritability. *e.* Inherited disposition. *f.* Changes in the placenta and umbilical cord. Rational treatment depends upon the cause—anti-syphilitic treatment where this is indicated, strengthening the system, curing local disease, and otherwise obviating the exciting cause.

Artificial induction of labour at the thirty-seventh week, or two weeks before the usual termination by death, is the plan suggested for saving the life of the child. The time chosen will depend upon the state of the fetal circulation and the probable cause of the premature death of the fetus.

---

*Ovariectomy with Pregnancy—Cæsarian Operation—Cure.*

By THOMAS HILLAS, M.R.C.S. Eng.

At a meeting of the Medical Society of Victoria, held December, 1874, the following case was related:—Mary M'C., aged twenty-four years, single, was admitted to the Ballarat District Hospital, 4th June, 1872. The history of her case was peculiar. She believed that she became pregnant in March, 1871, and, not wishing to be confined in the district in which she lived, she sought admission to the lying-in ward of the Ballarat Benevolent Asylum. She was admitted there in November, 1871, and after staying there until the following June, a consultation of the honorary staff was called, and she was discharged, her case being deemed ovarian dropsy, and not pregnancy. On her admission to the hospital she was examined by the resident surgeon, and subsequently by the honorary surgical and medical staff, all agreeing that she was suffering from ovarian dropsy,

and that it was a suitable case for operation. On 13th June, assisted by the honorary surgeons, Messrs. Nicholson and Whitcomb, and the resident surgeon, Mr. Owen, and the honorary medical staff, the patient being under chloroform, I commenced the operation, by an incision midway between the umbilicus and pubes. On arriving at the peritoneum, I made a small opening into it, when out spurted a large jet of venous blood which the pressure of the finger controlled. I came to the conclusion that I had wounded, unwittingly, a gravid uterus, and, feeling sure of this, I extended the first incision upward to the umbilicus, when a large uterus rolled out on to the thighs, and the ovarian sac protruded. This was tapped, and about eleven quarts of fluid were drawn off; there were but few adhesions, which were easily broken down, and there was no hemorrhage. The sac contained about a dozen small cysts, but the external wound being large, there was no occasion to tap them. The pedicle was short and thick, and, after being tied firmly with a double whipcord ligature, the clamp was securely applied, and the pedicle divided, the ends of the double ligature being tied over the ends of the clamp. Now came the difficulty. The uterus was all this time lying on the thighs, with a fetus in it, and a wound through its muscles probably into the placenta. Some of the bystanders advised that the wound in the uterus should be sewn up, and that organ replaced in the abdomen; but seeing that labour must come on soon, and that the rupture of the uterus would most likely occur at the seat of injury, I personally decided to perform the Cæsarian operation, as being the most likely means of giving the patient a chance to recover. The uterus was incised to about five inches, and the placenta and a fetus, alive, and well developed, at about the eighth month of gestation, extracted. I then stitched up the wound in the uterus, with about nine or ten silver-wire sutures, carefully tucking the cut ends down into the incision. Immediately on completing this the uterus contracted firmly. I then sewed up the wound in the abdomen with deep and superficial stitches, the deep stitches including the peritoneum, leaving the clamp at the lower margin of the wound, and a good deal dragged upon. The right ovary was the one affected, and the patient measured sixty inches round the abdomen before the operation. The sac and its contents, after removal, weighed thirteen pounds, and are preserved in the hospital dispensary. The patient vomited for about forty-eight hours after the operation, having been an hour under chloroform. This was relieved by morphia and ice, and on the fourth day all unfavourable symptoms abated. There was a discharge of pus from the lower portion of the wound, which ceased in about a fortnight, and then it completely healed. She was discharged, cured, at the end of six weeks. On 3rd July, a month after the operation, she menstruated moderately for four days, and again on 28th August. I have seen her several times since, and she is in perfect health.—*Australian Medical Journal*, February, 1875.

*The Obstetric Arms of Honiton.*

A correspondent writes to the *Med. Times and Gazette* as follows : —“The ancient coat-of-arms of the Borough of Honiton, in Devonshire, exhibits a pregnant woman praying to an idol, with an obstetric hand at the top, and a branch of honeysuckle below. The origin and meaning of this singular device, which is said to be of Saxon origin, are quite unknown, though it is said that they commemorate a time when the town contained a large number of barren wives. The “obstetric hand” is a right hand, with the little and ring finger doubled down, and the fore and middle fingers extended, as if about to perform the act of examination, which the French call *toucher*.

## Gynecic Summary.

*Wernich on Hematoma of the Vulva and Vagina.*

Judging from modern literature, it would appear to be a rare affection. Valenta gives but four cases as coming under his own observation. There are a few other isolated cases reported in the *Vienna Press*. Thirty or forty years ago judging from the then literature, its rarity was by no means noticed. Winckel collected between 1400 and 1500 cases from writings of that date and previous to it. This great disparity in the records arises, the author believes, from its formerly being considered most dangerous, and consequently more worthy of note.

Deneux's cases tended to support this view, as out of sixty instances collected by him twenty-two died, or a mortality of thirty-eight per cent., which, with the exception of cancer and ovariectomy, gives a higher ratio than any other affection that implicates the female genital organs. Later experience has falsified this high estimate. Out of fifty collected by Winckel he found only six fatal, or only twelve per cent. In order to indicate what line of treatment should be adopted, and the etiology, the author reports a case he met with in a primipara, aged twenty-one, who was prematurely delivered of an eight-months child, the result of a fall three days previously. The labour was natural, lasted but six hours ; pains strong. Immediately after parturition the patient was seized with sharp pain in the rectum and vulva, which caused her to faint. When first seen, a blue, boggy tumour, about the size of an ordinary placenta, was found distending the lower part of vagina and vulva. Morphia was hypodermically injected to relieve the urgent pain, and cold cloths were applied and the hips raised. Temperature rose on fourth day. Tumour the same, perhaps a little softer. Temperature rose. Disinfectant injections ordered on the sixth day. It broke spontaneously. Sausage-like lumps of coagula came away all that day and the next, in all about 16 ounces. The pain left the patient, and the temperature fell directly, and continued so. Disinfectant injections were persevered with. Was quite well and up by the fourteenth day. When carefully



examined, there was no swelling or any remains of the opening to be found.

Its pathology is simple—an infiltration of blood from a ruptured vessel into the cellular tissue beneath the mucous membrane or skin. It is rarely arterial, generally venous. The hemorrhage proceeds usually from the corpora cavernosa, or the clitoris, which was the case in this instance. Between the clitoris and the orifice of the urethra there is a large plexus of veins with remarkably thin walls. The forceps appears to have no effect, as out of fifty cases collected by Winckel they had only been applied seven times; nor does there seem to be any connexion between a varicose condition of the veins of the labia or legs, as out of these very fifty cases of Winckel's only six had varicose veins.

Scanzoni only met with one instance out of fifteen cases where there was any varicose state of the veins of the body. D'Outrepointe reported four, and Braun eleven cases, without varicose veins. As to its treatment, Hohl proposed to act tentatively, and leave the hematoma alone. Scanzoni, however, advises the same treatment as long as the tumour goes on diminishing in size, but as soon as the active process of absorption becomes arrested, recommends an opening to be made, "and the coagulated blood turned out, and any fresh profuse hemorrhage to be stopped by ice-cold water injections into the sac, or by lumps of ice, or a bladder of ice-water placed in the vagina, which, according to the danger of a return of bleeding, may be left in for six or eight hours. Should these means not arrest the hemorrhage, pledgets of charpie dipped in cold water must be thrust into the cavity of the wound, and the vagina most carefully tamponed." Schröder directs the very reverse method of treatment, stating that "the most favourable result is always in reabsorption without opening the swelling." Wernich, however, is inclined to the opinion that the best plan to adopt is to allow the process of absorption to go on as long as it proceeds actively, but on account of the tediousness and delay there must necessarily be in the entire removal of so large a mass of blood, he holds an incision in reserve, trusting to a spontaneous emptying of the sac as long as possible. He rests this recommendation upon the results of Winckel's cases, as out of six deaths following incision there were sixteen whose results were favourable; four from reabsorption, whereas, with one exception, twenty-three got well from bursting and emptying of the sac within eight days of the accident. In the discussion that ensued on this communication the majority of the members present favoured a passive procedure in the treatment of hematoma of the vulva, supported by personal experience in the management of this and other like blood tumours. Louis Mayer maintained that the possibility of absorption depended on whether the blood remained fluid or not. If it coagulated it would continue for years. He instanced a case of Virchow's, who describes a hematoma of the iliacus in which blood corpuscles were recognisable after three and a half years' duration.—*Beiträge zur Geburtshülfe und Gynecologie*. Band iii. Berlin, 1874.



*On the Influence of Heart Disease on Menstruation, Pregnancy, and the Fetus.*

Dr. Duroziez (in *Arch. de Tocologie*, Nov. 1875) concludes a series of papers in which he has entered somewhat fully into the question.

Although patients suffering from heart disease may go through labour without any severe accidents, and recover perfectly, the malady not even being aggravated by pregnancy, yet generally miscarriages or premature labours are of frequent occurrence. There is risk both to the mother and child.

The treatment is the same as in heart disease uncomplicated by pregnancy, but this latter forces upon us the question of inducing labour. And were it not that there are dangers incidental to the remedy itself, we could resort to it with less doubt and hesitation than we do at present. The risk to the fetal life is of course increased; but this is already seriously compromised, and the danger to the mother is unquestionably materially lessened.

*On the Uterine Lymphatics, and their influence in Uterine Pathology.*

Dr. Lucas-Championniere (in *Arch. de Tocologie*, October, 1875) insists upon the necessity of avoiding injury to the cervix during labour, especially as regards unnecessary interference. When labour is completed, he considers excessive cleanliness, in the way of washing, injections, and extraction of clots, to be as harmful as excess of negligence.

When labour is prolonged and causes pressure upon the neck of the uterus or vagina, it is wise to aid the forces of nature by the judicious and timely application of the forceps.

The value of sulphate of quinine has not been properly estimated, owing to the small doses usually employed.

*The Dysmenorrhœal Membrane.*

Dr. P. Finkler, of Kiew, has examined seven specimens of dysmenorrhœal membrane obtained from four patients. One of these was under observation in the hospital for three months. During that period she menstruated three times, and passed a membrane on each occasion. Dr. Finkler believes that the membrane is the mucous membrane of the uterus, inclusive of the blind extremities of the glands, but that in some cases the deeper layer of the membrane and the terminations of the glands remain in the uterus. The structure of the seven membranes examined was essentially the same, but with varying proportions of blood, round cells, and epithelium, and similar to what had been described before as the structure of the uterine mucosa during the early part of menstruation. The names "apo-

plectic," "villous," "inflammatory," which have been given to different states of the membrane by different authors, depend on the quantity of blood, and granulation or round cells, found in the tissue; the villous character of the membrane depends on strings of fibrin which, in some cases, appear attached to its inner surface. The membrane is the product of disease, and not of impregnation and abortion, as was shown by the case referred to above. Nor is it the product of the increased physiological action which takes place in the generative organs periodically; but it is the result of a pathological condition which is to be looked for in the character of its elements, in the great accumulation of granulation cells, many homogeneous, others granular, some nucleated; in the blood and fibrin found in the tissue; in the epithelium swollen, glistening, or lost in the flocculent outer surface of the membrane; persistence of the disease, its recurrence after cold, abortion, labour, acute inflammation of the uterus; the condition of the uterus itself, its false position, and abundant flow of mucus (often with acid reaction) from its cavity. All the membranes examined gave the amyloid reaction with iodine and sulphuric acid, and Dr. Finkler thinks this is probably the real pathological condition. The cause of the separation of the membrane he finds in the accumulation of granulation cells in the mucous and submucous tissue, and in the flow of blood, which in some cases takes place into the whole of the mucous membrane, and in others only into the deepest layer of it; in consequence of these two forces the mucous membrane becomes detached, acts like a foreign body, and excites uterine contractions.—*Med. Times and Gazette.*

---

#### *Early Menstruation.*

"M.D." writes: "I have now under my observation a girl, aged two years, who has regularly menstruated for some time. She is in perfect health, and her courses, which are in every respect natural in colour, occur every month, causing her much discomfort and pain. I may also mention another curious case: my patient, a girl aged eight, had been subject to fits for two years when I first saw her. The regularity with which they occurred (about every four weeks), induced me to ascribe them to some uterine derangement, particularly as she complained at the same periods of intense pain over the pubes and sacrum. Upon examining her, I discovered the pubes covered with thick black hair, and a perfectly formed penis (excepting the absence of urethra), over two inches long, and capable of complete erection. Below the penis is a rather large urethra, and on each side a large and pendulous vulva, which has very much the appearance of testicles; so much so, that the parents always had their doubts as to sex of their child, never having consulted a medical man previously. The symptoms from which she suffers every month are similar to what occurs before and during ordinary menstruation."—*Brit. Med. Journ.*

## Pædiatric Summary.

### *On Surgical Anesthesia in Infants by the Administration of Chloral by the Stomach.*

Dr. Bouchut (in the *Bull. Gén. de Thérapeutique*, Oct. 30th, 1875) gives his conclusions as regards the action of chloral in infants, which differs materially from that in adults. It may be given in doses up to forty-five or sixty grains, without provoking pyrosis, gastritis, vomiting, or diarrhea, and it produces anesthesia more or less complete. He has employed it repeatedly in cases of chorea, cerebral rheumatism, opening abscesses, and tooth-drawing. Sleep and absolute insensibility have been obtained by a dose of forty-five to sixty grains given in one draught.

A similar result may be obtained by means of suppositories, but the rectum is more irritable than the stomach, and tenesmus, heat, and irritation occur if the method be employed for many days. The stomach is the best channel for the administration of chloral in infants. A quarter of an hour after taking the full dose anesthesia commences and is completed at the end of an hour.

When it is necessary to apply Vienna paste to a nevus, or to open an abscess, or draw a tooth, chloral is indicated.

The child may groan and move, without waking, whilst the operation is being performed, but then falls again into a passive condition, and on awakening some quarter of an hour later, it is unconscious of having suffered pain, and has been spared the fright of seeing the operator.

## BOOKS, PAMPHLETS, AND PAPERS RECEIVED.

"A System of Midwifery, including the Diseases of Pregnancy and the Puerperal State." By William Leishman, M.D. Second edition. Glasgow: James Maclehose. 1876.

"The Uterine Souffle and the Fœtal Heart." By James Cumming, M.D. Edinburgh: Oliver and Boyd. 1875.

Communications, &c., from Dr. Meadows, Dr. Routh, Dr. George Calderwood, Dr. James Clapperton, Dr. Robert Bell, Dr. J. Ashburton Thompson, C. J. Cullingworth, Esq., W. Draper, Esq., Dr. Thomas Radford, Dr. Henry, Dr. Edis, Dr. Fordyce Barker, and Dr. Godson.

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.

# THE OBSTETRICAL JOURNAL

OF  
GREAT BRITAIN AND IRELAND.

---

No. XXXIV.—JANUARY, 1876.

---

Original Communications.

---

## ON LACERATION OF THE GRAVID UTERUS.

By J. ASHBURTON THOMPSON,

Fellow of the Obstetrical Society of London, and Surgeon-Accoucheur  
to the Royal Maternity Charity.

THE recent trial\* of a duly qualified practitioner for manslaughter, in a case where the death of his patient resulted from rupture of the uterus, alleged to have been caused by malpraxis on his part, lends a special interest just now to a review of the modes in which that injury may arise, and the symptoms by which it may be manifested. The circumstances of this case and trial not only suggest a recapitulation of our previous knowledge of the subject, but may well be taken as showing the need of a more critical inquiry, inasmuch as the evidence for the prosecution was based almost entirely upon the assumption that a rupture of the uterus occurring during labour must at once have betrayed itself by obvious symptoms. To this assumption the defence appears to have offered no opposition. It is a little surprising, under the circumstances, that such unanimity of opinion should have prevailed upon this point; but reference to the authorities most likely to have been

---

\* Regina v. Wood. Lincoln Spring Assizes, 1875.



consulted in a medico-legal case affords a partial explanation.

If the different text-books of Obstetric Medicine be compared, it will be seen that they agree very closely in their descriptions of the symptoms of laceration of the uterus during labour; and from them it is plain that in the event of a case presenting itself with the symptoms they describe, those symptoms would be at once sufficiently obvious and sufficiently alarming to attract the attention even of an uninstructed observer, and would lead the accoucheur to diagnose their cause correctly. This is as it should be; the description is of a typical case. But if this comparison be followed further, it will also be found that the text-books agree quite as closely upon another point, and that is in the cursory manner in which they refer to what, it may be inferred, are looked upon as exceptional cases of ruptured uterus—cases in which many or all of the symptoms by which the accident may generally be recognised are wanting. From the accounts generally given the reader is compelled to conclude that a case which is deficient in any of the graver symptoms most usually present is very rare at all events, even if not absolutely phenomenal.

In the ordinary cases of disease, perhaps the first thing which the student learns, on bringing his book knowledge to the test of clinical experience, is that the presence of *every* symptom, the occurrence of which constitutes a typical instance of the disease, is practically rare, and not necessary to its correct diagnosis. Although not generally asserted, it is equally true of the injury under consideration, that it does not on every occasion give rise to all the symptoms which are commonly described as appertaining to it. But the usual opportunity of clinical experience does not present itself in the case of rupture of the uterus, for that is a comparatively rare injury; and one is therefore prone to entertain—what is perhaps the most natural impression—that a lesion not only so grave, but in every case anatomically so similar, must necessarily be accompanied by the grave and noticeable symptoms described as characteristic of typical cases; and it would therefore be assumed that the

absence of those symptoms was proof of the absence of the injury, or what is almost the same thing, that the typical is the most common form in which it manifests itself. Whether this be so it is my purpose to inquire.

From the tone and nature of the evidence given at the trial referred to, it appears that the occasional occurrence of cases in which the rupture gives rise to *no* immediately noticeable symptom, is not quite so widely known as it might be; but I now desire to draw attention not so much to that class of cases as to the large number of recorded instances in which *many* of the graver symptoms are wanting, with a view of removing the impression (which I believe is justly received from most obstetric works) that such cases are rare. The limited clinical experience of this accident, which is, fortunately, all that is possible in any single practice of an ordinary kind, may be supplemented and extended by that derived from the isolated clinical reports of others; and I propose now to analyse twenty-three cases of rupture of the gravid uterus, recorded in the periodicals during a few years last passed. From them I shall show that it is unusual to find a complete set of symptoms present in any one case; that very often it is the more obvious symptoms—those upon which most value is set in making a diagnosis—which are absent; and that a typical example is, in fact, the exception, and not the rule, as it appears to me is ordinarily supposed.

Apart from the abstract value of a precise knowledge of the manner in which this injury shows itself and of its importance in medico-legal questions, it is often of the greatest moment for many reasons that the accoucheur should be prepared to recognise its occurrence as early as possible. In the first place, immediate delivery affords the patient almost her only remaining chance of recovery, and the sooner she is delivered the better that chance will be. As to the medico-legal question, the consideration is of a more personal but not less important kind; for should the attendant find the patient in a condition necessitating immediate delivery, and should she die of rupture of the uterus after an operation performed to effect delivery, without his having stated the

nature of the accident which rendered his interference necessary, he will certainly be suspected of causing his patient's death ; and he can scarcely succeed in clearing himself of blame if he have failed to recognise the possible cause of her danger before proceeding to operate. By announcing his suspicions in the first place he may perhaps succeed in warding off an imputation which is sure to be made after the fatal event, and which is most often entirely without foundation. Before proceeding to the analysis, I will recite the description of the symptoms of rupture written by Dr. Andrew Douglas. I select this account not only because it is concise and graphic, but because it has been copied by Rigby, who gives it in place of any description of his own, in his "*System of Midwifery* :"—"When a rupture of the uterus has really happened it is generally marked by symptoms which are decisive ; but it being a case which occurs so very rarely, they do not immediately create suspicion. When labour has continued violent a considerable time, if a pain expressive of peculiar agony be followed by a discharge of blood and an immediate cessation of the throes, there is reason to apprehend this mischief. If nausea and languor succeed, with a feeble and irregular pulse, cold sweat, retching, a difficulty of breathing, an inability to lie in a horizontal posture, faintness or convulsions, there is still more reason to suspect the nature of the case. But if the presenting part of the child which was before plainly to be distinguished has receded and can no longer be felt, and its form and members can be traced through the parietes of the abdomen, there is evidence sufficient, I believe, to determine that the uterus is ruptured. The labour pain in consequence of which the rupture is supposed to have happened is often described by the patients as being similar to cramp, and as if something were tearing or giving way within them. It has been said likewise to have produced a noise audible to the bystanders." Such, on the one hand, is a concise description of the symptoms as they are usually given ; and to them a very few others are added by some authors. On the other hand, Rigby refers to exceptional cases (under the heading of "*Premonitory Symptoms*" alone), in these words :

"In many cases, especially where the muscular substance of the uterus only is torn, the pains have continued in a sufficient degree of power to expel the child ; in others the mischief has been attended with so little suffering at the moment, and for the time with so little constitutional derangement, as to excite no suspicion, either on the part of the patient or her attendant." Under the heading of "Symptoms" he makes reference to no exceptions ; and the former can scarcely be taken to apply to cases showing any remarkable or persistent absence of the usual symptoms. Burns refers to exceptions in the following short sentence only :—"These symptoms do not all appear in every case, nor come on always with the same rapidity." But the immediate illustration of this opinion by a single example in which there was not at any time either during labour or afterwards a single bad symptom, and which is moreover an example of rupture of the vagina and not of the uterus, sufficiently indicates the writer's opinion that the exceptions which he hints at are truly remarkable. Tyler Smith says : "The symptoms which denote the actual occurrence of rupture of the uterus are generally very marked, though cases sometimes occur in which the evidences of the accident are so uncertain that it cannot positively be known until after death." And, farther on : "In some cases . . . the dangerous symptoms come on some hours, or even days, after the accident." Yet even this author, whose relative account of cases and exceptions I believe to be more complete than any other, does not refer to any example in which the patients have recovered without manifesting *any* symptom (the evidence of rupture depending upon an accidental discovery of the wound), nor to the relative frequency with which certain leading symptoms are absent, and in which, therefore, the diagnosis is on this account difficult and uncertain. Beyond the statements quoted, which are vaguely modified by the words "occasionally" and "sometimes," nothing appears from which the reader may infer that exceptions either partial or complete are met with in any great degree of frequency ; and the space occupied by his description of the type is such as to lead naturally to



no other inference than that they are very unusual indeed. Dr. Leishman refers to exceptions in two sentences. In the first he says : "In some cases the occurrence of rupture is not marked either by acute pain or by the symptoms above enumerated, and the dangerous condition of the patient may only become apparent after a considerable period has elapsed—it may be hours, or even days." So that the alternative is offered of cases presenting all the symptoms and of cases presenting none of them, save long subsequent collapse or peritonitis. These are two examples which might have been made more extreme by reference to those in which even the latter symptoms never appear. Numerous examples may be found, however, of every grade of severity between these two ; and to one of them the author refers in the second sentence I have alluded to. Speaking of retrocession of the presentation, which is a result of occasionally occurring relaxation of the uterus towards the termination of the second stage of a normal labour, the reader is reminded that "in some cases (of rupture) it would appear that the sudden cessation of pain was the only symptom of any importance." Sudden cessation of pain is too common an incident during natural labour, and (as will be seen hereafter) unless accompanied by other symptoms, too uncommon after laceration of the uterus, to render it of much diagnostic value ; for when arising from the last-named cause the throes far more usually continue until the uterus is emptied—either in the natural way or into the abdominal cavity ; or else they cease quite gradually. This writer then, like those already quoted, makes no attempt to show the relative frequency with which particular symptoms—whether one or many of them—are absent ; and his account also allows of no other inference than that typical cases are the rule. I now propose to show in what proportion of the twenty-three cases I have collected the leading symptoms of laceration were absent, and this, I think, will best be done by taking these symptoms singly. The symptoms themselves are seven in number, and they will be considered in the following order—viz. : 1. Violence of the throes before rupture. 2. A peculiar pain at the time of rupture. 3. Hemorrhage.

4. Immediate cessation of the throes. 5. Retrocession of the presentation. 6. The speedy occurrence of collapse. 7. Convulsions. I propose also to make a few general remarks on each symptom in the order in which they are dealt with. The cases may be found by the following references, each of which is followed by the author's name:—The *British Medical Journal*. Case 1. 1871, vol. ii. p. 467 (Clements). 2. 1871, vol. i. p. 475 (Kilburn). 3. 1870, vol. ii. p. 187 (Denton). 4. vol. ii. p. 602 (Bryan). 5. 1869, vol. ii. p. 452 (Mitchell). 6 and 6a. 1866, vol. i. p. 92 (Cox). 7 and 7a. 1864, vol. i. p. 344 (Llewellyn). The *Lancet*. 8. 1873, vol. ii. p. 899 (Lowseley). 9. 1870, vol. ii. p. 633 (Tylecote). 10. 1870, vol. ii. p. 876 (Kay). 11. 1867, vol. i. p. 732 (Cullingworth). 12. 1866, vol. ii. p. 64 (Dalton). *Dublin Medical Press*, N.S. 13. 1862, vol. vi. p. 92 (Copeman). *Dublin Quarterly Journal*. 14. 1857, vol. xxiv. p. 450 (M'Clintock). *Medical Times and Gazette*. 15. 1859, vol. ii. p. 502 (Aveling). *Transactions of the Obstetrical Society of London*. 16. vol. viii. pp. 184—187 (Radford). 17. vol. lx. (Madge). 18. vol. ix (Dunn). They comprise nine published in the *British Medical Journal* between the years 1864 and 1871; five from the *Lancet* between 1866 and 1873; three from the *Obstetrical Society's Transactions* for the years 1867 and 1868; and single cases from other journals between the years 1857 and 1868, including three originally reported by Dr. Jolly in the latter year.

1. *Of the Character of the Throes preceding Rupture*.—It is stated that in those cases in which rupture is about to occur, the throes are either excessive in force or continuous, or excessive in force but short; partaking therefore, as it would seem, rather of the nature of tonic and clonic spasm respectively. They produce no alteration in the position of the fetus. Great stress is not, however, generally laid upon the frequency of this symptom, and it does not often occur in this series of cases. In eleven of them (2, 3, 6, 7, 7a, 8, 9, 11, 14, 16, and 19) the throes appear to have been of the usual kind—in some instances, perhaps, weaker than usual. Of seven others, in one it is doubtful whether there

was at any time any regular contraction of the uterus ; in two, the rupture occurred before labour set in ; and in the other four the laceration was traumatic, occurring under operation. Of the remaining five, in which the throes were modified, in Case 4 they were "violent" during the last two hours of labour ; in Case 6*a* they were "violent and expulsive" (that is, I presume, apparently expulsive) ; in Case 12, "almost continuous" during the last six hours ; in Case 18, they were "frequent and excruciating," but particularly so at the last (when the rupture occurred) ; and in Case 21 (in which the predisposing cause of rupture was a fall downstairs, causing the death of the fetus eight days before labour set in) the throes continued with moderate force for five days ; but on the fifth a violent throe occurred, attended by a noise of breaking, which was supposed to have been caused by the laceration which then happened. Thus, omitting the seven cases already excepted, in five out of sixteen the throes were modified before rupture occurred ; but from these five the last must be subtracted, if this symptom be regarded as a prognostic, for it was only the throe attended by rupture which was at all peculiar. 26·6 is the percentage, then, in which the throes were peculiarly aggravated before rupture in this series of cases.

Regarding this spasmodic contraction as a prognostic of rupture, I would observe, that there is some reason to think that there is a cause for it peculiar to those cases in which rupture does actually occur. I base this opinion upon the fact, that while undue prolongation of labour, detachment of the placenta, or the excessive action of a dose of ergot are each a sufficient cause of spasmodic throes, rupture does not usually ensue upon them ; while, on the other hand, in the five cases which I am now considering it does not seem likely from the reports that any cause can be definitely assigned to the spasm which was observed. But I surmise that the cause might have been discovered on a more minute examination of the uterine walls, which might have been found to be, for example, of varying degrees of thickness in the same organ, or the muscular fibre to have been in a generally morbid condition. Another reason for this opinion may be

found in the generally accepted fact that these spasmodic throes are of graver import when, notwithstanding their apparent violence, they produce no effect upon the position of the child ; a condition which indicates a contraction which is unequal or irregular as well as spasmodic, and therefore probably owing to a peculiarity in the distribution or condition of the contractile tissue.

It will be misleading, however, to hold the opinion that rupture is to be apprehended only when the pains are either excessively violent, or violent and ineffective. For example, in primiparous women it is most often the case that the os uteri yields reluctantly ; and occasionally in young and vigorous persons, in whom the membranes have broken somewhat prematurely, it is obvious that the os is undergoing a forcible dilatation—that it is, in fact, being expanded almost entirely by the precipitate passage of the fetal head through it, instead of by the more benign gradual process which nature has provided for the purpose. Under these circumstances the pains can scarcely be considered excessive ; they are certainly not spasmodic ; and they are not without effect upon the position of the child, which is, indeed, being thrust too rapidly into the world. Yet in these cases the os uteri is exposed to the most imminent danger of rupture, and, I believe, very often indeed is ruptured to a small extent and without any serious result. When this accident is happening, the patient, although she may have conducted herself with calmness and patience hitherto, is unable to restrain her cries any longer, although the throes are apparently no more than she has suffered for some time past. Upon inquiry it will be found that she feels a pain in addition to that of the throes, which she refers to the sacrum and hips ; and it may be further noted, that upon the complete dilatation of the os, and the entry of the head into the pelvic cavity, this pain disappears, and the patient is again able to bear her sufferings with fortitude. This, no doubt, is the same kind of pain which is generally said to occur at the time of rupture (in another kind of cases), and which is accompanied by a characteristic scream. Should laceration happen in this way, it is usually a very slight rupture, although of course



subject to indefinite extension—so slight as to escape detection by the finger among the inequalities of the os of a recently evacuated uterus, and need not of itself give rise to any after symptom. It may, however, be sometimes diagnosed immediately after delivery by a continuous, bright-coloured hemorrhage, which may be considerable in quantity, and which is not controlled by contraction of the uterus. This organ may be as hard as the typical cricket-ball, and yet the hemorrhage continue. It may be arrested by making firm pressure upon the lower part of the uterus with the fingers forced towards the abdominal cavity just above the pubes, the palm being applied to, but not pressing, its body. Considerable hemorrhage is a rare consequence of the severer lacerations.

In another and larger class of cases, so far from there being any tetanic contractions, the throes are modified in the contrary way—becoming feeble and infrequent; while in the remainder of the 73·4 per cent. they are unaltered. Should the properly so-called tetanic contractions be observed then—although that which prognosticates rupture may not be distinguishable from that which is owing to one of the before-named causes, and although such tetanic contractions may be in all cases an indication for delivery—yet it will be more satisfactory if it can be correctly traced to one or other of these causes, for in that case rupture is not so likely to occur. Of the other modification of the throes—their failure, sudden or gradual—I will speak in considering the whole question of diagnosis of rupture of the uterus.

*Of Pain occurring at the time of Rupture.*—Simultaneously with the occurrence of rupture, and in consequence of it, a pain is said to be experienced which is sudden and excruciatingly sharp. Should it coincide with a throe, however severe that may be, this pain is said to be easily distinguishable from it, and so severe and peculiar that the patient is generally represented as screaming out upon its occurrence. Exceptions are not noted save in the cursory and general manner already described.

In four of the twenty-three cases (1, 10, 15, and 20) this pain was not observed; for in one the rupture was imme-

diately caused by a fall downstairs ; in the second, by version under chloroform ; in the third, during attempted version ; and in the fourth, the injury was a laceration caused by spicula of bone, removed during craniotomy. These cases I have excluded on account of their being traumatic. The remaining nineteen may be divided into two sections. In eleven of them (2, 3, 4, 6, 7, 8, 11, 12, 14, 16, and 19) there was no pain observed. In eight it was noted ; in Case 5, the uterus ruptured spontaneously, with characteristic pain before labour set in, consequent upon the patient being frightened. In Case 9, at the third hour of labour, a rather acute pain was felt in the abdomen, on which the patient jumped up on her knees, in which position she was found some time afterwards. In Case 13, before labour set in, a violent and painful movement of the fetus was felt ; subsequently the patient vomited, and felt a "very sharp" pain in the abdomen. In Case 17, the bowels being relieved at the fifteenth hour of a very tedious labour, a "sharp" pain was felt in the hypogastric region. In Case 18, the latter throes, especially the last (when the rupture occurred) were excruciating. In Case 21, at the fifth day of labour excruciating pain, with audible sound of rupture, is reported. It will no doubt be conceded that the tendency, in a matter of this kind, is rather to the use of adjectives of excessive force than the reverse ; and yet the word "excruciating" is seldom employed in this series of clinical reports. Be that as it may, however, it appears that an extra pain was observed at the time of rupture in the proportion of about 42·1 per cent. ; while in about 57·8, or in nearly two-thirds, it was absent. It is not plain, however, in all of these cases in which aggravation of pain was noted, that the pain was really such as could properly be called symptomatic. By that term I intend to distinguish the pain which is described by all authors in similar phrases. These phrases imply that it is not only excruciating, but sudden and distinct from any other pain incidental to labour, so that a person prepared to feel even very severe pain in the course of delivery is yet taken by surprise by the abrupt attack and peculiar nature

of this pain, and screams involuntarily, quite as much, probably, from her being unprepared for the suddenness of its accession, as from its actual severity and character. The development of this symptom, the sudden scream, upon which much value would appear to be put in the diagnosis of the injury, depends upon too many accidental circumstances to recur with any great frequency in a series of cases. The pain, which is its immediate cause, takes its origin probably only in the process of tearing of the uterine tissue. I have already spoken of a minor degree of laceration in which, no other tissue being injured, a pain distinct from that of the throes is experienced. In addition to the pain from the laceration, it does not appear that any severe pain is caused in the peritoneum by contact with the child, or with internal effusions of blood, until inflammation has begun. The amount of pain owing to rupture will depend, then, certainly upon the cause and mode of rupture. Thus when disease of the uterine tissue is at the bottom of it—as in Case 11, for example—it cannot be expected that the giving way of tissue already deprived of its vitality by disease will cause any sudden accession of pain; and in such cases there will be no scream. So again, should the cause of rupture be long continued compression of the uterus between the fetus and the pelvis, although the patient will already have suffered from a persistent aching pain, which should attract attention, yet no sudden accession of it will attend upon the actual occurrence of rupture, which takes place in a part of the organ numbed by pressure, and perhaps even already gangrenous. Further, if the patient be already occupied with severe uterine contractions, especially if rupture do not occur until the child is nearly born, or with the throe which expels it, it cannot be expected that the pain will either add greatly to the severity of that caused by the throes and distension of the *ostium vaginae*, nor, at all events, that the patient, whose respiratory muscles are already spasmodically fixed, will be able to give vent to a loud cry. Case 18 is an example of another set of conditions, under which the pain of laceration gives but a doubtful assistance to the diagnosis. The

throes throughout labour were excruciating and ineffective ; but it is impossible to say at what point of labour rupture began from any characteristic scream, for none was observed, although the patient's distress was unusual throughout. That at the last she should take a little more notice of her suffering—not by a sudden scream, but by continued manifestations of anguish—is no more than is expected at the termination of every normal labour case. In that case, the manner of rupture modified the development of this symptom. That was gradual, and the distress it gave rise to, though severe, was protracted and gradual also ; hence the patient's complaint was not sudden. Lastly, the uterine tissue itself must vary in sensibility in different persons, for in the majority of cases of rupture no accession of pain is noted at the time of rupture. I have stated their proportion to be, in this series, about  $58\frac{1}{2}$  per cent., which I believe to be even below the actual number ; but the wording of the reports make precision difficult. I conclude, then, that should a patient, in course of delivery, give vent to an abrupt, sudden expression of anguish, which she is able to refer to a particular part of the uterus, a very strong presumption in favour of the occurrence of partial or complete rupture will be raised ; but that such a symptom is rare even in cases in which, by other signs, it is known that rupture has happened. Equally, should a patient be found suffering from some of the other symptoms of rupture (or, as is far more usual, from somewhat indefinite symptoms, collapse alone being prominent), the absence of any history of the particular symptom now under discussion is not in the least damaging to a diagnosis otherwise well founded.

*(To be continued.)*

---



# ON THE TOLERANCE OF SEDATIVES IN MALIGNANT UTERINE DISEASE,

WITH A RECORD OF TWO CASES IN WHICH PAIN  
WAS THE CHIEF SYMPTOM.

By WILLIAM HENRY DAY, M.D., M.R.C.P. Lond.

Physician to the Samaritan Hospital for Women and Children.

PAIN in uterine disease is frequently the first symptom that attracts the notice of the sufferer to her ailment, and induces her to seek medical advice. She may have lost flesh and strength for some weeks previously, and her ordinary duties may have produced fatigue: she may have long suffered from leucorrhœa, or a sanguineous purulent discharge, and her face may have grown cachectic and thinner. Yet none of these symptoms, which bespeak so much to the physician, awakens the patient's alarm like that of pain. It is a symptom that asks for relief at once, for the patient can obtain neither rest nor sleep till it is mitigated, and in her agony she submits to any examination or surgical proceeding that may be proposed.

The two following cases possess some features of interest, chiefly in regard to pain, and the different methods by which it was mitigated. In the first case which I shall relate, no dose of any sedative that could be taken by the mouth afforded any relief, when the pain had reached a certain degree of intensity; in the second case, hypodermic injections failed to afford the comfort and soothe the pangs of the sufferer like large doses of morphia given in solution by the mouth. Here was a striking contrast—pain occurring in the same sex, occupying the same locality, and most likely due to very similar morbid changes. In the first case there is an interest belonging to the doses used, and the tolerance of the system under them; whilst in the latter we have invariable relief from swallowing sedatives in increasing doses, but in spite of them a rapidity of progress which is seldom witnessed.

CASE I.—I was requested to see Mrs. M., aged fifty-five, on April 1st, 1874, with Dr. Evan Roberts, who was in

attendance. She was never pregnant, and till her present illness had experienced excellent health. For about a year past she had been suffering from pain in the left inguinal region, which prevented her from sleeping at night, and there had been a variable amount of uterine hemorrhage; but this does not appear to have ever happened to any great extent. Menstruation ceased two years ago; it recurred again in June, 1873, and since then there has always been just enough loss to remind her that the catamenial functions had not ceased entirely. The uterus was freely moveable, and the os small. Immediately above the neck, which was well defined, the body dilated abruptly, and involved the fundus. No enlargement was felt above the pubes. The swelling, which was rather hard and cartilaginous, occupied the upper and left side of the vagina, and gave the impression as though it might be the left ovary; but I inclined to the opinion that it was a fibroid growth of the uterus. On introducing a speculum no ulceration could be detected, nor vaginitis worth mentioning. She thought she had lost flesh, but this was not evident to all her friends; the nervous system was much depressed; the face thin and haggard, and the pulse very weak. I advised that she should remain in bed for a few days, and a pessary containing the  $\frac{1}{18}$ th of a grain of atropine to be introduced into the vagina every night. During the daytime some glycerine on cotton wool to be applied to the os to relieve congestion, and promote a watery discharge. We gave a mixture containing bromide of potassium and tincture of quinine during the day.

On the 15th I was requested to see the patient again, with Dr. Roberts. About 4 P.M. daily she had a severe attack of pain in the left inguinal region and lower part of the back. It resembled neuralgia in its periodicity, its sharpness, and sudden departure. As she took her chief meal about 2 P.M., flatulent distension of the bowels may have partly induced it, by causing more pressure on the pelvic contents. The bowels were disposed to be loaded. We proposed leeching the uterus three times at intervals of three days, and to dilate it afterwards with a sponge tent.

29th.—The pain after my visit on the 15th was in some measure relieved, but the last four days it had come on regularly at 4 P.M. in the pelvis and lower part of the back, which seems in some way to be connected with digestion. I introduced a small tent at 10 A.M., and another at 3.30 P.M., which was followed for some hours by a steady trickling of blood; there was constant pain, sometimes bearable, but at 4 P.M. often becoming intolerable, the patient requiring the presence of her friends to console her; the bowels were always costive, and the confection of senna, which formerly answered, was now inert. A carminative and aperient mixture was ordered.

June 26th.—Saw her again, and found her not worse; on the whole rather better, but the pain still comes on daily at 4 P.M. in the left inguinal region and rectum, lasting about two hours. There was a little sanious loss, and the vagina and uterus were less tender; the urine was of a pale straw colour, acid, sp. gr. 1020; there was a trace of albumen, without any corroborative evidences under the microscope of organic kidney change. I attributed the albuminous impregnation to the admixture of uterine secretion with the urine, and as it was sometimes present, but more frequently absent, we may infer that this was its true source. We agreed to give small doses of the liquor hydrargyri bichloridi with tincture of cinchona three times a day. Ten days later (July 6th) she was in great pain, chiefly in the left hip and inguinal region of the same side; the uterus felt full, but less hard and tender; the pain now generally came on in the morning, instead of later in the day. It is excruciating whilst it continues, and defies chloral, bromides, and sedatives of all kinds. The third of a grain of acetate of morphia was injected into the left hip.

Aug. 13th.—Severe recurrence of pain the last few days, extending from the top of the sacrum to the loins, and nothing that she takes and no position that she assumes afford any comfort. Lately the pain has come on at 11 P.M., and lasted the greater part of the night: from the suddenness with which the attacks come and go, it would appear that neuralgia must be mixed up with it. The patient, how-

ever, gradually becomes paler and more exhausted, and is so strengthless that she can scarcely walk across the room without assistance ; her arms and legs are much emaciated, and her appetite so poor that some days she refuses the most delicate food. She admits that she takes too large a quantity of wine and brandy, but contends that when the paroxysms of pain come on she is obliged to resort to stimulants, and so stupefaction drowns her suffering, as drunkenness temporarily veils the mental tortures of the wretched. Some days later (Aug. 29th), as the pain continued, I put the patient under the influence of bichloride of methylene, and pushed the tumour up towards the abdomen for two inches, and heard the adhesions give way. Although the uterus seemed to be moveable independent of the growth, I was still inclined to regard it as a fibroid outgrowth from the upper portion of the uterus, wedged into the pelvis, and that any further attempt to elevate the mass would aggravate the existing evil.

*Oct. 6th.*—No relief followed the last operation. There was a new feature in the constant dysuria : the urine is of a pale straw colour, throwing down on standing a copious deposit of lithates ; acid ; sp. gr. 1020 ; slight albuminous impregnation. The last few days the hypodermic injection of morphia has been rendered necessary, which produces ease for many hours, and gives her a good night. Mr. Spencer Wells joined myself and Dr. Goddard (who had now charge of the case) in consultation. Mr. Wells thought the case a doubtful one for diagnosis, both as to the seat and character of the growth. At first he was inclined to regard it as ovarian, and to hold out the prospect of removal if the pain should continue and the strength go on failing.

*Nov. 3rd.*—A grain and a half of acetate of morphia have been injected twice daily by Dr. Goddard, and she has obtained ease from it. Mr. Spencer Wells met us again in consultation, when we found the patient in great suffering. Mr. Wells was now disposed to regard the growth as fibroid, and the pain to be due to pressure on the sacral nerves. He proposed dilatation of the cervix by means of three sponge



tents, so as thoroughly to explore the uterus, and ascertain whether it contained a polypus, or what was the nature of the enlargement.

9th.—Some relief from the sponge-tent, but nothing was found in the cavity of the uterus.

March 10th.—Since last report the patient had never been free from pain; she gradually became more exhausted, and died this morning.

Note.—From Oct. 25th to Dec. 30th, 1874, fourteen hypodermic injections, each averaging three grains of acetate of morphia, and the third of a grain of atropine; from Jan. 1st to Jan. 22nd, 1875, two and sometimes three injections were used daily of the same strength; from Jan. 22nd to March 10th, two and sometimes three injections were also daily used, each averaging five grains of morphia and half a grain of atropine.

Remarks.—This case presents many points of interest. In the first place, these large injections never caused vomiting, nor active cerebral disturbance. When pushed in such doses as were sometimes found necessary, the patient presented the ordinary condition of labouring under narcotism, but her life was never placed in danger from it. It was also noteworthy, that not the slightest local irritation around the punctures in the arm ever followed the introduction of the syringe. No one has ever explained why these large hypodermic injections should be tolerated by some persons and not by others. It cannot be from the mode of operation, because it may be done by different operators, and the physiological effects are the same in all instances. Independent of the liability to puncture a vein, or injure a nerve, there are some cases which seem never to tolerate hypodermic injection in the smallest quantities. A married lady, forty years of age, whose life was trying and anxious, consulted me in December, 1874, for agonizing pain in the lower part of the back, over the centre of the sacrum, which was well covered with adipose tissue. The uterus and kidneys were healthy. As the pain had resisted all kinds of liniments and tonic remedies, I injected the  $\frac{1}{6}$ th of a grain of acetate of morphia over the seat of suffering, and

in about ten minutes after my leaving the house I heard on the following day that the patient became sick and faint, her vision confused, and her limbs so tremulous that she could not stand for some minutes. Cases are recorded by Dr. Handfield Jones,\* where the subcutaneous injection of morphia produced faintness, perspiration, and rapid falling in the frequency of the pulse: in one case from 135 to 80, and in another from 120 to 60. The Report of the Royal Med. Chir. Soc.† also contains some instructive information on this subject. I believe it to be of great importance to combine atropia with the morphia, and not to commence with more than the sixth part of a grain. Unless the pain is severe, it is better to begin with the eighth, or even the tenth part of a grain. I am satisfied that mixing the alkaloids lessens the tendency to syncope, and has not that depressing effect on the cardiac nerves which morphia seems to possess in many cases. Then, too, the injection should be slowly performed, and as much as possible out of the way of all veins, and the instrument held horizontally under the skin, so as not to penetrate too deeply into the tissues.

CASE II.—Mrs. B., aged forty, a fair woman, with light hair and grey eyes, called at my house on March 5th, 1875, complaining of uterine discharge. Has two children living, and lost two when fifteen months old from dentition, though she stated that neither child had any signs of teeth. The last child was born six years ago, and all her confinements had been good; never pregnant since. The catamenia had been regular till a month ago, when a sanious and watery discharge replaced it for a week: the period came on last time after a regular interval, and was natural in colour and quantity. Since then there had been a watery discharge, and pain in the bottom of the back, but none in defecation; the bowels were regular and the urine clear, and free from albumen. The patient stated that she was habitually pale, and never experienced any illness till last summer, when she had a slight attack of gravel, and the urine now frequently throws down a pink sediment. The patient is

---

\* *Clinical Transactions*, vol. iv., 1871, p. 72.    † *Med. Chir. Trans.*, vol. l., 1867.

rather stout, and does not appear to have lost flesh ; tongue pale, clean, and flabby : pulse weak and slow. On examination per vaginam I detected an irregular excavated ulcer, the size of a sixpence, on the posterior lip, with a reddish, angry margin. The centre contained some ashy, purulent matter, and appeared likely to extend rapidly in depth and circumference : the os was free, and had the appearance of belonging to a woman who had borne children, but the cervix was indurated and enlarged. I cleaned the ulcer with a piece of cotton wool, and applied the solid nitrate of silver. I also ordered a chloride of zinc lotion, and told the patient not to leave her house to visit me again. I considered the appearance of the ulcer extremely suspicious, though with the exception of a little pain in the back, she had not suffered discomfort, nor would she have now taken an opinion except for the discharge.

On the 10th of March, at the patient's own house, I found that the ulceration was extending, and that its appearance was rather cancerous than phagedenic ; the posterior lip was much excavated, and the ulceration of irregular outline. A solution of chromic acid was thoroughly applied to the affected parts, and afterwards a piece of cotton wool soaked in oil. The patient was ordered to keep in bed for a week, and to take a grain of opium if she should be in pain.

26th.—No pain followed the last application. Large films of decayed-looking membrane or slough had come away from time to time, and pain was still complained of in the lower part of the back. The period came on two or three days earlier than was expected, and though it did not last longer, it was rather more profuse. The application had not arrested the progress of the disease ; the upper part of the posterior lip was ash-coloured and excavated, the lower part was cleaner-looking, as if granulating, but a light touch with a piece of lint set it bleeding at once ; the discharge was very watery and free. I applied the solid nitrate of silver, and ordered a continuance of the lotion as it corrected the fetor. On the 31st, as there was no improvement, the acid nitrate of mercury was freely applied to every part of the disease.

*April 8th.*—There has been considerable pain to-day. On examination all the slough had disappeared, and the wound looked clean, but angry, irregular, and excavated. It wears the appearance of a rodent ulcer of the uterus. Fearing hemorrhage, which is so common in these cases, I applied the tincture of perchloride of iron to the ulcer.

*24th.*—Mr. Spencer Wells, who saw the patient in consultation, confirmed the diagnosis, and recommended that no stronger application should be used than Dr. Richardson's styptic colloid twice a week.

*May 14th.*—For some time past the patient has felt great forcing pain in the vagina and lower part of the rectum ; she is seldom free from suffering, and can obtain no rest at night. Sedatives lock-up the bowels and so cause discomfort, whilst hydrate of chloral, which formerly gave relief, has no effect now. The styptic colloid has failed to control the disease, and the ulceration has extended into the body of the uterus, which is now hollowed out into a basin-like cavity, and when the patient is placed on her back, the thin anterior lip falls over the wound, and obscures it like a valve or curtain : the whole of the posterior lip and sides of the neck are gone ; the discharge is thin and purulent, but with the use of carbolic acid lotion, there is an absence of fœtor. Notwithstanding so much pain and local mischief, the appetite is good and the face bright and cheerful.

*June 17th.*—Severe hemorrhage came on, which was checked by the local application of perchloride of iron ; the ulceration is extending, and the face assuming a waxy pallor.

*27th.*—More suffering and smart hemorrhage. The uterus was getting fixed, and causing much pain in passing water, a few drops only coming at a time. A catheter was introduced, and about an ounce of turbid urine drawn off, acid and non-albuminous. Five minims of Battley's sedative solution of opium, and thirty minims of the *liquor potassæ* in an ounce of camphor mixture were given every four hours. At 5 P.M. the pain continuing, gr.  $\frac{3}{4}$  of morphia, and gr.  $\frac{1}{60}$  of atropine were injected into the right arm, which gave partial relief for seven or eight hours, but an erysipelatous blush, the size of a crownpiece, appeared



around the puncture, followed by general cutaneous irritation, nausea, and restlessness.

28th.—Pain returned last night, and the hypodermic injection was repeated with the same unpleasant symptoms.

July 15th.—The great suffering had continued. Last evening at 4.30 gr.  $\frac{3}{4}$  of morphia and gr.  $\frac{1}{50}$  of atropia were injected into the arm, and she slept till 9 P.M., when she took forty minims of the morphia solution, as the pain and irritation were returning. At 11.30 she repeated the morphia and slept till 1.30 A.M., when she took another dose, and rested till 7.30. At 8.30 she was comfortable, and her mind clear. Menstruation began on the 10th, and is now going on.

August 23rd.—On returning from my autumnal tour, I found that she had woke up from sleep at 12.30 and died at 1.15. During the last week of her life one drachm of the solution of the bimeconate of morphia for a dose failed to give relief, but half the dose, with thirty grains of hydrate of chloral, never failed to procure sleep and relieve suffering. The effect was not always immediate, as if the pain was great it took longer time, but it generally gave four or six hours' rest and sleep. Codeia, belladonna, cannabis indica, sedative pessaries of all kinds, and strong solutions of opium applied to the ulcer failed to afford comfort. The patient took a fair amount of nourishment, and alternately brandy, sherry, port, in moderate quantities. Her appetite never failed, and she relished her food to the last. During the last week when exhaustion had so much increased, and there was a good deal of wandering, she came downstairs one night in her sleep: in the early morning she was confused in conversation, and in dates and days of the week. As the day wore on her intelligence became clearer, but the pain returned.

*Remarks.*—These two cases suggest several points for consideration.

1. *Pain in its mildest form may be defined as a disagreeable sensation, not admitting of a precise definition.* It is symptomatic of a disturbance in some organ or tissue of the body, and each variety has its special characters and pecu-

liarities. Thus numerous names are given to describe the kind of pain—it is sharp or lancinating, it is heavy, tensive, or throbbing, burning, pungent, forcing, gnawing, and so forth. Patients will give different definitions to pain occurring in the same disease, and in the same organ.

2. *Pain is endured by women of a certain age without complaint or apprehension of danger.* Women come before us who refer their suffering to one groin, or down the front or back of the thigh, which we can trace to uterine irritation: others who consult us say that they have endured pain for some months over the sacrum and loins, and if the patient has approached the climacteric period, she is reconciled to her suffering as inevitable to her age. Her age, in fact, even throws her friends off their guard, and though she looks sallow, thin and worn, and the malar and temporal bones are prominent, it is considered a natural process, which will rectify itself as time passes on.

3. *Local symptoms and changes in their relation to pain.* The patient perhaps tells us that menstruation ceased three or four years ago, but of late she has noticed a little discharge, sometimes sanious, sometimes semi-purulent or like dirty water: the vulva is red, and here and there the mucous membrane is excoriated and painful. When the finger is introduced into the vagina a little irritating discharge escapes; it is thicker in some cases than in others, and has a sickly, unpleasant odour. Occasionally it is creamy and irritating, with scalding in micturition; the uterus may feel nearly as moveable as it ought, but the soft elasticity of health has departed, and the os and cervix uteri have an indurated feel, and yet we cannot say positively that anything is wrong, though we are suspicious that fibroid or cancerous change will be developed later on. If we introduce a speculum we may observe some general vaginitis with points of vascularity here and there: the whole vagina may be relaxed, or there is some constriction and discomfort experienced on passing a moderate-sized instrument, even if the patient has borne children in early life. The cervix has a similar unequal redness, and a thick irritating discharge may escape from the uterine orifice. If you introduce a sound

it readily enters, and a little blood follows it : there is pain at the fundus, and the organ may be somewhat retroverted and depressed in the pelvis. This is compatible with a low degree of chronic inflammation, especially of the fundus (endometritis), and we cannot say that such is not the condition which masks the greater evil. By-and-by, if the case is of malignant origin, a breach of surface commences near the os on the anterior or posterior lip, the discharge increases, and continues in spite of injections and applications till the general health shows signs of failure. Such is the history and early progress of many cases of malignant uterine disease.

To refer especially to the cases under consideration, pain may be an *early* or a *late* symptom. In both these cases it was a *late* symptom, and the constitutional powers were considerably reduced before any attention was directed to the local trouble. In the *first* case remedy after remedy failed, before any examination was instituted, and not until the patient was obliged to relinquish her household duties did she realize her critical condition. The *second* case is characterized by a rapidity of progress rarely seen in the worst forms of malignancy, and an absence of severe pain till the greater portion of the uterus was destroyed by ulceration. The amount of pain which is tolerated by different individuals is very remarkable. It is a question that enters into our daily calculation, and our practice is influenced by the weight we assign to the pain described by our patients. Undisciplined people of sanguine and excitable temperament, who have experienced an unbroken career of prosperity and sound health, bear pain badly. They have no power of self-government, fortitude deserts them when it is most needed, and they exaggerate their suffering, or bear it so badly, that they prefer death to its continuance. In examples of this kind the imagination fancies the corporeal pain is insupportable. It is a suffering doubly reflecting itself through the mind, which it prostrates at once, deprives reason of her sway, and implants despair—the condition of the mind allows of no escape, but it crouches under the conflict and feeds on every twinge. But those persons who

have experienced temporary drawbacks from health, whose life has been chequered by sickness, will bear serenely the infirmities of age or suffering, and scarcely any torture prompts them to wish for the deliverance which death brings. "So much," says Montaigne, "are men enslaved to their miserable being, that there is no condition so wretched that they will not accept for preserving it."

---

## A CASE OF DIFFICULT LABOUR FROM ABNORMAL RIGIDITY OF THE OS.

By GEORGE CALDERWOOD, M.D.

ON the 13th of October last I was called in to attend Mrs. G. in her third confinement. I arrived at the house about five o'clock in the morning, and proceeded at once to make a vaginal examination. The os, wholly undilated, could be felt high up, very much indurated and very rigid, but no presenting part could be reached by the finger. Labour had commenced about three hours before my arrival, and the pains came quickly, were moderately strong, and somewhat short. I remained by her all the morning and the greater portion of the day, during which time there was no alteration in labour, and little or no alteration in the rigid os. At two o'clock in the afternoon I was compelled to leave for a time to attend to other work, and as labour was keeping well up, I was anxious to allow it to go on as long as was safe, in the hope that the uterine pains might overcome the unnatural rigidity.

Two hours after leaving I got back to my patient again, but a digital examination revealed no improvement so far as the rigid os was concerned. The os uteri did not feel so high up, but it was quite as rigid and indurated, and the finger passed through it was firmly constricted. After three hours more of hard labour, I could feel that the presenting part was a head, but still there was no improvement in the rigidity. The uterus, as a whole, seemed nearer the vaginal outlet, and this I presume enabled me to feel the presenting



part. I waited on till ten o'clock at night of the same day, when finding that uterine action, though strong, made not the slightest impression on the os, fearing rupture of the womb, and the woman beginning to get into a semi-delirious condition, I plainly saw that operative interference would have to be had recourse to.

The question was, What was to be the nature of that interference? Forceps were inadmissible, as the os was undilated. The one great object to be attained was to effect its dilatation. The question was how? I have the greatest confidence in Barnes's dilating bags where there is no unnatural rigidity, but in this case I felt they would have been powerless to effect the purpose intended. Another method recommended in these cases is to make small incisions, cutting the os in several directions; but had I done this, I was afraid such incisions would act as centres for subsequent tearing, in any further efforts that might have to be made to effect delivery, and even in the indurated state of the parts by the natural contractions of the womb itself.

Considering all these points, and the case being now urgent, I resolved to dilate gradually with the hand, and then turn and deliver. I accordingly proceeded to do so. One finger was first passed through the os, then after a time a second, and so on till all the fingers were introduced. The constriction at this time was most powerful, and I therefore rested for a considerable time without making any further efforts at progress. At last with some difficulty I succeeded in introducing the whole hand within the uterus, without any perceptible laceration. I then, after waiting a short time, turned and delivered, in the performance of which no unusual difficulty presented itself. The same difficulty that existed to the passage of the child existed to the passage of the after-birth. The womb contracted well, but neither by external manipulation, nor by gentle traction at the cord, could the placenta be removed.

After waiting a proper length of time, I was compelled a second time to introduce my hand through the os, having much the same rigid feel. I went through the same slow process—one finger after another—and found the placenta

completely detached from the uterine wall, and readily brought it away. The only object preventing its passage was the rigid os. The patient made a good recovery.

My object in relating this case—and similar cases must be frequently occurring to daily practitioners—is to show what may be done by the hand, provided due caution be used, and above all, provided the operation be not hurried. Milder remedies should first be tried, but should they fail, the gradual dilatation of the os by the hand may with proper care be attempted. In ordinary cases of dilatation of the os, or where it is desirable to produce it with a view to bring on labour, the use of Dr. Barnes's bags is, I believe, the proper course to follow. In such a case as this the choice was betwixt the operation performed and incisions. The latter is, I think, much more likely to be followed by disastrous results than the former when judiciously employed. The laceration produced in this case must have been very slight, if any at all, as was shown by the same condition presenting itself to the passage of the placenta after the passage of the child. Incisions must have produced a breach of surface, and each incision must have been the seat of laceration to a greater or less extent. In that case absorption of septic matter would have been rendered an easy matter and the danger greatly increased. I ought to mention that her two previous confinements were difficult—instrumental, as I was informed—and it is possible and highly probable that in one or other of these, such changes may have been produced in the neck of the womb as have resulted in its present rigid condition.

To sum up. Use Dr. Barnes's dilating bags if they are suitable for the case—that is, if you have time and if they are sufficient to effect dilatation ; if not, use the hand as I have described, in preference to incisions.

---

## Reports of Hospital Practice.

---

### ST. MARY'S HOSPITAL, MANCHESTER.

---

#### CASE OF EXTRA-UTERINE FETATION.

*Under the care of MR. CULLINGWORTH.*

MARGARET B., aged twenty-six, residing at Rochdale, was sent by Mr. W. E. Richardson, of that town, to St. Mary's Hospital, Manchester, on the 2nd October, 1875, and was subsequently (November 3rd) admitted as an in-patient.

She was married two years ago, and had hitherto borne no children. Menstruation occurred a week after marriage. The three subsequent catamenial periods passed without any appearance of the menses; the general health meanwhile remained undisturbed. After this suspension, the menstrual flow reappeared, and continued normal in quality, quantity, and duration, until November, 1874, when, for the first time, the discharge was less profuse than usual. She was suddenly seized soon afterwards with severe paroxysmal pain in the lower part of the abdomen while passing urine: this pain returned from time to time for six or seven weeks, occurring most frequently during micturition. For at least six weeks she was very ill indeed. The urine was passed with pain, difficulty, and increased frequency; it was scanty in quantity and accompanied with a pink deposit. Swelling of the abdomen was first perceived a month or two afterwards; then also indistinct movements were felt in the abdomen, and the breasts enlarged. At one time a little milk exuded. After the scanty catamenial flow in November already alluded to, no further discharge took place for eight months, at the expiration of which time she experienced at irregular intervals for several days a series of severe pains in the lower part of the abdomen, simulating labour pains; no vaginal discharge of any kind attended these phenomena. Soon after the occurrence of these pains, a discharge similar to the catamenial flow was observed, which, however, lasted

for the unusual period of a week. During this flow the patient consulted Dr. James Whitehead, of Manchester, who, after a stethoscopic examination, pronounced her pregnant; he said he thought the pregnancy had reached to about the seventh month, and that the child was feeble. During the last two months normal menstruation has occurred twice at the usual interval.

*Oct. 2nd, 1875.*—The patient is a short, well-formed woman, of unimpaired general health. The breasts present the appearance usual in women who have been pregnant; they are now no longer enlarged or full. The abdomen is occupied by a tumour, dull on percussion and giving a distinct sense of fluctuation throughout; no part of it can be distinguished from the rest as being of firmer consistence. No souffle or fetal pulsation can be heard. The following are the measurements:—Girth at umbilicus, 33 in., that of the two sides being equal; distance from umbilicus to pubes,  $7\frac{1}{4}$  in.; from umbilicus to ensiform cartilage,  $6\frac{3}{4}$  in.; from umbilicus to anterior superior spine of left ilium,  $7\frac{1}{2}$  in., and to anterior superior spine of right ilium the same; upper limit of dulness on the left side,  $3\frac{1}{4}$  in. below the ribs in the nipple line; on the right side,  $\frac{3}{4}$  in. below the ribs in the nipple line; in middle line, 4 in. above the umbilicus; limit of dulness to right of umbilicus, 8 in., and to left none.

On vaginal examination, the cervix uteri is soft and flexible, projecting about  $\frac{1}{2}$  in. into the vaginal canal; the os uteri is sufficiently open to admit a large quill; the uterine sound cannot be made to pass beyond the cervix. Pressing down the vaginal wall in front of the cervix is a smooth elastic immovable tumour; the encroachment on the vaginal canal is not very considerable. The posterior part of the vagina is normal.

The patient unfortunately refused at this time to remain in the hospital, and it was not until the 3rd November (a month afterwards), when symptoms of severe illness had set in, that she yielded to advice and presented herself for admission. She then gave the following account. She had remained in good health up to Saturday, October 16th, on the evening of which day she attended a theatrical perfor-



mance and got very wet. The next day she shivered and thought she had taken cold, and on the 18th she began to vomit and to suffer from diarrhea. From this time she felt very ill, lost her appetite, and became thinner.

*Nov. 4th.*—The patient no longer wears the healthy look she did a month ago. Her face is flushed; her tongue dry, and red in the centre; temp. in axilla,  $100^{\circ}8$ ; pulse 124; resp. 28. A soft hemic murmur is heard at the base of the heart. There is nothing abnormal in the condition of the lungs, beyond a few large râles and rhonchi at both bases posteriorly. The abdominal measurements are exactly the same.

*9th.*—On examination to-day the physical signs in the abdomen are found to be completely altered. There is no longer any fluctuation to be detected, and the abdominal tumour is tympanitic throughout. Immediately beneath the umbilicus is a hard round mass, about equal in size to a fetal head at the seventh month; on pressure with the finger at certain points the surface yields and rebounds as do the parietes of the fetal skull; separated from this mass by a narrow groove is another solid substance, which gives to the touch the impression of an upper extremity flexed at the elbow. Other, though less easily distinguishable, solid portions can be felt laterally. There is universal tenderness on pressure over the tumour, and the abdominal parietes crackle beneath the fingers. The patient does not complain of pains. The function of micturition is performed easily; the action of the bowels is frequent and the stools watery; a large mass of hemorrhoids lie outside the anus without causing the patient any suffering from their presence. The tongue is dry and red; no appetite; considerable thirst; temp. *in recto*,  $102^{\circ}8$ ; pulse 132; resp. 24; breath has a sweet mawkish odour.

*10th.*—An enema having been administered, the patient was anesthetized by means of the bichloride of methylene, and in the presence of Dr. Radford, Dr. Lloyd Roberts, Dr. Nesfield, Mr. Heslop, Mr. W. E. Richardson, Mr. Ewart, and the resident staff, an exploratory incision was made in the median line, three inches in length, extending from the

umbilicus downwards. This incision needed enlarging at a later stage of the operation to the extent of half an inch upwards and to the left, and half an inch downwards. The sac of the tumour was closely and universally attached in front to the abdominal parietes; the sac was therefore divided by means of an incision corresponding to that in the abdominal wall. A small quantity of yellowish pultaceous matter, of a fecal odour, immediately escaped from the sac. Then the arm of a fetus, of about the seventh or eighth month, came into view; on grasping this, the head was easily reached and a female fetus extracted. The umbilical cord was unbroken, thin, and shrivelled; the placenta was only represented by a few strongly adherent shreds of tissue attached to the anterior wall of the sac. The fetus measured 18 inches in length; considerable shrinking of its tissues had taken place; the skin, however, retained its integrity without even an abrasion of the cuticle; the odour was fetid without being unbearable. The sac measured internally  $7\frac{1}{2}$  inches in length by 7 inches in breadth; its inner surface was rough and uneven; there was, in its very thin posterior wall, a longitudinal rent half an inch long, the edges of which were sutured without any disturbance of adjoining parts, even for purposes of examination. The lining of the sac was sponged over with a weak solution of tincture of iodine, and the edges of the external wound having been brought into apposition by the aid of six sutures, a metallic gilt spiral wire drainage-tube was inserted below the lowest stitch, and the wound covered with a dressing of carbolized oil and gutta-percha. 2.45 P.M. Pulse 140. A little warm beef-tea was administered as soon as the patient had recovered from the anesthesia, and at five o'clock a pint had been given. She then asked for a little toast and water, which was allowed. She had not vomited, nor complained of any pain; pulse 126; temp.  $101^{\circ}$ . 11 P.M. Cheerful and talkative; wishes to sleep but is unable; pulse 112. A quarter of a grain of morphia given hypodermically.

11th.—Slept for two hours after injection. At 2 A.M. took a few spoonfuls of beef-tea, and then fell asleep until nearly six. 9.30 A.M. Face pinched; pulse 20; temp.  $101^{\circ}9$ ;

no pain ; no sickness. 2.30 P.M. Paroxysms of severe abdominal pain ; tympanitic distension of abdomen ; pulse, 29. Some animal charcoal and an aniseed draught were ordered by the house-surgeon. 5 P.M. Sudden and copious discharge through the drainage-tube of flatus and fluid blackened with charcoal ; pain relieved thereby temporarily. 6 P.M. Face from time to time expressive of sharp passing pains ; pulse 140. 9 P.M. Pulse 160. 11 P.M. Repeated subcutaneous injection.

12th.—Slept a little at intervals during the night ; very restless when awake. From this time the patient's pulse became uncountable ; liquids were swallowed greedily without causing any nausea or vomiting ; the features became pinched and deeply flushed ; busy, excited delirium supervened, and after a very restless night, she died at 4.25 A.M. on the 13th, sixty-six hours after the operation.

*Autopsy.*—A hurried examination was made twelve hours after death by one of the resident officers. The body was much emaciated ; there was intense rigor mortis ; over the crest of the right ilium the skin presented a livid shiny patch, as of an impending slough.

The sac was intimately adherent to the surrounding parts, and contained a large quantity of perfectly black thin fluid, consisting apparently of fluid food, intimately mixed with charcoal. The charcoal greatly obscured the condition of the parts, and the situation of the communication with the intestinal canal was not discovered. The bladder was adherent to the lower portion of the sac. The uterus lay behind and was also closely adherent. The uterus and appendages, with a portion of the sac, were removed for further examination, which, owing to the right Fallopian tube having been divided across at a point  $1\frac{1}{2}$  inch from the uterus, only elicited the following facts. The uterus was empty ; its length  $2\frac{1}{2}$  inches ; length of its cavity 2 inches ; the left Fallopian tube and left ovary were normal ; the right Fallopian tube was normal from the uterine end to the point where it had been unfortunately divided—*i.e.*, for the distance of an inch and a half.

---

# THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

JANUARY, 1876.

---

## THE BRUSSELS CONGRESS ON MATERNITY HOSPITALS.

AMONG the proceedings of the section of Gynecology and Obstetrics at the Brussels Congress, we find valuable information as to the present opinions of Continental medical men concerning Maternity Hospitals. The subject was opened by a report presented by M. Hubert, Jun., Professor of Obstetrics at the University of Louvain. His conclusions were that small Maternities are better than large, and that every Maternity should have a spare house adjacent, to which infectious cases might be removed. He also advised the careful disinfection of attendants, instruments, clothes, &c., and assumed it as beyond dispute that the delivery of women assisted at their own homes was favourable, and the mortality in Lying-in Hospitals frightful. M. Lefort, whose name is so well known in connexion with midwifery institutions and statistics, went still further in his condemnation of Maternity Hospitals, and would have them abolished altogether. From statistics based on 1,843,093 labours, he showed that, in Paris, outside the hospitals, the mortality had been 1 in 212, whilst in the Maternity Hospital it had been 1 in 29. He contended that homeless women should not have to pay the price of their lives for the hospitality afforded them, nor should they be sacrificed to the necessities of instruction. The alternative he suggests is, that women requiring homes and aid during their confinements should be received into the houses of the midwives under the control and at the cost of the Authorities. This plan was tried provisionally in 1865, and adopted definitely and officially in 1867; and M. Lefort quotes the



following statistics in support of his scheme. Last year the mortality among women delivered at the midwives' houses was 1 in 312; in the General Hospitals, 1 in 24; in the Lying-in Hospitals, 1 in 42; and at the Clinique d'Accouchement, 1 in 12. These figures, which are certainly very startling, produced a profound impression, and the following conclusions were adopted by the section:—"1st. A medical reform of the means for the delivery of women is urgent. 2nd. The total giving up of large Maternity Hospitals. 3rd. Replacing the large Hospitals and Midwifery Schools by lying-in houses, with isolated rooms. 4th. The establishment of a spare house, placed close to the lying-in house, but with separate administration. 5th. Extending as far as possible the home assistance by aid and succour of every description." The plan advised by M. Lefort is not, as he admits, without drawbacks, even in Paris, where midwives are instructed and supervised by the State. Legal records show that abortion as well as midwifery is occasionally practised at the houses of midwives. The expense of this mode of assistance is also greater, but this difficulty he overcomes by an ingenious calculation, in which he demonstrates that the life of a mother can be saved for 635fr., and asks whether such a purchase does not become a duty to be performed by the Administration. We may congratulate ourselves in being unable to furnish from our Lying-in Hospitals statistics exhibiting anything like the amount of mortality which M. Lefort has found in Paris. With us the question of abolishing our Maternity Charities is not one demanding attention. Our most needed reform towards the prevention of puerperal mortality is in the direction of ameliorating the present condition of midwives. This is indeed urgently required, and if anything is to be done during the next session of Parliament, it is time the Obstetrical Society and the British Medical Association began to mature their plans and endeavour to obtain from the Government a promise to deal with a Bill based upon their suggestions. For the instruction of midwives, schools will be required which ought when possible to be connected with maternity institutions. These, if con-

structed upon the excellent plans furnished by Miss Nightingale, might be used with a minimum rate of mortality for the reception of homeless parturient women, and for the instruction of women who desired to fill the responsible office of midwives. The experience of our Continental brethren will be of great service in assisting us to carry out correctly the details of our necessary midwifery reforms, as also will be the general considerations arrived at in the Obstetric section of the Brussels Congress.

---

### Abstracts of Societies' Proceedings.

---

#### OBSTETRICAL SOCIETY OF LONDON.

*Wednesday, December 1st, 1875.*

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., *President,*  
*in the Chair.*

#### *Anencephalic Fetus.*

Mr. OLIVER BARBER (of Sheffield) exhibited a specimen where the calvarium was altogether absent, and the base of the skull formed simply by the basilar portion of the occipital bone. The *laminae* and spines of the upper cervical *vertebrae* were undeveloped, leaving the spinal canal open. There was no appearance of spinal cord on dissection, the canal itself being lined by a thin fibrous membrane. There was an enormous quantity of liquor amnii present; the open spinal canal closely simulated the anal aperture.

Dr. WILTSHIRE thought the specimen one of considerable rarity, as well as of great practical importance. It answered mainly to the description given by Geoffrey St. Hilaire of the variety of anencephalic fetuses called der-encephalic, and as specimens of this were extremely rare, he hoped Mr. Barber would present this one to the Society. The aperture in the upper part of the spinal canal had very naturally caused embarrassment in the diagnosis of the presentation. He praised the practical manner in which Mr. Barber had dealt with the case.

In reply to a question from Dr. CLEVELAND, Mr. BARBER stated that no fetal movements were noticed during pregnancy.

The PRESIDENT inquired if any dissection had been made of the cranial and spinal nerves, with the view of ascertaining how they terminated in the skull and vertebral canal respectively. Some years ago he had seen an anencephalic child, born alive, which had breathed

and sucked for some days. The malformation was less extreme than in the specimen before the Society, for the spinal cord was present; but there was no roof to the skull, and the brain substance was represented by a flattened mass of cystic structure, into which the cerebral substance had degenerated, or which had been developed there, instead of the brain. He had made a careful dissection of the cranial nerves in that case, and found that outside the skull, instead of terminating in loops, the nerve filaments gradually merged and were lost in the cellular tissue which lay between the small cysts composing the mass which represented the brain in the base of the cranium. In the preparation shown there was no opportunity of ascertaining the termination of the spinal nerves in the spinal canal, when the cord itself was absent.

---

#### *Irregular Pigmentation.*

Dr. GODSON exhibited a drawing of the breasts of a girl, aged eighteen, pregnant seven months, affected with chorea. The inner half of the *areolæ* of each nipple was entirely free from pigmentation; that of the outer portions being uncommonly dark and crescentic in form. The girl was present for the Fellows to inspect.

Dr. BARNES said the case was extremely interesting. The symmetrical defect of pigment might be explained by like areas of nerve distribution. In all probability the altered nervous condition of pregnancy had much to do with pigmentation. Respecting the chorea, he (Dr. Barnes) had long since called attention to the fact that, under the exalted nervous tension wrought by pregnancy, aided probably by some change in the constitution of the blood, any pre-existing disposition to chorea, or other nervous disorder, was apt to be evoked. Thus pregnancy was an exciting cause of chorea.

The PRESIDENT inquired whether there was any history of injury from a scald or burn. The case presented the usual characters of some skin eruptions, in that it was symmetrical.

Dr. WILTSHIRE thought the case peculiarly interesting, and worth preserving in the *Transactions* of the Society, if Dr. Godson would give the coloured drawing.

Dr. GODSON stated that there was no hereditary tendency to such a condition. The mother had two nipples on one breast.

Dr. HAYES thought the fact of chorea and pregnancy existing together was one of much interest. He had seen 100 cases of chorea, and had noted the very marked relation between chorea and rheumatism.

---

#### *Epithelioma of the Cervix Uteri and Pregnancy.*

Dr. EDIS exhibited the specimen removed post-mortem from a patient. She was thirty-three years old; married six years; two

children, youngest three years, which she nursed for one year and nine months. Dr. Marsh, of New Wandsworth, had sent the case up to Dr. Edis, at the Middlesex Hospital, in the latter end of June. She was then found to be the subject of epithelioma of the cervix uteri, and was as near as could be judged between seven and eight months pregnant. She had had frequent floodings, and a more or less continuous offensive watery discharge. As the patient could not be admitted at the time, the nature of the case, and the difficulty likely to occur when labour set in, were explained to her, and she was desired to report herself later on. On August 7th she presented herself, stating she was in labour. The disease extended in a crescentic manner round two-thirds of the os uteri; the remaining healthy portion seemed disposed to dilate, and as the pelvis was roomy, it was thought the risk to the mother and child would not be greater than if Caesarian section, which had previously been contemplated, should be resorted to. Dilatation of the cervix was facilitated by means of Barnes's bags, and at 10 A.M. on August 8th, the cervix being dilated sufficiently, chloroform was administered, and the long forceps applied, a female living child, weighing  $6\frac{1}{2}$  lbs. being extracted. With the exception of some diarrhea, the patient progressed fairly well for the first week, when the temperature rose to  $105^{\circ}$  F., and the patient died on the 21st, a fortnight from the date of delivery.

Dr. ROUTH thought that Caesarian section ought to be more often performed in these cases: it gave the best chance of life to the child. Where labour was allowed to take place naturally, the death of the mother usually resulted from puerperal fever and sloughing.

Dr. BARNES could not assent to the conclusion that the Caesarian section should be resorted to in every case of cancer of the uterus. The cases cited by Dr. Routh occurred in the Vienna Hospital, where many other causes of puerperal fever were rife. He himself had seen women survive labour complicated with cancer: one woman had been delivered twice. It was not advisable to lay down a general rule.

Dr. MEADOWS stated that his experience entirely corroborated that of Dr. Routh, that the risk to the mother in cases of extensive cancerous disease of the cervix was so great that the Caesarian section was preferable to any other mode of delivery. He had seen four cases of this kind, and all ended fatally to the mother, while only one child was saved. In one, craniotomy had been performed; in two, premature labour was induced. He believed that the Caesarian section gave a better chance to both mother and child, in cases where the disease was so far advanced that an operation of some kind was necessary for delivery.

The PRESIDENT thought this statement should be qualified by the extent of the disease. He inquired if Dr. Meadows really meant that he would perform Caesarian section in *all* cases of labour complicated with malignant disease of the cervix uteri, for if so, he believed this



was an opinion which most practitioners would think required some qualification. While not doubting the propriety of this operation in cases where the passages were much obstructed by morbid growth, or where serious laceration of the parts must ensue if delivery was effected through the natural passages, he could not concur in regarding it as necessary in all cases where disease was less advanced, and he was sure the Society would be glad to hear the opinions of men with such practical experience as Dr. Barnes and Dr. Routh.

Dr. AVELING thought that the operation of Cæsarian section would not be justifiable if only one lip of the uterus were involved.

Dr. ROGERS stated that in thirty-five years only two cases of this nature had occurred. The question of Cæsarian section was one of much importance.

Dr. BARNES, in reference to Dr. Meadows's assumption, that if all the cases were analysed it would be found that the results would be as in those observed by himself, replied that this analysis had not been made; and it was not safe to frame a general law upon an assumption. The best plan at present was to consider each case as it came before us on its individual merits. In cases where the disease was of small extent, it might be safer to let delivery take place in the ordinary way. Speaking from memory, he thought Dr. Oldham had published in the Guy's Reports cases which showed that it was better to take this course—*i.e.*, not to induce labour, but to let pregnancy go on to term, and then to act according to indications.

Dr. ROUTH stated that in the cases he had seen it was impossible to deliver except by craniotomy.

Dr. EDIS explained that he brought forward the present case not with the view of advocating similar treatment in all cases. Each case required to be treated on its own merits; but as an instance of delivery *per vias naturalis*, where the disease was very extensive. Nature herself took the initiative, and seemed to indicate that delivery was possible, and for this reason Cæsarian section was not resorted to.

---

*Note on the Post-mortem Diagnosis of a Nulliparous Uterus.*

Dr. ALFRED MEADOWS referring to the now celebrated trial of the brothers Wainwright for murder, remarked on the extreme importance of being able to diagnose post-mortem whether or not a uterus had ever borne a child, and on this point he invited the opinion and experience of the Society. In the case referred to he thought it was probably not too much to say that if any absolute test existed whereby a positive opinion in the negative could have been given, the whole case for the prosecution must have failed on the first indictment, for it was well known that the person alleged to have been murdered had borne children. He, however, knew of no such test, nor did he think it could be affirmed on the contrary that this uterus had ever been gravid so far as appearances went. The thickness of the uterine

walls, which in this case barely exceeded a quarter of an inch, and the distinctly convex shape of the internal aspect of the uterine walls, induced him to the opinion that this was a nulliparous uterus. Accordingly the evidence which he gave at the recent trial was to the following effect :—First, that as a general rule no absolutely certain opinion can be given by post-mortem examination only, even under the most favourable circumstances, on the question whether or not a woman has borne a child. Secondly, that on the question of probability some reliance may be placed on the internal appearance of the uterine walls, especially with reference to their convexity. Thirdly, that so far as this particular uterus was concerned there was certainly no proof that it had borne a child, but that on the contrary the evidence, doubtful and unreliable as it was, tended rather to the opposite direction, and justified an expression of opinion to that effect. The author then referred to a very delicate question of a medico-ethical character—viz., as to whether a scientific witness is at liberty to allow his private judgment on the general merits of a case to influence his opinion on a purely scientific question. Upon this point also he invited the opinion of the Society.

Dr. EDIS inquired whether any notice had been taken of the condition of the ovaries. He thought some inference might have been drawn from their appearance.

Dr. PALFREY wished the discussion to be postponed, as he was collecting statistics bearing upon the subject. He had taught and been taught that the condition of the cervix was some guide as to previous pregnancy or not, but he was rather inclined to believe that the condition of the os was not so clear in deciding the question. It was far too important a matter to be discussed in a few minutes, he therefore proposed an adjournment.

Dr. AVELING suggested that the presence of a large polypus or other morbid growth in the uterus would modify the condition of the cervix.

Dr. BARNES seconded the proposal to adjourn the discussion upon the scientific question as to the evidences of pregnancy, but he wished to express his satisfaction at the course taken by Dr. Meadows in laying a formal statement of his views before his professional brethren in a form that would be understood and appreciated, instead of trusting to the garbled reports of newspapers and the distortions to which scientific evidence is liable under the manipulation of counsel.

The discussion was then adjourned to a future meeting.

---

#### *Notes of a Case of Ruptured Vagina during Labour, with Recovery.*

Dr. HEYWOOD SMITH narrated the particulars of a case of premature labour at the seventh month, where rupture occurred and the patient recovered. The case was complicated by *placenta prævia*, and turning was resorted to. In similar cases he thought it im-

portant to keep the patient lying in such a position as that the uterus should lie above the seat of the rupture and to administer opium for the first forty-eight hours.

---

*Spontaneous Rupture of the Vagina—Recovery.*

Dr. WILTSHIRE also narrated a case where the promontory of the sacrum projected more than usual. The head, with the right leg, right arm, and funis presented. The laceration was transverse at the junction of the vagina with the cervix uteri and extended into the peritoneal cavity. Intestines were felt, but none prolapsed. Delivery was safely effected by turning, and the patient made an excellent recovery. The paper concluded with remarks on the nature, causation, and treatment of the accident.

Dr. BARNES wished to state his objection to accept in its absolute sense the doctrine that the uterus never ruptured unless its tissue was diseased. He himself had seen cases where no disease could be detected. He might distrust his own competency as an observer, but he had submitted specimens of one case to examination by Drs. Bristowe and Montgomery, who confirmed his observation. The conditions under which the uterus sometimes burst were quite compatible with the hypothesis of sound structure. Sudden violent contraction of the uterus upon fluid contents would, as in the case of the Florentine experiment of hydrostatic pressure inside a golden globe, cause rupture, unless the fluid escaped by a natural opening.

Dr. PALFREY had met with cases. He objected to the use of the syringe in these cases. In one instance immediate collapse had occurred in consequence; nothing would induce him to sanction it again. The irrigator answered every purpose where it was necessary to wash out the vagina. If the rupture favoured prolapse of the intestine, he advocated sutures.

Dr. EDIS referred to a case where turning and attempt at extraction had been resorted to in a primipara, the outer posterior diameter of the pelvis being less than two inches. After prolonged efforts the body was extracted, the head being left behind. When Dr. Edis examined the patient the hand passed readily through a large rupture in the upper part of the vagina and neck of the uterus into the peritoneal cavity, the head of the fetus lying close under the liver. The patient died. He mentioned the case as illustrating the necessity of carefully determining on first examination the exact condition of the pelvis.

Dr. BARNES inquired if in Dr. Wiltshire's case the child was dead before rupture took place, because death of the child was often a cause of rupture.

Dr. WILTSHIRE replied that the funis had ceased to pulsate before the rupture. The patient had been kept on her back to prevent the descent of intestine.

*Case of Cesarian Section*

Dr. J. W. J. OSWALD communicated the particulars of a case where the pelvis was very deformed from rickets. The patient died on the fourth day. The antero-posterior diameter of the pelvis was only three-quarters of an inch.

Dr. H. SMITH inquired how far through the walls the sutures were put, what quantity of opium was given, and what was the cause of death. There seemed to have been an absence of peritonitis, and the temperature was never very high. Did death result from septicemia from the fluid effused into the peritoneal cavity? It was a question whether the abdominal wound should not have been reopened and the fluid sponged out. He had seen this done in one case successfully.

The PRESIDENT remarked that the value of the paper would have been considerably increased if the measurements of the pelvis, the curvature of the spine, and the length of the extremities had been given more in detail.

Dr. PALFREY inquired whether sutures had been placed in the uterine walls.

Dr. ROUTH, who performed the operation on the patient, stated that catgut ligatures had been employed; they were of the best kind, and had been kept in carbolized oil. They were securely tied, but had loosened and finally untied themselves, allowing effusion of a quantity of fluid into the peritoneal cavity, and so poisoned the patient. Had silk or metallic sutures been employed, possibly the patient might have recovered.

Dr. HAYES called attention to three loose catgut ligatures being inside the specimen then going round.

Dr. MEADOWS remarked that this was certainly the second case in which death had resulted entirely from the use of catgut sutures in the uterus. He had himself lost one case, which at first seemed to be doing perfectly well, when collapse suddenly occurred and death ensued. The stitches were found to have given way and the wound in the uterus gaping. In future he would employ wire for the uterine stitches.

Dr. CLEVELAND alluded to the common form of weather indicator, constructed on the principle of catgut possessing the property of relaxing in a moist and contracting in a dry atmosphere. Was there no method of preparing it properly beforehand?

Dr. ROGERS questioned whether sutures at all were requisite in the uterus. In two cases he had seen where none were employed, one recovered and one died. One distinguished obstetrician never uses sutures, and others use silver sutures with success.

Dr. OSWALD, in reply, stated that opium in one-grain doses had been given every four hours, and nourishment in form of milk, beef-tea, brandy, &c. The patient was only four feet high, there was no curvature of the extremities, and the pelvis was flattened. He would give a further report of the case for the *Transactions*.



## OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, July 14th, 1875.*Dr. MATTHEWS DUNCAN, *President, in the Chair.*

Professor SIMPSON showed two polypi. The one, a simple polypus, removed by the *écraseur*. The second one was from a patient who had been sent to him as suffering from cancer of the uterus. On examination, the vagina was found filled with a sloughing mass, and there was a quantity of stinking discharge. The patient was very anemic, and her look was very like that of a subject of cancer. The tumour was removed by twisting, and was with difficulty extracted from the vagina after separation. No hemorrhage at the time. The patient did well at first, but died suddenly the following afternoon. There was no blood found in the vagina or peritoneum; the heart was soft and flabby; the fatal issue, which was quite unexpected, may be put down to shock after the operation.

---

Dr. RATTRAY read the following note on a case of hypogastric fistula, &c:—

Mrs. M., a short and stout woman, whose age is thirty-nine, has enjoyed excellent health, except that she suffered occasionally from sore throat. Has had four children, all living and well. In 1871 her mother died of cancer of the womb, aged fifty-two. Nine years ago she had a sister who died of liver and stomach disease, with a doubtful history of "uterine disease." Seventeen years ago Mrs. M. observed a small swelling appear below the navel, which came on shortly after lifting a tub, and has increased in size. Mrs. M. takes simple food, but has had no motion *per anum* since 1st June; but a constant yellowish-coloured discharge, without smell, is coming away from the fistulous aperture in the hypogastric region.

---

Dr. MATTHEWS DUNCAN exhibited some specula for examining the female bladder, of various sizes, and stated that he had recently been paying renewed attention to diseases of the bladder, especially chronic cystitis. With a view to the thorough examination of the organ, he continued to find the sound, used as he had formerly described, very advantageous; showing the size, hardness, and tenderness of the diseased viscus. Recently he had repeatedly rapidly dilated the urethra to a large degree in different cases, and had found this operation give relief to the cystitic symptoms. Through the urethra so dilated, or as dilated by Simon's vulcanite tubes, the interior of the urethra and bladder could be very indistinctly seen. With a view to its thorough visual inspection, he now used common glass specula with internal mirror surface, such as he now exhibited. They were of considerable size, as of  $\frac{3}{4}$  inch in diameter or larger, and showed the inside of the organ as well as similar but larger

specula showed the vagina and cervix uteri. For facility of introduction into the dilated urethra, such specula should be bevelled to a blunt point, as is generally done with vaginal specula. The whole operation of dilating and examining the bladder, exclusive of the use of chloroformization, does not take more than five minutes.

---

The following gentlemen have been elected office-bearers for the next two years:—President, Prof. A. R. Simpson. Vice-Presidents, Dr. James Young and Dr. Alexander Milne. Treasurer, Dr. William Craig. Secretaries, Dr. Charles E. Underhill and Dr. James Carmichael. Members of council, the office-bearers and Dr. Angus Macdonald, Dr. Robert Bruce, and Dr. John Burn. Librarian, Mr. J. Jamieson.

---

*Meeting, Wednesday, December 8th, 1875.*

Prof. A. R. SIMPSON, *President, in the Chair.*

*President's Inaugural Address.*

#### EMMENOLOGIA.

In entering on the occupation of the honourable position to which you have called me, I do not attempt to express in words my grateful sense of your kindness and my high appreciation of the honour you have conferred upon me. Words would fail me worthily to thank you; and instead of having me dilate upon the dignity of the office, it will please you better to see me bind myself to the fulfilment of its duties.

In casting about, in the too brief time allowed me, for a subject on which I might venture to address you, it seemed to me that it would not be inappropriate and might be somewhat interesting if I asked you to look for a little at the phenomena which bring the members of the female sex within the sphere of our observation as Fellows of this Society, and see what insight we have gained into the meaning of menstruation.

It is the occurrence of menstruation, I say, which first renders the female an object of interest to an Obstetrical Society. Perhaps some would add that were there no menstruation our occupation would be gone. If one of the sexes rather than the other has a claim on the interest of the obstetrician when he meets them on their exit from their intra-uterine home it is the male. For his larger head makes his transit through the parturient passages more difficult and dangerous, not only to his mother, but also to himself, than the birth of his small sister; and though the number of boys born be in excess of that of girls, the extra mortality among the males, which is greatly due to the effects of their more prolonged delivery, serves ere long to bring the sexes to a numerical equality. Then differences in the

sex do not specially attract us. The one is so like the other that it requires careful investigation to distinguish between them. I once travelled in the same compartment with a gentleman who was bringing home his wife and baby from a visit; the lady had gone to pay her parents a convalescent visit after her first confinement. She was fond of a joke; and she told me that when her husband arrived she had another child dressed in her baby's clothes, and put into the arms of the unsuspecting father, who admired its growth and might have kissed it often, but that he was told that he was caressing not his own girl but a stranger boy. So like they are in infancy. And much alike they remain in their external appearance, in their mental characteristics, and in their relative proclivity to disease and death, not only during the first septennary and first dentition, but during the second also, when nutrition and growth of the entire system are in active process, until with the arrival of the third septennary the new set of evolutionary changes set in which mark the age of puberty, and which when completed leave the boy transformed through the youth into the man, and the girl through the maiden into the woman. Without dwelling on the diversity of features that now appears in the physical frame and in the psychical manifestations of the two sexes, we may note the remarkable difference they exhibit in their capability of resisting morbid influences. Till now the mortality between the sexes has been nearly equal, boys dying about cent. per cent. with girls. Between the ages of fourteen and eighteen, however, statistics show that for every 100 births as many as 128 maidens die. And this higher mortality continues through all this third septennary, though not so markedly in the later as in the earlier section of it. From eighteen to twenty-one or twenty-two the relative mortality is 105 women to the 100 men.

It is at some time during these years so fatal to her sex that the girl becomes the subject of the change, of which to her the most striking feature is the escape of a bloody discharge from the genital orifice. The date of its first appearance varies considerably in different individuals. Statistics have been collected with great assiduity by many emmenologists, and collated with the view of determining the mean date of the first discharge of the menstrual fluid. When we look at the tabulated results we notice that the year opposite which stands the heaviest figure in the total column is the age of fifteen. Closely following it, and of nearly equal value are the figures opposite the ages sixteen and fourteen. Considerably behind these are the figures opposite the years seventeen and thirteen: still fewer are those at eighteen or twelve. The figures become very low opposite nineteen and eleven, and still lower at twenty. There is still an appreciable proportion at twenty-one; but the figures opposite the years beyond this are so small that the non-appearance of menstruation in any individual who has passed this age may give rise to grave apprehensions regarding her general health or the condition of her sexual organs; and although now and again cases are met with of its out-

burst at the age of ten or earlier, we regard the occurrence as quite exceptional, and we may sometimes find the genital hemorrhages of childhood to be something quite other than a menstrual discharge. The causes of the great range of date in the first appearance of the flow are not easily determined, any more than the diversity in the dates of the evolutions of the teeth in infancy and childhood. The causes may sometimes be due to individual peculiarities of constitution or hereditary influence. But there are some conditions that have been distinctly shown to have a modifying influence.

1. The effect of climate appears in a comparison of the statistics gathered from countries under different degrees of temperature. The tables I looked at in citing the figures above are drawn from information gathered at different points within the temperate zone between  $33^{\circ}$  and  $54^{\circ}$  of N. latitude. If we look at tables showing the mean date of commencement of menstruation in warm climates—between  $33^{\circ}$  and the equator—we see the heaviest figures in the total column opposite the years, not from fourteen to eighteen, but from eleven to fifteen. The heaviest are opposite twelve and thirteen, at which ages a nearly equal number begin to menstruate. Then follow at a marked distance those opposite fourteen and eleven. At fifteen and sixteen a considerable proportion begin to menstruate; but the retardation beyond the age of seventeen is extremely rare, whilst many begin as early as ten, some as early as nine, and a few even at eight years of age. On the other hand, a reference to the tables of the first eruption of the menses in colder climates, between  $54^{\circ}$  N. and the pole, informs us that the mean date is somewhat later. The heavy figures here run from fifteen to nineteen. The heaviest is at sixteen, though fifteen is almost equal, and seventeen not far behind. Opposite eighteen and nineteen the figures are still large, and even opposite twenty. At twenty-one they become distinctly less; but there remains a notable proportion in whom menstruation is delayed till the twenty-second or twenty-third year of life. The figures opposite the years fourteen and thirteen are nearly the same as at twenty-one. At twelve, however, there is a decided diminution, and the appearance at an earlier age in these cold climates is almost unknown. But however well marked the influence of climate may be in the acceleration of menstruation in warm and its retardation in cold countries, there is reason for believing that—

2. *Race* exerts a distinct influence in determining the date of its first appearance. We know, for example, that Anglo-Indians retain in this respect the habit of their race, and instead of beginning to menstruate, like the Hindu women, at twelve or thirteen, first menstruate like their relatives at home between fifteen and sixteen. Among different races in Hungary one observer has noted these diversities in the mean age of the first onset of menstruation. Among Steyerians it began chiefly between the ages of thirteen and fourteen; among Jewesses between the ages of fourteen and fifteen; among Magyars



between the ages of fifteen and sixteen; and among slaves between the ages of sixteen and seventeen.

3. A third element that exerts a notable influence on the date of the first menstrual discharge is the social condition of the female. Thus where the mean period of commencement was fifteen years and four months in all classes of the community, the mean date among the richer classes was fourteen years and eight months, and among the poorer sixteen years. So that *ceteris paribus* we may expect the menstrual discharge to begin earlier in a maiden well nourished and in easy circumstances than in one who leads a life of privation, toil, and care. Other countries have been looked to as modifiers of the date of commencing menstruation, but these are the chief; and passing from this it is time to inquire, What is this discharge that is taking place? Is it a simple hemorrhage?

*Nature of the Discharge.*—Some physiologists note three stages in the flow: first, a stage of invasion; second, a stage of persistence; and third, a stage of decline. In the first brief stage of invasion the discharged fluid is pale and scanty, not unlike the discharge that escapes from the genitals of some of the lower animals at the period of rut, and usually having a peculiar odour, more marked in some individuals than in others. It is like a leucorrheal mucous discharge faintly tinged with blood, and contains many epithelial cells derived from the different mucous membranes lining the genital canals and their follicles, quantities of mucous corpuscles or wandering cells, and a few red blood globules. This preliminary pale discharge has not lasted long when it is followed by the bright red discharge, which continues through the several-days' stage of persistence. The fluid that now escapes presents all the peculiarities of blood drawn from a wounded surface, except that it has little tendency to coagulate. But it is known that the admixture of a small proportion of pus or mucus to fresh-drawn blood prevents coagulation; and when we examine the menstrual discharge chemically and microscopically, it is found to consist mainly of blood mixed with scales and fragments of epithelium from the walls of the vagina and vaginal portion of the cervix, cylindrical epithelium from the canal of the cervix and from the cavity of the uterus and the uterine follicles, many rounded nucleated cells like pus and mucous globules, and compound granular corpuscles. In the third stage of decline or cessation, the fluid gets less in quantity, becomes first darker and less arterial in colour, then pale and turbid, and then more clear, till it returns to the condition in which it was before the invasion. During the day or two of the decline the fluid is seen to contain chiefly mucus and pus with many cells in various stages of fatty disintegration.

*Quantity.*—The amount of blood that thus escapes it is difficult to estimate. The statement that we may count an ounce for each cloth that the patient wears during a period must be received with considerable qualification. The sense of comfort and propriety in different women differs too widely to render such a standard available,

and to estimate the loss of blood at an ounce per diaper would certainly give an exaggeration in the ordinary run of cases. The difficulty of determining this point has led writers to give quite different statements as to what we are to consider as the normal loss—some giving it at two or three ounces, others at double that amount or more. There is certainly such considerable diversity in different women, as to make it impossible to say that every woman who loses seven or eight ounces of menstrual blood is menstruating too freely, or that one who loses less than two ounces is amenorrhœic: and in dealing with individual cases, the important point to be ascertained is the patient's own habitual loss. For though at the first appearance of menstruation the amount may vary from much to little for a few months, it soon steadies to a quantity which the woman will regard as her natural standard, whether it be free, as 53·5 per cent. express it, or moderate, as in 16·5 per cent., or small, as in 30 per cent. is said to be the case. The habitual loss once established should persist throughout the reproductive period, and its modification in any marked degree, either in the direction of plus or minus, we properly regard with suspicion as an indication of disease.

*Duration of the Discharge.*—Closely connected with the question as to the amount of the discharge, is that as to the duration of the flow. In this matter also there is great variation. But most women tell us that the discharge lasts with them month after month for the same length of time. With only some hours of more or less, the flow lasts for the same number of days. Some 92 or 93 per cent. thus report the duration of the discharge to be steady. In the remaining 7 or 8 per cent. it lasts a variable length of time, sometimes for three days, sometimes for six or eight; so that they cannot tell how long the courses will run. Of those with steady duration of flow, some—and these number fully 26 per cent.—menstruate for eight days each time; some (21 per cent.), three days; some (17 per cent.), four days; fewer (12 per cent.), five days; still fewer (10 per cent.), six days; and a very few (4 per cent.), seven or two days. Duration of less than two or more than eight may be regarded as abnormal, or should at least call for special inquiry as to the general or local condition of the individual case. It has been observed that there subsists this relation between quantity of menstrual fluid escape and the duration of its flow, that where the blood is paler and scantier, the duration is shorter; and the longer the duration of the discharge, the more profuse is the amount of blood discharged, and the greater tendency does it show to be mixed with clots.

*Frequency of the Flow.*—But we have still to note another feature, and that to us as obstetricians the most important in connexion with this hemorrhage, and that is the frequency of its appearance and the regularity of its return. It is this periodicity of the hemorrhage which has given to it its common designations—catamenia, menses, monthly periods, les règles, les époques, &c.; for once it has become established, it regularly recurs at somewhat definite periods.

In something like 87 per cent. of women, at least, the type of menstruation is reported to be regular. In the remainder the menstrual cycle is of indeterminate length: the interval between the commencement of one period and the recurrence of the next is so uncertain that the woman does not know when to expect it. Among the women in whom the cycle is constantly or nearly constantly of the same duration, we find it presenting different types. Thus some, and these by far the largest number (—71 per cent.) have menstruation of the 28-day type, 28 days intervening between the commencement of two successive menstruations. The next most common is (—14 per cent.) the 30-day type. More rare are (—2 per cent.) the 21-day, or (+1 per cent.), the 27-day types. Figures such as these, gathered as they are by inquiries made of patients, are not to be received as altogether exact; and there is good ground for doubting whether in almost any woman we should regard the type as absolutely constant, and the menstrual cycle unvarying; but they are sufficiently correct to guide us in our deductions from their physiological or pathological interruptions. What we have to remember is, that the menstrual month, while it may correspond with the solar of  $30\frac{1}{2}$  days, or the lunar of  $29\frac{1}{2}$  days, or with the calendar month of from 28—31 days, may be distinct from all, but in general retains in each individual woman its own individual type.

*Ancient Theories.*—It is time we were inquiring, What is the meaning of this menstrual discharge? You know the theories with which the fathers of medicine were fain to satisfy themselves regarding it. It was held by some to depend on the elimination of some noxious material from the female economy; and this chemical theory met such wide acceptance that among many communities menstruating females were doomed to segregation during their periods, as though there was issuing from them some exhalation that could defile their neighbours, damage fruit-trees, poison bees, turn the milk, &c.; and this theory we trace in the Hippocratic designation “katharses,” our “cleansings,” and the German “Reinigung.” Again, by many it was held to be due to plethora. In the developed female it was supposed a surplus quantity of blood was formed ready to furnish material for the growth of an embryo, but no conception occurring the system got relieved by the periodical loss. Some were even so precise as to allege that the weight of an infant corresponded to the weight of the blood that would have escaped during its gestation had it not been present to appropriate it. Yet again it was argued that it was an unnatural occurrence—superinduced by the irregularities of civilization, said one—resulting from the non-satisfaction of the reproductive instinct, said another—due, said a third, to the erect position of the human female as compared with the lower animals, one of the drawbacks of the *vultus erectus ad sidera* of the human race.

*Source of the Discharge.*—But, as you also know, modern research has traced the discharge to its proper seat, and has shown it to be



associated with changes that certainly exert a modifying influence on the general system, but that have their immediate source and spring in the sexual organs themselves. When we come to search for the exact seat of the hemorrhage, it is easy to exclude the vaginal walls and vaginal portion of the cervix uteri. There is no good evidence to show that it even comes from the canal of the cervix below, or from the Fallopian tubes above; but there is the clearest evidence that it takes place in the cavity of the uterus itself. There was a time when the word *matrix* was used to designate all the hollow organs opening at the genital orifice. By-and-by, the vagina was differentiated from the body which we designate the uterus. But we cannot rest here, for the uterus proper, marked off by the *os internus* within and the *externus* on the surface, must be held to be a distinct organ from its cervix anatomically, physiologically, and pathologically. Very intimately connected with it, indeed, in all these respects, and yet perfectly distinct, as comes out very clearly when we examine more carefully the local changes associated with menstruation. There is at the period increased vascularity and hypersecretion taking place from the canal of the cervix, with intumescence through all its walls, as in a less degree there is in the vaginal tube. But in the uterus proper there are these, and more. If we look at the uterine mucosa some ten days before a menstrual epoch, we notice that the almost smooth surface is beginning to be interrupted by the elevation of patches with furrows between, marking them out into irregular folds: the orifices of the uterine follicles, before hardly discernible, become now easily visible; and the pale, reddish-grey tint gives place to a darker red as the deeply injected capillaries are seen running in lozenge-shaped loops around the follicular openings. This tumefaction of the mucosa affects all its elements. There is a proliferation of the epithelial cells lying on its free expanding surface, and lining its many follicles: the interglandular connective tissue shows also increase in number and size of its corpuscles, with infiltration of its intercellular substance, and even the capillary tubes seem to sprout and multiply. This hyperplasy of the mucosa which causes it to wrinkle up within its limited cavity, reaches its height when the return of the catamenia is due, but for the preceding week it has already been so great that, in the lower part of the cavity at least, the anterior and posterior walls of the uterus were crowded together, and the transmission of any fluid along it then must be regarded as exceedingly improbable. When the day has come the mucosa is in a condition of what has been fitly described as acute catarrh; the cellular elements on the surface and nearest to it, become the seat of cloudy swelling; it is but a step further, and that passes over into fatty degeneration; the disintegrated elements break down as the period of invasion sets in; the capillary tubes get broken open, and the extravasated blood with its admixture of mucus begins to trickle away, and then to flow more freely through the stage of persistence until it is succeeded by the mucous or muco-purulent discharge of



the stage of decline. At this time it is found that a distinct layer of the uterine mucosa has moulted or been shed off; for instead of the smooth surface covered with ciliated epithelium, continuous with that of the cervix, there may be seen from the sharply defined line of the os internum upward a somewhat fretted surface, in which it requires close examination to detect the follicles, with a fine gelatinous layer of connective tissue around them, through which may be sometimes seen the fibrous bundles of what has been shrewdly described as the muscularis mucosa—*i.e.*, the innermost layer of the muscular walls of the uterus, in which the blind extremities of the follicles are deeply and firmly imbedded. On the layer of mucous membrane that is left there begins at once a process of repair, and in a few days its restoration is complete.

But we have yet to find the impulse to this cycle of changes in the uterus and its mucosa in particular. To discover it we must look elsewhere. Ere we leave the uterus, however, let us note that the changes in it we have looked at point it out as an organ marvellously adapted for the function of serving as the nest in which a fertile egg might be incubated. Its soft and succulent mucosa will furnish an easy soil in which a body capable of self-development may root itself: its capacity for growth will enable it to expand with the increase of its growing contents; and when the embryo has reached maturity, its muscular fibres will be ready to effect its delivery. Hence the preparation in the mucosa for the reception of such an egg has been called its nidation, and the partial exfoliation that takes place at the period of the flow has been called the denidation of the uterus.

*Ovulation.*—It is therefore when we search for the egg and the conditions under which it is laid, that we find a key to the explanation of the changes which take place in its well adapted nest. There we find of course the egg-bearers—the ovaries. In the deep layer of their parenchymatous zone we find a fertilizable ovulum. According to some physiologists it takes nine months from the period when the ovulum begins to ripen until its ovisac reaches the surface of the ovary. However this may be, a time arrives when the nutritional impulse inherent in it starts it on a new stage of more active growth. As the ovisac expands it produces irritation of the ovarian nerves, which is transmitted to the central ganglia, and is reflected thence in such a manner to the whole of the sexual apparatus that hyperemia results—perhaps at first but as a faint and transient blush—but increasing with each new wave until the well-known changes take place which lead to the dehiscence of the ovicapsule. The ovum thus expelled has great potentialities within it: but these are all dependent on the access to it, and probably at this point, and the amalgamation with it of spermatozoa. Let the spermatozoa fail the ovum here, or in the oviduct into which it is received, and it will move along whithersoever it may be carried, exerting no further influence on its surroundings than any epithelial

cell by which it may be accompanied. So it will drift through the Fallopian tube, and on into the cavity of the uterus, where already it may be the surface has begun to melt down, and with the menstrual discharge it will be washed away. There has been a birth of an unimpregnated ovum.

Not, perhaps, with such perfect adaptation, but with some such close relationship in time and function, will the ovary discharge its ovisac, and the uterus shed its surface, period after period, as each new follicle takes on more active growth and rises to maturity on the disappearance of its predecessor. And when we are looking thus closely at the central point in the phenomena of menstruation, we can easily comprehend how apart from climatic and racial and other general conditions, the rate of rapidity with which each Graafian follicle ripens and bursts will depend on individual and special conditions in the ovaries themselves. The regular evolutions of successive ova once fairly started, might go on for long, it might be too long to render the young lady in whom we have traced the establishment of the function, an object of interest to us. So, as we have come within sight of a leap-year, let us wish her a happy new year, and a husband before it runs out. Then let us hope that one of us will be called in to interpret for her the non-appearance of the usual menstrual discharge.

*Conception.*—We will take it for granted that an ovum has been fertilized. But it may be questioned at what epoch did the ovum escape from its capsule. Was it the ovum from the period when last a catamenial discharge took place? or was it an ovum that was shed when the next discharge was due? Up till a recent date the almost universal answer would have been that it was the ovum separated at the time when a uterine discharge occurred that became impregnated: and hence it was supposed the sooner subsequently to a menstrual epoch that connexion was accomplished, the more chance there was of the ovum being fertilized. It was even averred that there was an agenetic period, dating from twelve or fourteen days after a catamenia till the next was due, during which semen might be discharged into the vagina without interrupting the menstrual cycle, because the ovum of the last epoch had by that time perished, and spermatozoa could not have access to another ovum till after the following epoch and the rupture of a fresh ovisac. Such a period of agenesis, however, cannot in the face of well-known facts be admitted, and if a single coitus practised within a week of the commencement of a menstrual epoch be less likely to prove fruitful than a single coitus of an earlier week, the circumstance is susceptible of another explanation. For during the pre-menstrual week, as we have seen, the uterine mucosa has swelled to such a degree as to block up the uterine cavity, and prevent the upward migration of the spermatozoa; whereas during the two post-menstrual weeks the whole canal is more easily permeable for them. It may even be that there is special facility for their transit during the first post-menstrual days,

ere the uterine mucosa has become regenerated and its epithelial cilia begin to cause a downward current, as they are believed to do, from fundus towards os. If we be correct in speaking of each ordinary menstrual discharge as the accompaniment of the birth of an unfecundated ovum, as many recent observations seem to indicate, then we are forced to the conclusion—and I am quite disposed to believe it is correct—that the fecundated ovum of a normal pregnancy is a product of the period that has become due, *and that has duly come*, though it be not distinguished by the occurrence of a uterine hemorrhage. The interruption to menstruation we trace to the presence of spermatozoa in the genital canals, and we have to inquire at what plane they come into contact with the ovum. We have seen how easily they have access to the higher planes during the early part of the intermenstrual interval, and there is no reason to doubt that when the generative *mucosæ* of the female are in a healthy condition the spermatozoa will retain their vitality in their mucus for a considerable period. That they meet the ovum in the cavity of the uterus itself we can hardly conceive: we may conjecture that they meet in the Fallopian tubes: but we know that they do sometimes meet on the surface of the ovary. In this situation, in fact, we sometimes find the ovum undergoes its further development. For though the uterus be its usual and most appropriate nest, it may get lodged and incubated in some other cavity. In most of these extra-uterine gestations, the nest fails to meet all the requirements of the growing ovum. Either there is not a soil in which the chorionic villi can take root, or the walls of the nest give way under the bursting pressure of the expanding ovum, or the fully developed product is imprisoned till it dies. But whatever the ultimate history of these ill-lodged ova, their occurrence teaches us that the spermatozoa traverse all the mucous canals sometimes, and warrant us in believing that when a menstrual era has arrived, they may in ordinary cases have found their way to the surface of the ovary, so as to fertilize the ovum at the moment of its escaping from its ovisac. The ovum thus fertilized is no uninfluential, passive, perishing cell, like its unfertilized predecessor of the foregoing epoch. It has within itself a new life-power, a power of self-development. Evolutionary changes are set up in it, and in virtue of these it will at once begin to exercise a potent influence upon the structures with which it comes into relationship. By the time it has passed into the cavity of the uterus its chorionic villi are ready to lay hold of the soft and swollen surface of its lining membrane; that mucous membrane which was in a state of growth verging on decay receives a new impulse which leads to active proliferation of its elements, so that it grows up around the body that has got engrafted on its surface till it surrounds it with a distinct capsule—the decidua reflexa. At the spot where the ovum became rooted in it (decidua serotina) a mutual inter-growth takes place between the structures on the chorionic surface and the structures in the mucosa, until the vascu-



larized villi are hanging free into blood-pools channelled in its tissues. Throughout the rest of the cavity the mucosa (decidua vera) goes on developing until, as the ovum expands within its new capsule, the outer surface of the latter becomes amalgamated with the free surface with which it is in apposition. In this way the disintegration of the uterine mucosa is for that period arrested, and there occurs no menstrual discharge from what has now begun to be a gravid uterus.

*Gestation.*—But probably our patient will be less curious to know the cause of the non-appearance of the menses than anxious to be told the probable term of their return, or rather as the question will present itself to her, what is the probable date of her confinement. Shall we attempt to fix a given date as woman's standard term of utero-gestation? You know how piously Harvey has fixed it at the 275 days which, as he states, begin "from the festival of the Annunciation in the month of March, to the day of the blessed Nativity, which we celebrate in December." But when we seek authority for the dates we do not find them in the only Book which could have told us, for it was given to tell us of other things. The dates are derived only from the teachers of the Roman Catholic Church, and when their true meaning is investigated it is found that the 23rd of March was held as Lady-day in Pagan Rome, in honour of Cybele, the mother of the Babylonian Messiah, long before the era of Our Lord; while the 25th of December was honoured among many Gentile peoples as the birthday of the son of that "Queen of Heaven." It thus carries back to far remote ages the observation that this length of time was looked on as the ordinary period of human gestation. Statistics meritoriously compiled have shown that what was true then is near the truth still. But the patient who has asked for our opinion as to her probable term is not likely to be able to give us a definite day to count from; and if she did and we were to refer her to the standard measure, we should be as likely to set her wrong as right. If we are to make the closest possible approach to the calculation of the probable date of her confinement, we must above all have regard to her own individual menstrual type, and ascertain what is with her the usual length of a menstrual cycle. The ovum which took nine months of so many days to ripen within its ovisac will after it has become impregnated take the same number of months to undergo its development within the uterus. We have therefore to find out the number of days between the commencements of the two menstruations that preceded conception, and multiply the figure by ten; and within a range of five days earlier or later the birth of the impregnated ovum will take place. In the round and rough we are safe in following the usual calculation of counting nine calendar months from the date of the commencement of last catamenia and adding a week, because the greater proportion of women have the 28-day type of menstruation which closely answers to this calculation. But where we are specially anxious to avoid error and to attain the



nearest approach to accuracy, we must, I repeat, ascertain the individual menstrual cycle and expect the delivery on some day in the decade, the central day of which corresponds to the date when the tenth menstruation should be due. For by the return of this tenth menstrual epoch the fetus has become fully developed: the once greatly hypertrophied mucous membrane of the uterus has become stretched and thinned, and its superficial portion has undergone in great part of its extent a marked degree of fatty degeneration, while according to recent observation the necks of its follicles have become dilated till there are mere threads of intervening connective tissue left between. As a result of the changes going on in the lining membrane of the uterus leading to partial desquamation of its surface, the sensory nerves are irritated. The irritation conveyed to the centres is reflected to the sexual organs, where it is manifested first in an increased mucous secretion corresponding, though in an exaggerated form, to what we see in the invasive stage of an ordinary menstruation, and secondly in contractions set up in the muscular walls of the uterus, at first so slight as to be hardly appreciable, but increasing in intensity and continuing until the ovum has been expelled *non sine labore*. Its expulsion is also attended, as was the non-laborious birth of its predecessor, with a hemorrhage (the red lochia), such as goes on during the Persistence stage of ordinary menstruation, and afterwards a paler mucous discharge (the white lochia), as in the stage of Decline. The internal surface of the uterus when examined at such a time presents appearances similar to those which we found immediately after the cessation of the catamenia, only on a greater scale, as the whole organ has undergone an enormous degree of hypertrophy during its gestation of the ovum. In a short time, however, the disintegrated surface of the mucosa is regenerated, and the worn-out muscular fibres have melted down and have been replaced by younger cells and corpuscles, and the uterus is ready to resume once more its nine-months-interrupted series of changes.

*Lactation.*—The date of the resumption of these changes will depend, however, on the condition of the ovaries; for as we have seen they take place in concert with the dehiscence of an ovisac. Now there is no evidence to show that ovulation goes on, for example, during utero-gestation. Rather we must believe that whilst the nutritional energy of the female is demanded for the evolution and growth of one fertile ovum, there are no fresh ovisacs ripening and bursting; and hence we look upon menstruation during pregnancy as an altogether abnormal occurrence. But the woman is not done with the claims of the ovum, whose history we have traced down to its expulsion from her womb. Further changes of growth and development in the child are to take place, which demand constant nutritive supplies, and for these it will be dependent on the mother's milk, and this demand on her nutritional energy will again run on for an average duration of nine more months. It is certain that during the period of lactation in a large proportion of women the

ovaries have already begun to resume their functional activity. Still we are to regard the non-appearance of the menses during lactation as the rule, and it is only after the infant is weaned and the nutritional energy—whatever that may involve, not only of blood-supply, but of nervous influence—is free to be directed once more to the ripening of ova within their ovisacs that we are to expect the normal re-establishment of menstruation.

*The Menopause.*—I will not weary you by dwelling on the pathological conditions under which menstruation may be modified, so that the discharge becomes too profuse, or painful, or perverted, or suppressed. I fear I have already drawn too long upon your kind attention: after all, some one may rightly charge me with having treated all too cursorily the great subject of emmenology. Let the suddenness with which the intimation came upon me that an address was expected from the new President to-night be some apology, and let me hasten to add in conclusion that in following out the history of our interesting patient, at whose several safe confinements we shall suppose ourselves during a series of years to have assisted, there comes a time when the menstrual discharge will cease to flow. Her married life may be running on its happy course; but, sometimes slowly, the discharge lessening month by month, sometimes suddenly, without any premonitory change in its character; sometimes again, after a sudden increase in the quantity, or some change in the quality, or brief repeated interruptions of its regularity, comes a time when it finally disappears. A very variable time the date of its cessation is, having a far wider range than the date of its first appearance. In some cases the menopause has already arrived at an age when the menses often first appear in other women,—at the age of twenty-one; and at the other extreme we find it running on till the age of sixty-five or seventy. The usual range which we may regard as normal, however, runs between thirty-five and fifty-five, the larger proportion falling within the central decade. In nearly a half, menstruation ceases between the ages of forty-five and fifty; in fully a quarter, between forty and forty-five; in rather more than an eighth, between fifty and fifty-five; and in the remainder between thirty-five and forty. It has been noted that women whose menstruation is late in making its first appearance, have a tendency to early menopause; the duration of the reproductive life in them being thus shortened at either end. On the other hand, the early menstruation is not so likely to be associated with delayed cessation. At least in hot countries, where the early menstruation is the rule, the menopause comes on at a much earlier period than in temperate climates, and the duration of the reproductive life of the female becomes so curtailed as to have led to the remark that the three or four wives of an Abyssinian only equal in fertility the single wife of a European. If now we examine the condition of the sexual organs to discover the cause of this cessation of the hemorrhage which for thirty years or more has been continuing with such regular periodicity, we find that

in the ovaries the relatively few ovisacs that remain unruptured show little tendency to further evolution ; and if there were the attempt at development in one, it would find its expansion impeded by the density which has settled down on the surrounding stroma, and its dehiscence prevented by the interposition of firm fibrous layers between it and the surface. It is quite possible that this state of matters obtains for some time in the ovaries before the discharge finally disappears from the uterus. But more or less consentaneously, and in some cases apparently at an even earlier date, changes set in in the uterus also which destroy the power in its mucous membrane of periodic proliferation, desquamation, and repair. Then many of the peculiar attributes of mind and body of the female become modified, so that again she comes to bear external likeness to her male companion—a likeness in some instances so striking, that a stranger meeting her and her husband casually will say, How like these two people are—they must be sister and brother ! In any case her name now disappears from the list of our midwifery engagements, and the happiest meeting we are likely henceforth to have with her is in circumstances where none can render such efficient help as she—at the confinements of her daughters or other women of a younger generation.

## OBSTETRICAL SOCIETY OF DUBLIN.

*Meeting, Saturday, November 20th, 1875.*

LOMBE ATTHILL, M.D., *President, in the Chair.*

### *President's Inaugural Address.*

GENTLEMEN,—The commencement of another session, the thirty-eighth of your Society, demands, in accordance with the usual custom, some observations from the outgoing President. First of all, I desire to thank you for the honour you have done me in placing me in this chair, an honour enhanced by the fact that it has been filled by men so distinguished as my immediate predecessors. Some of these eminent physicians are now present—of them it is unbecoming I should speak. Two others we have virtually lost since last we met—namely, Dr. Evory Kennedy and Dr. Churchill, who have withdrawn from practice, and ceased to attend our meetings. To them a few words of homage are, I think, justly due. The former, were it only on the grounds that he founded this Society, we should hold in grateful remembrance ; but he has, besides, other and substantial claims on our regard. Placed forty-two years ago at the head of that great institution over which I have now the honour to preside, he speedily recognised the field which lay before him in the investigation of the then unknown affections of the uterus and its appendages ; and if to us, who have profited by the labours and experience of Bennett, Simpson, Marion Sims, and a host of other



writers and thinkers, Kennedy's observations, published more than thirty years ago, seem imperfect, let us remember that he was a pioneer, an original observer. I have no hesitation in expressing my opinion, that as he was the founder of this Society, so he was the first Irish practitioner who directed special attention to that important branch of our profession now embraced in the term *gynecology*, and as such, we are deeply indebted to him.

Of Churchill I feel it difficult to speak, endeared as he is to us all—kind and frank by nature, rendered doubly so by that unostentatious, but fervent, piety which pervaded his every action. He is a kind and sincere friend, ever ready to impart to others that information which his great experience and untiring industry had enabled him to acquire. In his early career he had much to contend against, and when success had crowned his exertions, it seemed to be his greatest pleasure to encourage those who, weary with the struggle, were down-hearted and desponding. I shall ever remember with gratitude the words of encouragement offered by him to myself in bygone years, when, weighed down by care and disappointment, I almost despaired of success. Dr. Churchill retires from among us with the sincere and deserved regret of all, and the equally well-deserved respect of, I believe, every member of this Society. It is superfluous here to say aught of him as a practitioner. Few, indeed, there are of us who have not from time to time availed themselves of his professional skill in trying and difficult cases, while his reputation as an author is, probably, greater than that of any other Irish practitioner. Though removed from amongst us, he is still of us. Long may he be spared to enjoy his well-earned retirement.

Turning from those who have ceased to take an active part in our proceedings, I must refer briefly to the work done in the past session, and I think, on careful investigation, it will be found to compare favourably with that effected by kindred Societies. Bearing on the physiology of utero-gestation, as regards its departure from the normal process, we have had from Dr. M'Clintock an able and exhaustive paper "On Morbid Retention of the Dead Ovum," and a singularly interesting contribution from Dr. More Madden on "Monstrous Births and the Influence of Maternal Impressions on the Fetus in Utero."

In midwifery, "The Clinical Report of the Rotunda Hospital for the year 1874," so carefully and laboriously compiled by Dr. George Johnston, is deserving of special attention; while Dr. Denham detailed an interesting and instructive "Case of Extra-Uterine Gestation," and Dr. Roe one of "Retained Placenta." Dr. Byrne contributed an essay on "Combined External and Internal Version," and Dr. More Madden another "On the Use of the Short Forceps as a Tractor, and on the Long Double Forceps as a Compressor and Tractor." Dr. Nichols related his experience in cases of "Protracted Labour and Hour-glass Contraction," and Dr. MacSwiney the particulars of a case in which true epileptic convulsions occurred during



labour, rendering delivery necessary under circumstances of great difficulty.

In the subjects embraced under the head of Gynecology and Uterine Surgery we have had from Dr. Kidd a most important communication, embracing the details of several cases in which *uterine tumours* were removed successfully by means of an operation which, in its leading features, is essentially his own—an operation adopted now by gynecologists, not alone in this country, but also abroad, and which is, perhaps, the greatest improvement made of recent years in uterine surgery. I allude to the dilatation of the uterus by the introduction, at one time, of numerous pieces of sea-tangle, and the subsequent removal of intra-uterine polypi by means of the *écraseur*.

Dr. Croly brought under the notice of the Society "A Case of Ovariectomy," and Dr. More Madden one of "Metro-peritonitis following Vaginal Injections."

Such have been our contributions to the literature of obstetrics and gynecology. Several of these are of great value, and calculated to advance our knowledge of the subjects of which they treat.

In other Societies contributions of great value have also been made, likely to influence in a marked manner the future treatment of uterine disease. Of these none, in my opinion, are of greater importance than those of Dr. John Williams, of London, "On the Structure of the Mucous Membrane of the Uterus, and its Periodic Changes."

Let me for a moment recal to your minds what until recently has been the amount of our knowledge respecting the menstrual function. Dr. Carpenter, in his "Principles of Physiology," published in 1842, says:—"The catamenial discharge appears normally to consist of blood deprived of its fibrine, the fluid being composed of serum, in which the red corpuscles are suspended. . . . Much discussion has taken place respecting the causes and purposes of the menstrual flow, but no satisfactory conclusion has been attained."

This confession of inability to explain a function of such importance has been reiterated since then by most of the systematic writers on physiology, and is still commonly repeated in our schools. But of late numerous observations have been made with great care, by Dr. Arthur Farre and others, on the structure of the lining membrane of the uterus and the changes it undergoes, the result of which may be briefly summed up by saying that each menstrual period consists mainly in the disintegration, or, as some will have it, the "desquamation" of the lining membrane of the uterus, which process is accompanied by the exudation of a greater or less quantity of blood. The menstrual discharge then, does not, as was formerly believed, "consist of blood deprived of its fibrine," but of disintegrated epithelium, mixed with blood in very variable proportions. But if this view is true, as I believe it to be, the menstrual flow becomes merely the termination of a process, slowly and gradually completed, and not a special function *per se*, for if the lining membrane of the uterus be

disintegrated and cast off at each menstrual period, it must be as frequently reproduced. The catamenial flow, therefore, must be to the intra-menstrual period what the lochia are to pregnancy—a discharge mainly composed of effete materials. To treat aright disorders of menstruation, then, it follows that we must rather consider the condition of the uterus during the intra-menstrual period than during the time occupied by the flow, which is but the termination of a prolonged process—namely, the gradual development of a lining membrane—a process to which Dr. Aveling has applied the expressive name of “NIDATION,” and which is defined by him to consist in the periodic development of a membrane lining the interior of the body of the uterus, “which fits it for the reception of the ovum,” while to the period occupied by the disintegration of this membrane, commonly called the “Catamenial,” or “Menstrual Period,” he applies the term “DENIDATION.”

On the present occasion it is not my intention to discuss at any length the physiology of menstruation, a function still but imperfectly understood. To those who desire to become acquainted with the exact state of our knowledge on this subject I commend the perusal of the articles published by Dr. John Williams, of London, in the second volume of *The Obstetrical Journal*, and which I have already referred to. I desire merely to point out the bearing which, in my opinion, our recently-acquired knowledge of the changes which the uterus periodically undergoes has on the treatment of uterine disease, but to do so it is necessary to summarize the results which the researches of Dr. Williams and other observers go to establish.

As may be inferred from the quotations I have already made, Dr. Williams holds that there is no such thing as “a period of uterine rest,” what we usually term “the menstrual period” being but the termination of a period of extreme uterine activity, during which the uterus is being fitted to receive the impregnated ovum. In fact, he considers the time occupied by the catamenial flow, so far from being a period of unusual excitement and activity, to be probably the very reverse.

During the continuance of the menstrual flow the mucous lining of the uterus rapidly undergoes fatty degeneration and disintegration, and at its termination the inner surface of the uterine wall is found to be bare, the muscular fibre cells being exposed; this condition, however, soon ceases to exist. Within three days of the cessation of the flow the muscular fibres are covered by a thin layer of superficial tissue, which rapidly becomes developed, and soon presents the appearance of a thick, soft, mucous membrane, which in its turn undergoes fatty degeneration and disintegration, unless impregnation taking place, a further series of changes occur, to which it is foreign to the scope of my present purpose to refer.

The views I have thus briefly summarized open up numerous questions of interest. Prominent among these is one, hinted at rather than expressed by Dr. Williams—namely, Is the uterus a

muscle, properly so called, at all? But, leaving the discussion of this and other purely physiological questions to those who are more capable of deciding them, I will ask you to consider the practical inferences to be deduced from the facts recorded by Dr. Williams.

Till within a comparatively recent period no attempt was made to cure patients suffering from uterine disease, except by constitutional treatment and by the application of such agents as the solution of nitrate of silver to the vagina and vaginal surface of the cervix uteri. Now, however, it is almost unanimously admitted by those who have paid careful attention to "those diseases which are peculiar to women," that in the great majority of such cases it is not the vagina which is the seat of disease, nor yet the cervix uteri, but the body of the uterus, and of the intra-uterine surface specially. Speaking of cases of uterine disease met with in my own private practice, and of which I have the notes, I find that the body of the uterus was implicated in not less than 70 per cent., excluding all those in which tumours of any kind existed.

This being so, it becomes necessary in such cases to direct our treatment to the body of the uterus, and frequently to make applications of various kinds to the interior of the organ. The success of this kind of treatment, when judiciously carried out, has been very great, but I believe it will be still greater as our knowledge of the function of menstruation increases, understanding that term in its most extended sense as implying both "Denidation" and "Nidation."

I think I can without difficulty show that the knowledge attained by the recent investigations which I have alluded to already throws light on the treatment of certain forms of uterine disease. Every member of this Society is aware that strong caustics, such as chromic acid, nitric acid, and the solid nitrate of silver, are now daily used by gynecologists in the treatment of uterine disease. In employing these agents to the interior of the uterus, I long ago observed that sometimes the occurrence of the next catamenial period was accelerated and the amount of the discharge increased as the result of the application of these agents, and this even in cases in which the treatment had been adopted for the cure of menorrhagia depending on an unhealthy condition of the intra-uterine surface, and in which I had from previous experience anticipated an opposite result. In carefully watching the cases subjected to this treatment, I observed that an increased flow generally occurred when a caustic was applied shortly before the expected appearance of the menstrual discharge. The reason why this was so is, I believe, now capable of explanation, for if a caustic be applied to the interior of the uterus shortly before the occurrence of a menstrual period, it is calculated to destroy at once the vitality of the lining membrane, which is already ripe for disintegration, and therefore accelerates that process, while it is easy to conceive that the stimulating application made to the inner surface of the uterus increases the determination of blood, which naturally exists towards it, at the close of the intra-menstrual period, and that



thus the flow comes to be increased. Therefore, as a rule, I now never apply a caustic to the interior of the uterus in cases of menorrhagia, except immediately after or a few days subsequent to the termination of a catamenial period.

By selecting this time two advantages are gained :—Firstly, you apply the caustic directly to the true uterine wall, which, and not the ephemeral mucous membrane, is, in my opinion, most frequently the seat of disease ; and secondly, you apply it at the time in which the utero-ovarian system is at its minimum degree of activity.

But I believe there is another important practical inference to be deduced from the knowledge we now have as to the nature of the menstrual discharge—namely this, that if the discharge be mainly due to the disintegration and casting off at definite periods of the membrane lining the uterine cavity, may not certain forms of amenorrhea depend on some abnormal condition of that membrane, whereby the disintegration is imperfectly effected, or not effected at all, and should not such forms of uterine disease be treated by the application of agents calculated to hasten and bring about this disintegration ? I believe the practice based on this inference will prove to be correct. Long before my attention was drawn to the theory of menstruation, as elucidated by the researches of Dr. Arthur Farre, Dr. Williams, and others, I had commenced the practice of applying a caustic to the interior of the uterus in certain cases of amenorrhea. I was led to adopt this practice from the fact already referred to—namely, that I had observed, when a caustic was so applied shortly before the expected occurrence of a menstrual period, the flow was likely to be increased, and in several cases I obtained satisfactory results. The correctness of this practice, commenced as an experiment, founded on a clinical observation, is, I believe, confirmed by the results of Dr. Williams's observations, and I am of opinion that if suitable cases be selected—namely, those in which organic disease neither of the lungs nor of other organs exists, and which are free from any symptoms indicating the existence of inflammation of the uterus—the treatment I have pointed out will prove very useful. As yet my experience on this point has been but limited, and I am therefore unwilling to lay down very definite rules ; but the results of my practice has been encouraging. The caustic I have generally used for the purpose has been carbolic acid.

The following case is an example of the effect of this method of treatment :—

Mrs. —, the wife of an army medical officer, came under my care on the 1st March last. She had been confined of her first child in the previous month of November. Her labour had been difficult, and she had been delivered with the forceps. Three days subsequent to delivery she was attacked with some form of pelvic inflammation, which terminated in the formation of an abscess, which discharged per vaginam. She made a good recovery ; but, although



she had not been able to nurse, she never menstruated since her confinement. Her general health was now good, but the non-appearance of menstruation caused anxiety. On a vaginal examination the uterus was found to be in its natural position, and to be normal in size and shape; nor was there any symptom present indicative of uterine or ovarian disease. I advised her to go to the seaside, believing that in the absence of any actual disease menstruation would recur without any special treatment as her health became better. This opinion proved incorrect. In September, after a six months' residence in the country, a considerable portion of which time had been passed at the seaside, no return of the menstrual discharge had occurred, and this even though she had become quite robust. Acting on the principle I have already spoken of, I now decided to try the effect of the intra-uterine application of a caustic, choosing carbolic acid for the purpose. The result was marked. On the third day after the application menstruation set in, and lasted for three days without pain. The next menstrual period occurred at the regular time, and the function is now perfectly re-established.

I need hardly add, speaking before a Society such as this, that the treatment I have been speaking of is applicable to a limited number of cases only.

There is yet a further view of the possible pathology of some forms of amenorrhea opened up by the knowledge we now possess of the physiology of menstruation. If menstruation consist in an essential degree in the gradual development of an intra-uterine mucous membrane and its subsequent disintegration, may not amenorrhea in some of its forms, and dysmenorrhea possibly too, depend on the total or partial failure of the uterus to produce this membrane? My conviction is strong that such is the case, and here again the task is set before us of endeavouring to devise a scientific mode of treatment which will, by restoring a healthy tone to the uterus, favour the reproduction of this essential element of menstruation. To me it appears clear that the so-called "emmenagogues" are, in such cases, certain to fail, even if not productive of actual harm, and that we must direct our attention more and more to local treatment if we hope to avoid the imputation of being mere empirics—a charge unfortunately only too true when alleged with reference to many practitioners who undertake the treatment of uterine disease. The conclusion to which I am forced, alike by the result of clinical experience and logical inferences from the data supplied to us, is that both amenorrhea and menorrhagia are comparatively rarely the result of constitutional causes.

Of course, every one is aware that in phthisis, and in some other forms of organic diseases, amenorrhea is a prominent symptom. To such cases I am not at present alluding, but to that very large class in which the departures from normal menstruation cannot be explained by the existence of such affections, and in which it is too commonly the custom to exhibit what are, in my opinion, improperly

termed "emmenagogues." On the other hand, I see the danger which would occur were practitioners to treat cases of amenorrhea indiscriminately by the application of caustics to the interior of the uterus. I warmly protest against such an inference. The only hope of preventing the possibility of the occurrence of such grave errors rests with those engaged in clinical teaching. Let them be careful to point out that one line of treatment cannot be suitable to all classes of cases, and that great care must be exercised in the selection of suitable ones in which to carry out special treatment, whether that be by the exhibition of medicine or by local applications. That this care is exercised by the majority of obstetric teachers is, I know, the fact, and I look forward to a time when the lamentable errors in the treatment of uterine affections, which hitherto have been of but too frequent occurrence, will become rare.

In concluding the brief observations I have addressed to you to-night, I wish to point out that I have not attempted to lay down any definite rules for the treatment of the affections I have alluded to. My desire has been to draw attention to what I believe to be an important step in our knowledge of the physiology of menstruation, and the bearing which, to my mind, this may have on the treatment of uterine affections.

---

## THE BRUSSELS CONGRESS.

### SECTION OF GYNECOLOGY AND OBSTETRICS.

#### *Maternity Hospitals.*

M. HUBERT, jun., Professor of Obstetrics at the University of Louvain, presented his report on the above subject. The reporter assumes that these premises are above all dispute: "The delivery of women assisted at their own homes is favourable, the mortality in lying-in hospitals is frightful."

By statistics drawn from all the maternities in Europe, M. Hubert proves that the evil does not pervade one locality or attack one hospital alone, but that it is the same everywhere.

Before stating his conclusions, he examines puerperal fever from a pathological point of view.

In examining successively the various opinions which have been put forth as the cause of this murderous malady, he is inclined to attribute it to atmospheric action infecting the whole economy before producing morbid lesions.

As regards its transmissibility and inoculability by the finger, forceps, sound, or sponge, and as to its propagation through the medium of contaminated objects, M. Hubert reserves the consideration of these points pending scientific demonstration.

Therapeutic means are powerless to combat the evil, prophylactic

means must therefore be resorted to, to save humanity from this terrible scourge. He advises:—

1st. Any person having the care of a woman stricken with puerperal fever, or having assisted at a post-mortem, to take every possible precaution before going to the bedside of a lying-in woman.

2nd. Medical men to wash with chlorinated water or carbolized water after performing a post-mortem.

3rd. To disinfect articles of dress, and even to burn them.

4th. To carbolize instruments, forceps, sounds, sponges, everything which could have touched the sick person, or which had been used during the labour.

M. Hubert does not suggest the suppression of the large maternities; but he, in conclusion, points out—

1st. The advantages of small over large maternities.

2nd. The necessity for spare houses to allow of the evacuation and complete disinfection of the wards used by the sick during an epidemic.

3rd. That these spare houses should be adjacent to the maternity hospitals, surrounded by gardens.

M. LEFORT, of Paris, begged leave to address the section in a paper "On the Measures adopted in this City to lessen the Mortality among the Lying-in Poor." The speaker did not wish to dwell on the nature of puerperal fever, or on its characteristic lesions. It is unknown at present, and medicine is useless against it. The observation of facts teaches us how it is propagated.

M. Lefort is a contagionist, and admits the epidemic character of the malady—*i.e.*, he believes that every disease capable of transport from one place to another in the form of an epidemic is contagious. Hence scattering the lying-in cases gives them chances against contagion, and shows the difference between mortality among women delivered in the city and in the hospitals.

From statistics based on 1,843,093 labours, M. Lefort showed that in the city in 934,781 labours the mortality had only been 1 in 212, while in 888,912 women delivered in the maternity hospitals 1 in 29 died.

Contrary to the author of the report to the section, M. Lefort advises the restraining and possibly the suppression of lying-in hospitals.

The speaker refuted the objections made to the suppression of maternities, and, first, the obligation to give shelter to houseless women and those abandoned by their seducers. The speaker said these might have an asylum where they would not have to give their life as the price of the hospitality given them. This asylum is the house of the midwife.

With this idea in view, M. Lefort, in 1865, when the mortality at the "Maternity" in Paris was excessive, proposed to the authorities to try as an experiment sending free of cost the lying-in women coming to the hospital to the houses of the midwives, there to be delivered.

The measure was tried provisionally in 1865; in 1867, backed by success, it was adopted definitely and officially; and in 1869 became permanent.

The midwives who wish to receive cases have to guarantee their conduct in the matter to the authorities. The salubrity of the dwelling, the resources in bedding, linen, &c. must be assured. If a patient dies, the midwife receives no lodgers for three months.

As regards the second objection to the suppression of maternity hospitals, "How would obstetrics be taught?"—if it be necessary to teach future medical men and midwives the science of midwifery, it is imperative that the lives of women should not be sacrificed to the necessities of instruction.

As to the objection, want of money, which is apparently the most serious, because based on a definite possibility, M. Lefort replies by figures. From statistics gathered by him, and from official documents, he shows the objection has hardly any weight. The cost of delivery at the houses of the midwives does not exceed that of delivery in the hospitals. M. Lefort, with the sincerity of conviction, comes to the following conclusions:—

1st. "Maternities" and special centres for accouchements are condemned by experience.

2nd. They can be replaced by home delivery; and for the homeless is offered the midwives' houses.

3rd. As regards education, they may be replaced by the "poly-clinical institution."

After discussion, the section adopted the following conclusions:—

1st. A medical reform of the means for the delivery of women is urgent.

2nd. The total giving up of large maternity hospitals.

3rd. Replacing the large hospitals, with the midwifery school, by small lying-in houses with isolated rooms.

4th. The establishment of a spare house placed close to the lying-in house, but with separate administration.

5th. Extending as far as possible the home assistance, by aid and succour of every description.

M. Lefort also added the following modified conclusion:—

Delivery at the midwives' houses free of cost, and under medical surveillance, should diminish the number of deliveries in the hospitals, and lessen the mortality. This measure, desirable at any time, becomes a pressing necessity during an epidemic.

M. Lefort urged the following points in support of his resolution:—

The five conclusions voted by the general assembly show the necessity for substituting small for large hospitals, to lessen as much as possible the number of deliveries in the large maternity hospitals. But it is not alone sufficient to point out to the administrative authority the hygienic principles to be applied; it is necessary at the same time to show that they are easy of application.

Medical men are too often accused of framing rules which are too



absolute, and which are not practical ; this cause of failure must be borne in mind.

Delivery at the midwives' houses enables us to apply the principle at once, while we can wait for a time before building those small lying-in hospitals suggested by the general assembly. The measure cannot be called good in principle but incapable of application, because it has been tried for two years in Paris, and 5020 women were delivered in this manner.

There would appear to be *à priori* objections to this system, which, however, may be met by the experience of facts.

Sometimes from worry, if not at times from misery and other more personal influences, the mortality of the midwives often leaves much to be desired ; their home does not always receive lying-in women only, legal records showing that abortion is occasionally practised as well as the delivery of women. Now the establishing a system for delivering the poor in the midwives' houses, under the eye and at the cost of the authorities, has the advantage of allowing them to interfere in the affairs of the particular houses in question, and to furnish midwives with increased resources.

It need hardly be said that all midwives do not indifferently receive the lying-in poor. The authorities (the administration) before granting permission, inquire into the mortality, visit their houses, assure themselves that all the necessary hygienic surroundings are present, as well as sufficient linen and furniture.

The institution proposed in the added resolution, far from showing inconvenience on this head, has numerous advantages, proved by experience.

Another objection was raised :—

If puerperal fever broke out in a patient thus confined, the midwife's house would infect like a maternity hospital. This is true, but as there would only be two or three patients at one time in the midwife's house, so the danger of infection would be as limited as possible.

When it happens that a patient should have been taken to the hospital, which is very seldom, or when a death occurs, which is rarer still, the midwife does not receive cases for a month at least, and new patients only after it has been certified that the house is disinfected, the linen renewed, and every precaution taken against infection.

The institution in effect offers security not only to the poor, but also to the practice of the midwife.

But, further, it is objected that in difficult cases the midwife is not sufficient, and particularly so in the event of disease. On this point, again, experience has spoken.

Since the 1st April, 1873, this is met by the medical officers of the Aid Society. In a case of insurmountable difficulty the midwife calls in the medical officer, and if the difficulty seems beyond them, the woman may be taken to a special hospital. In the same way

are relieved cases of illness after delivery, and it is just this medical necessity which increases slightly the cost involved in each delivery.

The results of this system are most remarkable. It applies, notably, only to those women who could not be delivered in their own homes, and who consequently would be delivered in the maternity or other hospitals. Now in the period embraced by the new regular service last year, for example, while the mortality among those women delivered at the midwives' houses was 1 in 312 (2189 deliveries, 7 deaths), in the general hospitals it was 1 in 24 (3538 deliveries, 142 deaths), and in the special lying-in hospitals 1 in 42 (1265 deliveries, 30 deaths), at the "Maternity," and 1 in 12 (560 deliveries, 45 deaths) at the "Clinique d'Accouchements."

These facts are more eloquent than words, and aggravation of mortality in the lying-in hospitals by the transport of women already ill, or because the case is difficult, does not account for the difference in mortality of one woman in 42 and in 12 on one part, and of one in 312 on the other.

It is said that the mortality of women "home delivered" is diminished by the removal to the hospitals of women who become ill subsequent to delivery. An official document enables this argument to be properly appreciated. In the report of the Paris hospital administration, to be published shortly—a report which is already printed and in the possession of M. Lefort and the section—during the years 1872, 1873, 1874, 33,304 deliveries were effected outside the walls of the hospitals, under the care of the administration, with a mortality of 1 in 186, be it at their home or at the hospital, whither they had been taken. Now of 33,304 women, 88 only were taken to the hospital, and only 9 died. So much for the worth of an argument frequently put forth.

With regard to the question of money, so important practically, it is found in Paris to be about 20 francs at least more for each delivery at the midwife's house. If, then, 66,081 women delivered at the hospital in 1874 had been delivered at the midwives', the hospital administration would have expended 532,140 fr. (21,284*l.*), a small figure in a budget of 25 millions. But instead of 227 deaths, there would only have been about 19. 192,146 fr. would have saved 208 women; and if for 635 fr. the life of a mother can be spared, this purchase becomes a duty to be performed by the administration.

The report presented by the section to the general assembly gave rise to a fresh discussion, ending in the definite adoption of the five conclusions, together with M. Lefort's amendment.—*Ann. de Gynékol.*

## Obstetric Summary.

### *On a Case of Cesarian Section.*

Dr. Bardy-Delisle (in *Ann. de Gynékol.*, November, 1875) narrates an instance where a living child was extracted—the patient recovering,

the child dying five days after from pneumonia. The patient, a dwarf, aged nineteen, had a pelvis, the antero-posterior diameter measuring only 5 centimetres (under 2 inches), the sacrum being convex in place of concave. Labour had already set in. Chloroform was administered; an incision was made in the usual way; smart hemorrhage occurred, but ceased after the extraction of the child and placenta, the uterus contracting immediately. The peritoneal cavity was sponged out, and the wound closed by deep sutures. Peritonitis ensued, but the patient convalesced perfectly. The child, a male, well-formed and developed, progressed favourably the first three days, but succumbed to an attack of pneumonia on the fifth day, probably induced by exposure at an open window.

---

*Astringent Injections in Post-partum Hemorrhage.*

Bezold, in his "Dissertatio de hæmorrhagia uteri partum insequente," published in 1780, writes as follows:—"Sed omnibus his incassum adhibitis sunt quidam, qui usum et applicationem pessorem illorum stypticorum maxime extollunt; inter quos præcipue Fr. Hoffmann et le Roux eminent. Non ego sum, qui clarissimorum virorum experientiæ contradicam, nec minus sentiam, aquam aluminosam vi adstringente maxima gaudere; sed liceat etiam tironi quædam non inutilia, et forte in praxi salutaria hic annotare. Lintea carpta aqua aluminosa, vel alio liquido adstringente imbuta, vaginæque intrusa, illam certe constringent: fieri etiam poterit, ut per consensum uterus contrahatur; sed quoque eveniet, ut, vagina his linteis repleta, sanguis in utero iam extrausatus, et ille qui identidem per vasa erumpit, in utero retineantur, collo uteri a remedii immediato contactu arctius constricto, moraque in grumos abeat multoties immensi voluminis, quo fiet, ut uterus in se coire impar, aperta vasorum oscula operire nequeat: insuper cruor in utero hospitans in putredinem necessario abeundo, totam humorum massam inficere: et in exhausto corpore febrem putridam generare poterit. Quid auxilii afferret hæc methodus in hæmorrhagia interna absque sanguinis per vaginam effluvio? quare per consensum contractio uteri tentanda est, cum per immediatum contractum eadem celebrari poterit, injectione in uteri cavum eisdem aquæ aluminosæ aut liquidi cuiuscunque adstringentis? Hac methodo celerior obtinebitur effectus, nihilque impediet; quo minus irritatus uterus in sese coire possit, imo sese contrahendo sanguinis grumos non tantum eius contractioni resistentes, verum etiam vasorum aperturam sustentantes successive foras expellat. Attamen, antequam in uteri cavum fiat injectio, obstetricans manu, aqua frigida, cui sexta pars spiritus vini affundi poterit, aut solutione supradicta, madida, iteratim grumos sanguinis ex utero educat, eamque circumvolvendo hoc viscus ad contractionem sollicitet. Hac methodo, citius, jucundius et tutius curabitur."

*On the Advantage of Milk Diet in Albuminuria of Pregnant Women, and its indication as Preventive Treatment in Eclampsia.*

Dr. Tarnier (in *Le Progrès Médical*, No. 50, Dec. 11, 1875) recounts two instances where milk diet succeeded perfectly in relieving the edema and albuminuria in two pregnant women, no other treatment being resorted to. The first day one litre of milk and two portions of food were given; the second day two litres and one portion; the third, three litres and half a portion; the fourth and following days, four litres or as much as desired, without any other food.

In one to two weeks after the commencement of this treatment, the albuminuria notably decreased, and at length ceased entirely; the patients being delivered of healthy living children, and convalescing rapidly and perfectly.

---

*Cesarian Section in Cases of Fibroid Tumours.*

Dr. H. Cazin (in *Arch. de Tocologie*, Dec. 1875) narrates an instance of a fibroid tumour blocking up the pelvic cavity, impeding delivery, where Cesarian section was successfully performed. Reference is given to twenty-one other cases, collected from various sources. The deplorable mortality to the mothers—only three surviving—is commented on, and contrasted with the results of the same operation in ordinary obstetric practice, where a relatively healthy uterus is incised on account of pelvic deformity.

He shows that when the fibroid tumours are adherent by a large base, the portion of the uterus which is dilated for the lodgment of the fetus, weakened by excessive distension and a proportionate thinning, cannot contract after the operation, and so encourages hemorrhage, old adhesions to the neighbouring organs contributing also to keep up the inertia. In certain cases the hemorrhage has been such as to necessitate compression of the aorta, sutures, &c., and may be directly credited as the cause of death in many cases, as well as indirectly of nervous accidents and collapse.

---

*Postural Treatment of Shoulder Presentations.*

Dr. P. R. Maxon, of Syracuse, New York, thus describes his plan of treating shoulder presentations:—"Gravity is the principle invoked; and I was led to the discovery in 1860 by placing a woman with prolapsed cord on her knees, with her head and shoulders low, as recommended by Dr. T. G. Thomas, of New York, in order to effect its reduction, and finding that while she was in this position an abnormal (abdominal) presentation was spontaneously converted into a normal one. Having reflected on this circumstance, I was induced a few weeks later, when called in consultation in a bad shoulder presentation, to try position as a means of rectifying it. I was very



anxious in regard to the case, because the lady had lost three children already from 'turning to deliver' in shoulder presentations. Her regular attendant, Dr. G. N. Dox, of Geneva, New York, a physician of attainment and experience, happened to be the confrère in whose practice the case of prolapsed cord, above referred to had occurred; and instead of 'turning' himself, as had been so unsuccessfully attempted in the lady's previous labours, he sent for me in consultation. Remembering the fate of the other children, and finding this one very large, I suggested the feasibility of correcting this shoulder presentation in the same manner as I had corrected the abdominal in the first instance. With his consent I made the effort in the following manner:—I folded several quilts compactly, laying them upon one another to the height of about one foot, and assisted her to kneel upon the quilts, with her head and shoulders resting upon the bed, and her face forwards, so as to bring her body to an angle with the bed of nearly 90 degrees. I then pressed my hand gently against the shoulder, which readily receded, until I was enabled to clasp the vertex with my fingers, and with the assistance of the next pain to so 'engage' it that, when the patient was placed upon her left side and the quilts removed, a perfectly natural presentation presented itself. In a few hours the labour terminated in the delivery of a healthy boy, weighing ten pounds. Only a few moments were occupied in the process, and subsequent experience convinces me that shoulder presentations can generally be converted in this way into natural ones, without a resort to 'turning,' and with no risk for the mother or the child."—*Lancet*.

### Gynecic Summary.

#### *Observations on Chorea cured by Chloral administered per Rectum.*

Messrs. Goeltz and Auger (in *Gaz. Hebdomadaire*, Dec. 10th, 1875) communicate the particulars of a case of chorea uninfluenced by the most varied treatment, but which was cured in a fortnight by injections of chloral in drachm doses night and morning.

The patient, a young girl, aged twenty-two. Catamenia regular since the age of fifteen. Suddenly exhibited choreic symptoms on November 1st, 1874, no assignable cause being apparent. Various methods of treatment were adopted—bromide of potassium, tartar emetic, sulphur baths, quinine wine, and bromide of camphor.

On Jan. 1st, 1875, opium was commenced, but produced no good effect. Chloral in thirty-grain doses twice daily was then tried; then arseniate of soda, syrup of strychnine; ether spray along the spinal column every second day, and injections of éserine; but the symptoms continued unabated.

On March 19th she was admitted at the Lariboisière. Cold douches and ten cupping glasses on the back of the neck were tried; then valerianate of atropine, which seemed to afford some relief for a time.

The tartrate of iron and warm baths prolonged for two hours for twelve days gave no relief.

Treatment was discontinued for a time, and the patient sent into the country; the choreic movements continued.

On May 18th hypodermic injection of chloral, and morphia and chloral in thirty-grain doses night and morning, were tried—the morphia being gradually increased.

The symptoms notably decreased, but convulsive movements of the muscles of the larynx and diaphragm came on. The patient improved and relapsed again. Increasing doses of morphia were given, until the patient slept continuously.

On July 31st injection per anum of forty-five-grain doses of chloral was commenced and continued twice daily, the dose being increased to one drachm each time.

On August 3rd the movements had greatly diminished. The treatment was continued until the 15th, when the chorea completely disappeared. On the 28th the cure was complete. Sulphur baths were given. On December 1st the patient continued quite well.

---

## *Pediatric Summary.*

### *Artificial Diet of Infants.*

Dr. B. F. Dawson says:—"If artificial diet be judiciously selected, there is no reason why a child should not thrive as well upon this kind of nutriment as when its support is derived from the mother's breast. No food is capable of properly supporting an infant unless it possesses heat and fat-producing properties. Any nourishment which does not come up to the requirements of a liquid food having the proper quality will produce intestinal troubles. What nourishment are we to give? Is there any one kind of nourishment which uniformly and perfectly supplies the place of breast-milk? The article which most perfectly answers all the requirements, and can be trusted to furnish all the elements of nutrition in the most digestible form, is milk from some animal. The milk of various animals varies to a certain extent in the properties of its different constituents; but that from the cow is the one which should ordinarily be used, and when properly prepared may answer all purposes. In its natural state it is not a fit article of food, and some article must be added which will effect a proper dilution. Water is the article commonly added; but by far the greater number of cases suffer in consequence of its addition. The addition of water alone does not improve the digestibility of the casein, for it does not dilute it; and when milk is introduced into the stomach, diluted with water, the water is soon taken up and the casein is left as undiluted and unchanged as before the food was given. Nor does the addition of sugar make the coagula any more easy of digestion; nor does skimming the milk act beneficially, but, on the contrary, deprives it of one of its most

important constituents. Inasmuch as the mother's milk contains proportionately more fat than other milk, may it not be true that the finer coagula which is formed, when the mother's milk is introduced into the stomach, is due to the presence of this fat? and would it not be better to use the milk as an article of food, from which casein has been removed, rather than use the milk which has been deprived of its cream? It had been his experience that this was the more proper course to pursue. The admixture of farinaceous articles with the milk also leads to disastrous consequences. There is one article, however, which contains such small quantities of starch that it can be used with very great success for effecting a proper dilution of cow's milk, and that is barley-water. Good cow's milk diluted from one-third to one-half with barley-water forms one of the best articles of food that can be used for infants when it is necessary to bring them up artificially. If barley cannot be obtained, oatmeal may be substituted, and answers nearly as good purpose. This article produces a real dilution of casein, and renders the coagula much finer and more nearly like the coagula which is found in milk from the mother's breast.—*Sanitary Record*.

---

### Obituaries.

WE regret to have to record the death of Dr. Eduard Martin, Director of the Institute for Clinical Midwifery and Gynecology at the University of Berlin, and Honorary Fellow of the Obstetrical Society of London. This Society has, we are sorry to add, lost during the past month two distinguished Fellows, Dr. Charles E. Squarey, and Dr. Lorenzo E. Desmond, of Liverpool.

---

### BOOKS, PAMPHLETS, AND PAPERS RECEIVED.

"The Student's Guide to the Practice of Midwifery." By D. Lloyd Roberts, M.D. London. J. and A. Churchill. 1876. 8vo. Pp. 299.

"The Cause of the Commencement of Parturition." By Charles M. Crombie, M.B. London. J. and A. Churchill. 1875. Pp. 38.

Communications have been received from Dr. W. Playfair, Dr. Meadows, Dr. Roper, Mr. Cullingworth, Dr. Calderwood, Dr. Ashburton Thompson, Dr. Draper, Dr. J. Patterson Cassells, Dr. Clapper-ton, Dr. Day, and Dr. Edis.

---

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.

# THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

---

No. XXXV.—FEBRUARY, 1876.

---

Original Communications.

---

## ON THE WEIGHT OF NEW-BORN CHILDREN.

By E. INGERSLEV,

Clinical Assistant in the Lying-in Institution in Copenhagen.\*

THE weight of new-born children has already for a long time been the subject of research in places where it can be specially carried out—namely, in lying-in and nursing institutions; partly with the view of determining, through the greater number of cases, the usual weight of children born at full term, and more particularly of ascertaining the conditions which have an influence on it, such as the sex of the child, the age of the mother, the number of pregnancies, &c.; partly also with the object of controlling the physiological weight and state of health of the child by repeated weighings after birth.

The statements of various authors respecting the average weight of the new-born child vary somewhat. The cause of this must be sought partly in the differences of race, partly in the very varying amount of material.

Scanzoni, from 9000 cases, fixes the average weight at 6 pounds (Danish) and 28 lod (=3437·5 grammest†);

---

\* From the *Nordiskt Mediciniskt Arkiv*, Band vii. 1875.

† The gramme (15·434 grains) is the unit of weight used by Dr. Ingerslev, and is retained in the translation.



Hecker at 3275 grammes; E. von Siebold at 3250 to 3500 grammes—the smaller number representing the weight of girls, the greater that of boys. According to the observations made on 3450 mature children in the lying-in institution in Copenhagen, the average weight is 3333·5 grammes.

The proportion between boys and girls is as follows: 1833 boys had an average weight of 3380·9 grammes, and 1617 girls of 3279·7 grammes; this gives a little over 100 grammes in favour of the former, a somewhat greater excess than Hecker found (80 grammes in a total number of 1096 children).

As a consequence of the accurate weighings undertaken in lying-in institutions, the enormous weights (15 to 20 lbs.) which at an earlier period were not unfrequently recorded in literature, have become very rare. Thus, among 3000 children, Siebold found only two weighing  $10\frac{1}{4}$  and 11 pounds,\* and says that  $11\frac{3}{4}$  pounds was the greatest weight he ever found in a new-born child. Lachapelle, among 7000 children, found 13 weighing 10 pounds; none of greater weight. Hecker twice found a weight between 10 and 11 pounds, and he regards a weight between 9 and 10 pounds as rare, as he only found four instances of it. With this observation my own agrees: among the 3450 children, 19 (13 boys and 6 girls) weighed between 9 and 10 pounds; one boy only weighed  $10\frac{3}{8}$  pounds—the greatest weight found in the whole number.

Among the conditions on the mother's side which may be assumed to have more or less influence on the nutrition and size of the child, some are of such a nature that their investigation will scarcely give a satisfactory result, such as the state of the mother's constitution and nutrition during pregnancy, and the shorter or longer duration of gestation, to which Frankenhäuser (*Monatsschrift für Geburtsk.*, vol. xiii.) attaches a certain degree of importance, when he attributes one of the grounds of the less development of

---

\* The Danish pound is somewhat greater than the English.

children in *primiparæ* to the supposed shorter duration of pregnancy. The same author also finds a certain relation between the size of the mother and the weight of the child, inasmuch as, according to his researches, the weight increases in proportion to the mother's height. The mother's age and the number of pregnancies have also been made the subject of investigations, of which I shall speak more fully. While the earlier researches indicated that children of the first pregnancy were heavier than those of the later pregnancies, and this was believed to be one of the causes of the longer duration of gestation in *primiparæ*, the investigations of Frankenhäuser and Hecker have shown that this opinion does not hold good, but that, on the contrary, the reverse is the fact: they found an excess of about 140 grammes in the children of *pluriparæ*. In the investigation made here, 1723 children of *primiparæ* were found to have an average weight of 3254 grammes, while in 1727 children of *multi-paræ*, the average weight was 3412 grammes, giving an excess of 158 grammes in favour of the latter. This increase of weight has been regarded by Matthews Duncan ("Fertility, Fecundity, &c.") as being in relation not to the increasing number of pregnancies, but to the age of the mother; so that children of the second, third, &c. pregnancies have a greater average weight than those of the first, not because the number of pregnancies is greater, but because the mother's age is more advanced. He lays down propositions to the following effect:—1. The weight of the children of *primiparæ* is not nearly uniform, but varies according to the law of the age of the mother. 2. The weight of the children of all mothers, whether *primiparæ* or *multi-paræ*, varies according to the same law of the mothers' age. 3. The average weights of children of the first and subsequent pregnancies do not show any regular increase or decrease corresponding with the number of pregnancies. Finally, he alleges that he has found from his investigations that this progression in weight according to the mother's age reaches its highest point between the twenty-fifth and twenty-ninth years, after which there is a slow fall. Hecker (*Monatsschrift für Geburstk.*, vol. xxvi.) has again investigated the question, using as his

material 4000 children, a number about twice as great as that on which Duncan founded the above-mentioned conclusions. He has found as the result that Duncan's statement that the weight of the child stands in a relation of direct dependence on the age of the mother is correct ; but, on the other hand, he has found no fixed limit to the progressive increase of weight, but believes that this extends to the end of the procreative period, and that age is not the only factor which determines the increase, but that the number of pregnancies has a recognisable influence. From an examination of more than 6000 children in the lying-in institution in Munich, Wernich has found Hecker's opinion corroborated, that the increasing number of pregnancies has just as evident an influence as the mother's age in the increase of weight ; but that the rates of progression in this increase, both that due to the increasing number of pregnancies without reference to age, and the reverse, are not quite regular, but show interruptions in the ascent. The investigation of the influence of these conditions on the weight of the new-born child will be aided by the following tables of 3450 children born in the lying-in hospital here ; they are arranged in quinquennial classes, from the fifteenth to the forty-fourth years of the mother's age, as in Hecker's and Duncan's researches.

The first five tables show the relation between the weight of the child and the age of the mother.

TABLE I.—FIRST PREGNANCY.

Age of Mother.	BOYS.		GIRLS.		TOTAL.	
	Number.	Average Weight.	Number.	Average Weight.	Number.	Average Weight.
15 to 19	112	3275·2	92	3190·7	204	3232·2
20 to 24	428	3317·7	413	3210·7	841	3267·5
25 to 29	262	3278·2	237	3204·7	499	3243·3
30 to 34	67	3352·5	57	3138·7	124	3254·2
35 to 39	28	3222·9	20	3320·5	48	3263·6
40 to 44	1	2870	6	2976	7	2960
	898	3300	825	3205·2	1723	3254·6

TABLE II.—SECOND PREGNANCY.

Age of Mother.	BOYS.		GIRLS.		TOTAL.	
	Number.	Average Weight.	Number.	Average Weight.	Number.	Average Weight.
15 to 19	3	3356·6	7	3312·8	10	3326
20 to 24	163	3416·4	114	3361·2	277	3357·6
25 to 29	255	3433·6	194	3364·7	449	3403·8
30 to 34	98	3400·7	96	3361·4	194	3381·3
35 to 39	20	3431·5	27	3300·7	47	3356·4
40 to 44	4	3200	5	3236	9	3220
	543	3422·1	443	3359·2	986	3391·8

TABLE III.—THIRD PREGNANCY.

Age of Mother.	BOYS.		GIRLS.		TOTAL.	
	Number.	Average Weight.	Number.	Average Weight.	Number.	Average Weight.
15 to 19	—	—	—	—	—	—
20 to 24	18	3302·2	11	3291·8	29	3298·8
25 to 29	87	3504·6	71	3295	158	3410·4
30 to 34	59	3487·6	65	3331·8	124	3406
35 to 39	17	3619·4	22	3328·6	39	3455·4
40 to 44	7	3285·7	8	3312·5	15	3300
	188	3482·2	177	3313·3	365	3400·3

TABLE IV.—FOURTH PREGNANCY.

Age of Mother.	BOYS.		GIRLS.		TOTAL.	
	Number.	Average Weight.	Number.	Average Weight.	Number.	Average Weight.
15 to 19	—	—	—	—	—	—
20 to 24	3	3603·3	1	3370	4	3545
25 to 29	22	3544·5	20	3348·5	42	3451·2
30 to 34	33	3415·7	28	3391·8	61	3404·7
35 to 39	18	3561·7	14	3253·6	32	3427·7
40 to 44	3	3610	3	3093·3	6	3351·7
	78	3499·3	66	3368·9	145	3424·8



TABLE V.—FIFTH AND SUBSEQUENT PREGNANCIES.

Age of Mother.	BOYS.		GIRLS.		TOTAL.	
	Number.	Average Weight.	Number.	Average Weight.	Number.	Average Weight.
15 to 19	—	—	—	—	—	—
20 to 24	—	—	—	—	—	—
25 to 29	11	3681·8	16	3520·6	27	3586·3
30 to 34	44	3460·7	34	3546·8	78	3498·2
35 to 39	43	3643·9	35	3457·7	78	3560·3
40 to 44	27	3561·1	21	3202·9	48	3404·4
	125	3564·9	106	3445·3	231	3500

In these tables of separate pregnancies, the excess in weight of boys over girls is apparent in nearly all the periods. On the other hand, the influence of age on the increase of weight is not clearly expressed : in the last three tables the numbers are small ; but even in the first two tables there is no regular progression, though the influence of age cannot be denied when we compare the weights at the earliest age with those at higher ages taken together. If all the pregnancies be classed together according to age, as in Table VI., a regular progression, according to the mother's age,

TABLE VI.—SUMMARY OF PREGNANCIES ACCORDING TO AGE OF MOTHER.

Age of Mother.	BOYS.		GIRLS.		TOTAL.	
	Number.	Average Weight.	Number.	Average Weight.	Number.	Average Weight.
15 to 19	115	3277·4	99	3199·4	214	3241·2
20 to 24	612	3344·9	539	3248·2	1151	3299·6
25 to 29	637	3387·5	538	3289·1	1175	3342·4
30 to 34	301	3417·4	280	3334·8	581	3375·9
35 to 39	126	3501·7	118	3350	244	3428·4
40 to 44	42	3467·7	43	3187·8	85	3326·2
	1833	3380·9	1617	3279·7	3450	3333·5

comes out far more distinctly than in the case of the individual pregnancies ; for it affects both boys and girls separately as well as together up to the fortieth year, as Hecker's tables have shown ; and there does not appear any fixed limit to the increase between the twenty-fifth and twenty-ninth years, such as Duncan has alleged to exist.

The following tables serve to show the influence on weight of the other factor, the number of the pregnancy. In Table VII. is given the average weight of different pregnancies without reference to the mother's age, but with the addition of the average age for each pregnancy.

TABLE VII.

*Average Weights of Children and Mean Ages of Mothers for each Pregnancy.*

	First Pregnancy.	Second Pregnancy.	Third Pregnancy.	Fourth Pregnancy.	Fifth and other Preg- nancies.
Average weight (boys)	3300'	3422'1	3482'2	3499'3	3564'9
Mean age of mother .	24'09	27	29'5	32'03	35'32
Average weight (girls)	3205	3359	3313'5	3868'8	3445'3
Mean age of mother .	24'03	27'6	30'36	31'88	35'1
Average weight (both)	3254'6	3391'8	3400'3	3424'8	3500
Mean age of mother .	24'06	27'36	29'93	31'97	35'21

There can be no doubt that there is here an increase of average weight in proportion to the number of pregnancies. There is, however, a break at the third pregnancy in the case of girls, a rise again being shown in the fourth and fifth. But, as the ages of the mother follow a similar progression, the chief importance might be attached to the age and the progression arranged accordingly. We may therefore inquire how the average weight in different pregnancies behaves with respect to similar ages of the mother, as in the following table.

TABLE VIII.

*Behaviour of the Average Weight in different Pregnancies with respect to similar Ages of the Mother.*

Age of Mother.	First Pregnancy.	Second Pregnancy.	Third Pregnancy.	Fourth Pregnancy.	Fifth and later Pregnancies.
15 to 19	3232·2	3326	—	—	—
20 to 24	3267·5	3357·6	3298·8	3545	—
25 to 29	3243·3	3403·8	3410·4	3451·2	3586·3
30 to 34	3254·2	3381·3	3406	3404·7	3498·2
35 to 39	3263·6	3356·4	3455·4	3548·4	3560·3
40 to 44	2960	3220	3300	3351·7	3404·4

Here the progressive increase according to the number of pregnancies shows itself not less distinctly : for in four of the quinquennial periods there is no interruption, and a considerable one is only found in the period between twenty and twenty-four years, in the third pregnancy. On reading the numbers horizontally, by which is obtained an expression in favour of Hecker's opinion that the number of pregnancies as well as the age has a marked influence on the weight, there is found to be a remarkably more uniform progression than when the numbers are read vertically ; and the high averages in the fifth and following pregnancies especially tell most in favour of Hecker's position.

Both factors have thus their influence, and often act in combination in individual cases, when the age at which the pregnancy occurs does not vary much from the calculated average. It may then be supposed, that this circumstance may have a favourable influence on the weight of the child, and I have endeavoured to show this in the two following tables, where the mean weight of children born in the average year of pregnancy is compared with the mean weight of children born before and after that year. Only the first three pregnancies are taken, for the small numbers in the fourth and fifth give a doubtful result.

TABLE IX.—FIRST PREGNANCY. (Mean Weight, 3254·6;  
Mean Age, 24.)

—	Before Mean Age.		In Mean Year of Age.		After Mean Age.	
	Number.	Weight.	Number.	Weight.	Number.	Weight.
Boys .	415	3301·7	85	3347	358	3286
Girls .	419	3207	86	3230	320	3196
Total.	874	3256·3	171	3288·7	678	3243·8

TABLE X.—SECOND PREGNANCY. (Mean Weight, 3391·8;  
Mean Age, 27.)

—	Before Mean Age.		In Mean Year of Age.		After Mean Age.	
	Number.	Weight.	Number.	Weight.	Number.	Weight.
Boys .	268	3413·2	45	3457·5	230	3421·1
Girls .	211	3378·1	30	3304·3	202	3342·2
Total.	479	3399·2	75	3396	432	3384·6

TABLE XI.—THIRD PREGNANCY. (Mean Weight, 3400·3;  
Mean Age, 29.)

—	Before Mean Age.		In Mean Year of Age.		After Mean Age.	
	Number.	Weight.	Number.	Weight.	Number.	Weight.
Boys .	86	3449·8	19	3561	83	3498·6
Girls .	82	3294·6	17	3359·4	78	3323
Total.	168	3374	36	3465·8	161	3413

From these tables it appears very distinctly that children born in the average year calculated for each pregnancy have the greatest weight—*e.g.*, in Tables IX. and XI. the



excess of weight is apparent both for boys and girls separately and for both together. In Table X. there is a break in the case of girls born before and at the average age, and a slight corresponding difference in the total; and hence Duncan's view, that the weight approaches its maximum between the twenty-eighth and twenty-ninth years, scarcely holds good. Thus, *cæteris paribus*, a primipara should bring forth her child in the most fully developed condition in her twenty-fourth year; one in labour for the second time in her twenty-seventh; and one for the third time in her twenty-ninth or thirtieth year.

It is not, indeed, statistics alone that show that the size of the child is dependent on the above-mentioned conditions: the course of parturition speaks also in favour of it, and gives evidence that this relation is of importance in labour. That male births are of longer duration than female, and more often require operative interference, is an established fact, which finds its explanation in the more vigorous development, in the greater weight, and naturally in the greater circumference of the head, and the greater firmness of the cranium. That a more considerable size of the child is not without influence on labour is evident; and as a decided judgment of its size from exploration and investigation of the abdomen may be deceptive, every circumstance which may in any way have an influence is of importance. A misproportion between the head and pelvis, with considerable development of the fetus, does not, indeed, often come under notice; for, as the size of the mother is generally in proportion to that of the pelvis, a greater development of the fetus in a large mother may give rise to mechanical misproportion when the pelvis is narrow, and in this case especially the increase of weight with the increasing number of pregnancies shows its influence; for here, as Michaëlis especially has shown, the later labours are more and more difficult, produce a greater mortality among the children, and render more frequent and greater operative interference necessary. While one of the causes of the greater danger or difficulty of parturition in *primiparæ* was formerly sought in the assumption of a greater size of the fetus, this explanation may fail, and the difficulties manifested in the more protracted labour may,

the proportions of the pelvis being normal, be attributed to the reduced distensibility of the soft parts concerned in parturition ; but if the pelvis itself be only slightly narrowed, the difficulties will in a great number of cases increase with succeeding pregnancies, and the increase of weight of the child following with the increased number of pregnancies has here great influence.

(*To be continued.*)

## FOLDING SHORT FORCEPS.

By W. DRAPER, M.R.C.S., L.M., &c.

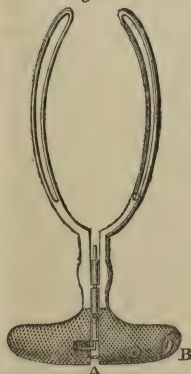
Surgeon to the York Dispensary ; late Resident Obstetric Officer to the Middlesex Hospital.

THE forceps about to be described take the name of *folding* forceps, in consequence of the two blades of the instrument being so constructed as to fold one within the other in such a manner that both are introduced at the same time. In the accompanying illustration, Fig. 1 shows the forceps closed for introduction, and Fig. 2 exhibits them open.

*Fig. 1.*



*Fig. 2.*



To the invention of the general principle of the forceps (that of the simultaneous introduction of the blades one within the other), I do not for one moment lay the slightest claim, for I believe Mr. Vacher was the originator of the idea. My forceps are a modification of Mr. Vacher's, and,

after a fair trial of both, I venture to say that my instrument is a considerable improvement upon the original design. A short description of both forceps will best show the relative merits of each.

In Mr. Vacher's forceps the handle is small and spindle-shaped, affording but a slight and uneasy hold for the hand ; in the middle of the handle there is a projecting knob, the proximal end of a hinge bolt : this knob forms a most inconvenient obstacle to the hand when the handle is grasped. Then, again, at one end of the handle there is an awkward lever, connected with a catch which locks the handles together. This lever projects considerably beyond the handle, and therefore forms another object of discomfort to the operator's hand ; moreover, the projection is very apt to catch the bedclothes, or the patient's dress, the result of which would be that the lock would be set at liberty, and the blades would lose their hold of the fetal head.

These matters may, at first sight, appear trivial, but as a matter of fact, practically, they are of no small importance both to the comfort of the operator and to the effectiveness of the instrument.

In describing these points I do not at all wish to detract from the value of Mr. Vacher's forceps, my only aim being to remedy defects, and to introduce a more perfect and convenient instrument.

As to my own forceps. In the first place, when open, the handle which is made of ebony, is of the form depicted in Fig. 2, and fits the palm of the hand and the fingers perfectly, and a good firm grasp of it may be obtained, and maintained, without discomfort. This handle is neither clumsy nor weighty. The proximal end of the hinge-bolt, which connects the neck of the forceps, is bent at right angles, and being let into one side of the handle, it is quite concealed, yet, if it be necessary to separate the blades during an operation, this separation can be quickly effected by a very simple movement. Then, again, my spring lever in connexion with the catch which locks the two parts of the handle together, is enclosed within a thumb depression at one extremity of the handle ; the lever, therefore,

and the parts upon which it operates, are quite out of the way, yet by placing the tip of the thumb in the depression, the spring readily acts.

The blades of my forceps are made as light as possible, consistent with a moderate degree of strength, and as the instrument is applicable only in cases in which no great amount of force is required, it is unnecessary for its blades to be either stout or heavy. The pair of forceps which I now have in use weigh only 8 oz. I consider, however, they might, with advantage, be made still lighter. My instrument, too, is electro-plated, the great advantage of which being, that the hinge-joint upon which the blades revolve, and the spring lever and catch, are not liable to become rusted by the secretions, &c., and their action thereby interrupted; and, further, the plating insures the general cleanliness of the instrument. The forceps are so small and portable, that they may be conveniently carried in the breast pocket.

A word or two as to the way in which the instrument is applied, and the conditions under which it is most useful, may not be out of place.

The forceps being *folded*, with the convexity of one blade lying within the concavity of the other (as Fig. 1), the *two* blades are simultaneously introduced exactly in the same way that one blade of the ordinary short forceps is introduced; this having been done, one-half of the cross handle is held *in situ*, while the other half is gently separated from it, the result of which is that the small hinge-joint, connecting the shanks of the blades together, is acted upon, one blade glides from under the other, over the fetal head, and, with a little management, this revolving blade is easily got into position opposite to the fixed blade: the two halves of the handle now come into apposition, when, by means of the spring catch, they are firmly locked together, and form the cross handle shown in Fig. 2.

If, during an operation, it be desired to take off the forceps, it is necessary simply to withdraw the hinge-bolt, when the two blades are at once separated and set at liberty.

A difficulty may probably be apprehended by some in



getting the instrument to act properly in a contracted tense vagina, in a narrow pelvis, or in certain positions of the fetal head, but the room taken up by the blades is so small, and so flexible are they, that no such difficulty *really* presents itself.

The forceps are *applicable* in all that numerous class of cases in which slight assistance is indicated to terminate tedious, but otherwise natural, labours—in fact, they may be employed in all cases in which ordinary short forceps are applicable. They are, I need hardly say, available only after the head has passed through the pelvic brim.

There is one more point worthy of note—viz., that these forceps may be used, if it be deemed desirable, under the bedclothes, without the knowledge of the patient or her attendants; and it is, I hold, often a matter of no small importance, that a sensitive and delicate patient should be saved the shock and natural dread occasioned even by the most gentle mention of the use of *instruments*.

Finally, I am satisfied, that if these forceps be carefully and judiciously employed, they are capable of being a great and efficient aid both to the patient and to the obstetrician.

## ON LACERATION OF THE GRAVID UTERUS.

By J. ASHBURTON THOMPSON,

Fellow of the Obstetrical Society of London, and Surgeon-Accoucheur  
to the Royal Maternity Charity.

(Continued from p. 645.)

*Of Hemorrhage, Internal and External.*—Hemorrhage in these cases may be either external or internal. The former is rare, as a result of rupture. It was observed in only one of this series of cases (Case 4), and in that it began before labour set in, and gradually ceased as delivery progressed. It is probable, therefore, that in this case the hemorrhage had nothing to do with the rupture, and was, perhaps, of the kind known as accidental. In one or two other cases it is observed that a little water tinged with blood, or a little bloody mucus, was pouring from the vagina at the time of examination; but in no case but that named was anything

which could be called "hemorrhage" observed. With regard to the present subject, external hemorrhage, since it is common enough in labours uncomplicated by rupture, is a symptom of little value. Internal hemorrhage, greater or less in quantity, is more common ; but as a diagnostic is, in the very great majority of cases, of no practical utility. Dr. Jolly has shown, however, that in one very interesting case it served to establish a diagnosis of rupture in the absence of all other symptoms. The effusion was in such quantity as to cause a tumour in the abdomen, which was at first supposed to be the distended bladder ; but that viscus being found empty, Dr. Jolly surmised that a rupture of the uterus had taken place, and that the tumour consisted of blood effused into the peritoneum. This turned out to be the case ; and Dr. Jolly lays claim to the discovery of this aid to diagnosis in doubtful cases. As a rule, the effusion is so small as to be discovered only after death.

On the whole, then, notwithstanding the vascularity of the gravid uterus, more than slight hemorrhage, either external or internal, is not a common result of its laceration ; but it seems reasonable to expect that there will be more bleeding, or at all events more remarkable bleeding, in case the vagina is involved in the wound.

*Of the Action of the Uterus subsequent to Rupture.*—Perhaps no diagnostic of rupture is more generally looked for or more relied upon than the sudden cessation of the uterine contraction, which is almost universally said to be its immediate consequence. More value is certainly put upon this symptom than upon any other, except, perhaps, that of sudden collapse ; and no writer with whose works I am acquainted, with two exceptions, refers to variations of the rule laid down with regard to it, save in the same cursory manner in which attention is drawn to the possible occurrence of cases altogether exceptional. These two writers are Dr. Leishman and Dr. Jolly. The former, more explicit on this point than other authors of English text-books, says : " If it should so happen that the entire thickness of the uterine tissues has not been involved, the ordinary expulsive contractions may go on, though probably modified in de-

gree. Dr. Jolly, in a paper to which I shall have further occasion to refer, abundantly shows by a large number of cases that the character of the throes after rupture depends not at all upon the extent or kind of rupture.

Of the present series of twenty-three cases, four must be omitted ; these are the traumatic cases. In one of these indeed it is said that the throes increased in force after the laceration, but the report is not precise enough to be available. In Cases 1 and 7*a* no note is made on this point. The number remaining for analysis is therefore reduced to seventeen. Of these, in nine the throes ceased suddenly upon rupture—(Cases 3, 6, 6*a*, 7, 9, 11, 12, 16, 18). In eight they were modified in some other way, or unaltered. They ceased gradually in Cases 2 and 17 ; they continued normal, or labour was terminated by the natural efforts, in Cases 4, 8, and 14 ; labour-pains began after rupture in Cases 5 and 13 ; and in Case 21 the throes became spasmodic.

In a little more than half, or in about 52·9 per cent., the throes ceased in a typical manner ; in three cases they were unaltered ; in five they were modified in some other manner. So that, if the two last-named classes be added together, it appears that in so large a proportion as 47 per cent. no conclusion could be arrived at as to the cause of modification of the throes from the manner in which they were altered ; for they did not suddenly cease in a characteristic manner.

On this point Dr. Jolly's papers (published in the *Archives Générales de Médecine*, for 1868), already quoted, may be referred to with advantage. That author brings forward a great deal of evidence to show that it is not the mere fact of rupture which interferes with the uterine contractions, but the sudden occurrence of collapse which sometimes follows on that accident : and this conclusion is borne out by this series of cases. Thus of the nine cases in which the pains are said to have ceased suddenly upon the rupture, in six the accident was attended by immediate collapse. Just, therefore, as in collapse the heart slackens in force and frequency of contraction, so does the uterus. So far these nine cases support Dr. Jolly's argument ; nor do the three exceptions militate against it so much as point an alternative condition under which the

throes may cease. In Cases 7 and 9, although there was at first no collapse, yet the throes ceased at once; the rupture was sudden and complete, and the contents of the uterus had in each case escaped into the abdominal cavity. When, therefore, the uterus is emptied, the throes may cease as suddenly as they do in consequence of collapse of the vital powers. The other case does not point to this alternative quite so clearly, but still, I think, plainly enough. Labour having continued some hours, shortly before the arrival of the attendant the throes suddenly ceased. There was no pain, and the fetus had not receded. The patient was found standing upright, and complaining of sickness and faintness merely. A shock sufficient to arrest uterine action had been inflicted. But a dose of spirit having been administered (for rupture was not at that time suspected), in ten or fifteen minutes the uterus began to act again. So, again, in Case 16, rupture occurred after twelve hours of labour, and the throes ceased immediately. But the cephalotribe being used about an hour after the accident, the head was no sooner extracted than the uterus began to act again, and so effectively as to expel the body without any assistance. As in the last case, the degree of collapse immediately induced by the rupture, though sufficient to cause a temporary arrest of the throes, was yet so moderate that after a short interval a small stimulant proved enough to set the uterus in action again; the stimulant being probably in the last case a reflex impression received from the distended perineum and adjacent parts.

If Dr. Jolly's view of the cause of cessation of the throes be accepted, it is perhaps superfluous to point out that the degree of laceration has nothing to do with that result directly, for, as I shall show below, collapse does not occur in proportion to the extent of injury. It may, however, be worth while to devote a few words to the point as illustrated by the present cases. To take, in the first place, the three Cases 4, 8, and 14. In all of these the throes were uninterrupted by rupture; but there is no reason to think that the injury was incomplete at the time of delivery. In Case 4, the patient showed no symptom but a little faintness until some time after the completion of the third stage



of labour ; yet she died in sixty hours, and it was found post mortem that the uterus and peritoneum were both torn transversely in front for several inches. Case 8 is one similar to the last. No symptom was observed until fifteen minutes after the completion of the second stage ; and it was then only in removing the placenta that a rent was discovered, through which the intestine descended into the vagina. In Case 14, the acumen of Dr. M'Clintock discovered the existence of rupture. The labour was obstructed, and the patient presented no unusual symptom at first sight ; but craniotomy being called for, in examining the abdomen to ascertain the state of the fetus, emphysema was detected in the hypogastric region. A laceration of the uterus was suspected to be the cause of it ; and so it turned out. In these three cases, then, complete rupture existed, with normal contractions of the uterus. To examine the question conversely, Case 17 is an example of rupture slowly completed. Somewhat feeble throes having persisted for six hours, the patient began to get exhausted ; the pains slackened, the exhaustion increased, and at the ninth hour the uterus ceased to contract. It was six hours subsequent to this that, in attempting to relieve the bowels, she felt something give way within her ; upon which she became completely collapsed, and died within fifteen minutes. In this case, then, the throes slackened in proportion to the advance of collapse ; and the latter having become severe, they ceased entirely, although the rupture was at that time incomplete.

It thus appears that cessation of the throes depends not necessarily so much upon the degree or extent of laceration as upon the amount of collapse induced on the evacuation of the uterus. It is to be expected, therefore, that this symptom will not, any more than those already considered, appear with any regularity in a number of cases ; for sudden evacuation of the uterus into the abdomen is occasionally witnessed on the one hand, and on the other, different persons are found to resist shock with different degrees of success. But a fact of far more importance is, that it is equally apparent that the mere continuance of uterine action is not

sufficient of itself to upset a diagnosis of rupture founded upon other symptoms ; for if the rupture be not of a size to allow the fetus to make its exit thereby at the same time that it has not induced collapse (as is often the case), the uterus will continue its efforts to relieve itself in the natural manner, and not infrequently with success.

*Of Retrocession of the Presentation.*—This symptom, depending as it does in part upon the size and situation of the laceration, can only be regarded as confirmatory of suspicions otherwise roused in such cases as offer it. It will not appear if the fetus be in any way impacted in the pelvis (itself a predisposing cause of laceration), or if the laceration should be small, or in such a position with regard to a surface of the fetus—its back or abdomen—as not to allow the latter to pass through it without some change of position. Also, if rupture should occur at the same moment that the head is expelled, the body will not recede ; and perhaps if the uterus become spasmodically contracted at the moment of laceration, and continue so, the fetus will not escape into the abdomen, though the rent may be large enough to allow of its passage.

Case 7a, in which the rent was in the median line, extending through the vagina and lower part of the uterus, is perhaps an example of this. It may be supposed, however, that in case the uterus should make no further effort to empty itself, the child need not traverse the rent ; of which Case 3 affords an instance. The laceration was longitudinal in direction, several inches long, and did not involve the vagina ; it was, therefore, in a position favourable to the passage of the fetus. But uterine action entirely ceased after one or two throes, which were somewhat stronger than those occurring previously, and during which the rupture occurred ; and the child lay in the ruptured uterus until the fifth hour, when the mother died.

The following is an analysis of the cases with regard to this symptom :—The fetus receded in Cases 1, 6, 7, 9. Retrocession is not noted in Cases 5, 6a, 11, 12, 17, 21—in the last the arm presented. The fetus did not recede in Cases 2 (head impacted), 3 (vertex presentation—rent

several inches), 4 (labour terminated naturally—rent transverse at neck several inches), 7a (already described), 8 (labour terminated naturally), 13 (breech presentation, delivery assisted), 14 (head impacted), 16, 18 (the pain which expelled the head coincided with the occurrence of rupture). In Cases 10, 15, 19, 20, the laceration occurred during operation.

*Of the Speedy Occurrence of Collapse.*—It is difficult to say which of these two symptoms—sudden cessation of the throes or sudden collapse—is the more generally received as a symptom consequent upon this injury, but perhaps the latter is the more so, for, not only is the substitution of faintness for collapse in these cases less clearly recognised in books of instruction than exceptions to some of the other symptoms—barely hinted at as *they* are—but it is more in accordance with what one expects of so sensitive a structure as the peritoneum, that so very serious an injury to it should be attended by the symptoms to which its injury usually gives rise, in their most urgent form. But the fact is not so; and recent events show that it is not at all unnecessary to demonstrate it by reference to clinical experience.

In Cases 1, 5, and 7a the time at which collapse occurred is not noted; it is left doubtful in Case 16 by the report; in Case 10 the patient was under chloroform, and collapse was not observed; I shall, therefore, omit it, although, strictly, I might fairly quote it as one in which collapse did not occur suddenly, since that symptom was observed in Case 20 under similar circumstances, and was at first referred to the anesthetic.

Omitting these five cases then, of the remaining eighteen, in six collapse set in suddenly upon the completion of the rupture (Cases 6, 6a, 11, 17, 20, and 21). It did not occur until hours or minutes after the rupture in Cases 3 (four or five hours), 7 (two and a half hours), 9, 12, and 13 (some time after), and in Case 15, not at once. It did not occur until hours or minutes after delivery in Cases 2 (fourteen hours), 4 (five hours), 8 (thirty minutes), 18 (thirty minutes), 19 (three hours), and 14 (time doubtful). Out of eighteen cases then, in six only did collapse occur im-

mediately—or, in no more than 33·3 per cent. ; while in more than 66 per cent. this symptom did not make its appearance until some time, and very often a considerable time, after complete rupture. And it is especially important to notice that in so large a proportion as 33 per cent. collapse did not occur until labour had been terminated either by nature or by art. In considering the action of the uterus after rupture, it was asserted that the extent of laceration bears no direct relation to the amount of collapse. Upon this analysis it is to be observed, therefore, that Cases 7 and 9 are examples of the severest injury of this kind possible, although they appear among those in which the appearance of collapse was markedly delayed. In Case 7 the head had begun to enter the world when a complete rupture suddenly occurred, and the child was found on examination to have receded beyond reach—that is, to have entered the abdominal cavity—and yet collapse did not come on for two and a half hours. In Case 9, the patient, on the occurrence of a rupture so extensive that the fetus at once entered the abdomen, actually, as before stated, jumped up on her knees, and was found still maintaining herself in that position some time afterwards. In Case 12 there is no reasonable doubt but that the rupture occurred before the attendant saw the case ; yet he found the patient standing upright, and in such a condition that he did not even suspect rupture for a considerable time. With these compare Case 20 ; in which, although the laceration was, in fact, no more than a perforation caused by a spicula of bone, collapse was immediate ; and so complete that it was supposed the patient was about to die of chloroform, under the influence of which craniotomy was being performed. These four cases are extreme cases chosen for the sake of illustration, but no constant proportion between the size of the wound and the amount of collapse can be established by any comparison of this series. Hence it is plain that the evidence of shock manifested in any particular case will depend upon other circumstances than the size of the wound or, probably, the mode in which it occurs. It may be that the chief of these circumstances is the vital energy of the individual, which varies in each



person ; and with it the rate at which collapse appears may be expected to vary too. Cases are on record in which the fact of laceration has been discovered accidentally during a vaginal examination ; no symptom of any kind attending or ensuing upon it.

*Convulsions* are not reported to have occurred in any of these cases.

The foregoing analysis of twenty-three unselected—and in that sense, therefore, consecutive—cases of rupture of the gravid uterus, shows that variations from the classical descriptions of such cases are not only common, but are actually more common than the typical cases themselves. To take two symptoms singly : it is found that the aid to diagnosis to be gained from the character of the uterine contraction was in 73·4 per cent. none ; from the occurrence of characteristic pain at the moment of rupture in 57·8 cases per cent. none ; from the action of the uterus subsequent to rupture in 47·1 cases per cent. none ; from the sudden occurrence of collapse in 66 cases per cent. none ; from retrocession of the child (in such cases as retrocession was not mechanically obstructed) in 75·8 cases per cent. none ; from the symptoms of external hemorrhage and of convulsions none.

It now remains to take a general view of these cases ; from which it will appear that in some an early diagnosis of the patients' condition was impossible, for the simple reason that they showed no extraordinary symptom whatever ; while in many, that although the condition of the patient attracted attention, there was no one symptom marked enough, or any combination of lesser symptoms, which would have justified the diagnosis of laceration had the presence of that injury been suggested.

In Case 2, labour began at 12 P.M. At 3.30 the throes slackened ; at 4.30 they ceased. A dull, heavy pain about the umbilicus, increased by pressure, was then noticed. At 6.30, assistance being sought, the head was found to be impacted, but was easily delivered with the forceps. Post-partum hemorrhage none. After-pains slight. At the fourteenth hour from delivery was doing well. At the twenty-

seventh hour aggravated ventral pain. At the thirty-fourth hour, death. There were no other symptoms at any time. Yet rupture of the uterus existed.

In Case 8 the attendant was present for about two hours before delivery, which occurred in the usual manner and without any unusual symptom. About fifteen minutes after the birth of the child, the placenta not following, gentle traction was made on the cord, and the finger passed up towards its insertion. The hand was then passed into the vagina, and a rent discovered at the lower part of the uterus, through which a part of the small intestine was brought down and identified. Soon afterwards the patient became faint; the placenta was then removed, and she became collapsed. Ultimately she recovered.

In Case 12, the eighth labour, there were strong pains for eight hours—almost continuous for the last two hours. Accoucheur summoned (from some distance); shortly before his arrival the pains entirely ceased. The patient was found standing upright, and complaining of a little sickness and faintness merely. The accoucheur did not suspect rupture to have occurred; he gave hot spirit-and-water, and determined to wait; but in fifteen minutes there was another slight pain, from which time the patient began to collapse, and she died in an hour.

Case 14 was under the care of Drs. Montgomery, M'Clinck, and others. The rupture was diagnosed by the latter only, who referred emphysema, accidentally discovered in the hypogastric region, to that injury. There were no symptoms at all until after delivery, which was effected by craniotomy. This was necessitated by the state of the pelvis, not by the laceration.

Case 19, from the collection of Dr. Jolly, is of especial interest on account of the accuracy with which it was observed. Ineffectual efforts at delivery in a case of shoulder presentation had been made before the patient's admission to a maternity hospital, under the care of M. Tarnier. The latter gentleman turned the child and delivered the woman, who, on account of the peculiarity of the case, was under close observation. Three hours after delivery she was doing

well, with a pulse of 88. Nevertheless she died at the thirty-sixth hour, when she was found to have a perforation in the neck of the uterus, and a rupture just above it .05 metre in length.

In the Cases 8, 14, and 19, it will be observed that there was no symptom indicating the occurrence of rupture until long after delivery in any one of them. In the others, which I have selected for the sake of example, it will be admitted that the symptoms which arose, although they are easily enough referred to the laceration upon subsequent reflection, were not such as would have suggested that accident at the time. As in Case 2, for instance, there is nothing distinctively peculiar in the failure of uterine contraction at so early a stage of labour as the third or fourth hour, nor in the aching pain which was then observed, as it is described in the report. I watched a case quite recently, to which the description of Case 2 applies with curious exactness. The patient had been in her third labour about six hours, and up to that time the pains had been regular (ten minutes), and fairly expulsive. But upon examination it appeared that the pains had slackened during the first half of the seventh hour, and it was because they had entirely ceased at the end of that hour that assistance was sought. The patient was faint and despondent; there was no uterine contraction, and in place of it there was a dull, fixed pain of considerable severity, which was very much increased by pressure in the left hypogastric region. The possibility that laceration had occurred suggested itself; nor did the probability that the pain referred to arose from the disturbance of old adhesions, the result of peri-uterine inflammation, from which the patient had suffered after a previous confinement, enable a favourable diagnosis to be given with more confidence; for such inflammations are among the predisposing causes of rupture. Under all the circumstances, however, the case was not interfered with and its termination was happy; the throes returning in about two hours, and delivery occurring in four hours.

So, again, in Case 12, the only symptom which it may be thought, upon reflection, should have attracted attention,

is that of cessation of the pains. But it is contrary to the ordinary teaching, and to our practical experience of other injuries to the peritoneum, that a patient, with a rent in her uterus, should be able to maintain the erect posture, and should complain of a "little" sickness and faintness merely; and the accoucheur is not to be blamed if he do not at once seize upon these ordinary incidents of labour, and connect them with the serious and unusual injury which may ultimately prove to be their true cause.

---

## Reports of Hospital Practice.

---

### UNIVERSITY COLLEGE HOSPITAL.

---

#### INTERMITTENT ECZEMA—DYSMENORRHEA.

*Under the care of* JOHN WILLIAMS, M.D.

M. A. D., aged twenty-seven years, a servant, came to the hospital in August last, complaining of headache, giddiness, pain under the left breast, shortness of breath on exertion, palpitation of the heart, and flatulence. She was very pale. Pulse was regular, large, and soft. Tongue pale, but clean. Appetite bad. Bowels regular.

The catamenia were profuse, lasting a week, containing clots, and for the first three days accompanied by pain.

The four fingers of the right hand and a part of the dorsum and palm were affected with eczema. This came on three years ago, and was attributed to having inserted the hand when wounded into putrid water.

She gave the following history:—When she was eleven years of age a white vaginal discharge made its appearance, and has continued to this date. For about two years before the appearance of the catamenia the white discharge used to increase in quantity at monthly intervals. When she was fifteen, for one year before the appearance of the menses a raw place appeared between the middle and ring fingers of



the right hand every month, lasting a fortnight from its appearance to its disappearance. The menses appeared for the first time in the seventeenth year; the raw place did not afterwards return. She suffered no pain with them, but was not regular, for they were absent sometimes for three months. At eighteen she had a discharge of blood, more or less, for a whole year. She had no pain during this time, but became very weak. She went to Scotland, recovered her strength, and afterwards enjoyed good general health until twelve months ago. Pain with the catamenia began when she was twenty-one years of age—in the back and ovarian regions—and has continued since. She knows of no cause for it. The eczema appears on the hand about a fortnight before the catamenia, weeps, forms cracks on the flexures of the fingers, and causes great irritation. A day or two before the appearance of the flow it dries up, gets better, ceases to weep, but does not get well.

The uterus was well developed, much anteverted, cervix soft, and os patulous, lips had a velvety feel. The left ovary could be felt high up. (The catamenia appeared later in the day.) She was ordered ammonio-citrate of iron and nux vomica. The next menstrual flow appeared twenty-eight days after the appearance of the last, and was accompanied by much pain, especially in the left ovarian region, and bearing-down. On the second day she passed a clot, after which she became free from pain. The flow was followed by a profuse leucorrhea. Ordered four minims of Fowler's solution three times a day.

During the following intermenstrual interval the hand continued better, but still the eczema reappeared about the middle of it.

The next period set in three days before it was due. The day previous to its appearance she had much pain in the back and pelvis, and passed blood by the rectum. (Bowels were constipated.) During the first two or three days of the flow she had pain, and the discharge contained clots. The right kidney could just be felt, and when pressure was made upon it pain similar to that she had at one period of the flow was produced. The next flow appeared at the

right time. It was, however, preceded by great pain in the right ovarian region and bleeding at the nose. She had had headache for the previous fortnight. The menstrual discharge contained clots. The hand was much better. After the cessation of the catamenia a tender swelling (probably the right ovary) was found on the right of the uterus and close to it. She was ordered flying blisters to the right ovarian region and to continue the arsenic. She has continued the treatment up to the present date, and is now apparently well. She has regained colour, and lost the symptoms dependent on the anemia; the catamenia are regular and painless, the menstrual discharge containing no clots; leucorrhœa has ceased. There is no uterine or ovarian tenderness.

This case is curious, and the only one in which I have met with eczema alternating with menstruation. Leucorrhœa setting in five years before, and increasing periodically for one year before the appearance of the catamenia, the periodical increase alternating with eczema of a finger-web, and then a prolonged menorrhagia, followed soon by dysmenorrhœa and painful discharge of ova, seem to point to a condition of the inner surface of the uterus similar to that of the hand—that is, an eczema of the uterine mucous membrane. If such was the condition of the uterus, is it not possible or even probable that the ovaries became affected similarly by extension of the disease along the Fallopian tubes? There are one or two points which favour such a supposition. The uterine was apparently the primary lesion, and the ovarian secondary to it. Eczema sometimes extends very rapidly over a large surface, but chronic eczema remains often almost stationary. And supposing the eczematous condition of the mucous membrane of the uterus to have begun at the same time as the leucorrhœal discharge, it took ten years before it had manifestly affected the ovary, for the patient had no ovarian pain until she was twenty-one years of age. There appears to be no anatomical reason why eczema should not affect the ovaries, for their surfaces are believed to be mucous rather than serous. Again, arsenic cured the lesions in the ovary, uterus, and hand, and the latter was clearly eczema.

---



---

# THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

FEBRUARY, 1876.

---



---

## THE COLLEGE OF SURGEONS OF ENGLAND, AND OBSTETRIC MEDICINE.

THE earlier history of the College of Surgeons of England, like that of all our other medical corporations, displays not only a disregard but a disdain for Midwifery practice. In the charter obtained in 1843 it is provided that no Fellow shall be eligible to be a Member of the Council unless it be certified "particularly that he does not practise, and has not within five years practised Midwifery." So deeply rooted is this traditional antipathy to Obstetrics, that even now no candidate for the Membership is examined in Midwifery. Time, however, which restores the balance in all things, and raises to their proper position those things which have been unnaturally depressed, has of late years been rapidly elevating Obstetric Medicine. Those pursuing this branch have displayed learning and skill equal to the best physicians and surgeons; they have united and formed powerful societies, and their interests and claims cannot much longer be disregarded. A few years since, the College, finding the importance which the Obstetric art was assuming, determined to recognise it, but very much in the same way as it has since Dental Surgery—coily and uncomfortably—as if we were ungainly country cousins, whom it could not ignore, but with whom it was ashamed to be seen. In 1852 it obtained a charter, by Clause 17 of which the College was empowered to test the fitness of persons to practise Midwifery and to grant certificates of such fitness. About one thousand qualified medical men have availed themselves of the opportunity thus afforded them of obtaining certifi-

cates in Midwifery. One has also been received by a gentleman having no medical qualification. Possessors of this certificate can register under the Medical Act. The College considerably secured this right when the Medical Bill was passed, but it would have been all the same had they not, for the Medical Council were obliged to register the Irish Midwifery licences, although they had been left out of the schedule. The number of men now seeking this certificate is not large, but the deficiency certain women seem anxious to remedy. Three have expressed their desire to appear before the Board of Examiners in Midwifery, and their application has been duly considered. A legal opinion has also been obtained, and the Council have decided that the word "persons," used in the clause, applies to both sexes; and that if any woman should desire to have their certificate and can conform to their regulations she must be admitted to examination. It turns out that neither of the three aspirants could completely satisfy the College as to preliminary requirements, so for the present that question rests. But some members of the Council, desirous of seeing women associates of the College, used their influence so effectively as to lead to the appointment of a committee, to consider whether the regulations might not be altered to oblige those of the other sex in such a way as to render it impossible for any one to say that the College was unreasonably exacting in its demands—*i.e.*, whether less time of study and a less extensive curriculum might not be found sufficient for those seeking the Midwifery certificate. The decision of this committee is what might have been anticipated. Many difficulties presented themselves. Among others was this—what would be the position of these certificated Midwifery practitioners when the Conjoint scheme came into operation? They would not be allowed to pass through the one portal, and it would be exceedingly awkward to create a class of practitioners capable of being registered, and yet of whom a lower examination had been demanded than from those who had entered the profession after having only shown themselves possessed of the minimum amount of medical knowledge compatible with



public safety. For a season, therefore, the College has decided not to make any new regulations facilitating the acquirement of its Midwifery certificates. It will probably wait until the Conjoint scheme is settled. This will give the Council time to reflect, and the profession an opportunity to express its opinions. No one could believe that the Council of the College, in coming to their recent decision, had been actuated by any but the highest moral and philanthropic motives. Gentlemen of such high culture and character could not lower the standard of an examination either to augment the income of their College or gratify the wishes of a few ambitious women. Neither would they without a struggle consent to carry out a law, the effect of which would be derogatory to the medical profession, and tend to provide parturient women with inadequate assistance. All this every one will readily admit, but at the same time it appears to many exceedingly doubtful whether the determination of the Council, if it should be eventually carried out, would prove satisfactory to medical men or conducive to the welfare of lying-in women. It would be unsatisfactory to medical men, because the majority of them believe women to be deficient in the nerve and strength necessary for the successful treatment of Obstetric emergencies. Because even if they had this nerve and strength it is believed that they could not practise Obstetric Medicine and Surgery efficiently without having passed through a curriculum coextensive with that demanded of medical men ; and because the easy admission of "persons" to the position of registered practitioners would open an equivocal side-door not only to women but to hundreds of unqualified men who would only too gladly avail themselves of such a ready method of escape from their present unenviable status. It would most certainly be very unsatisfactory to those alive of the thousand gentlemen who have obtained the Midwifery certificate, for it would be sadly depreciated in value and its holders proportionately degraded. It would also be particularly distasteful to our branch of the profession, for we believe and know that a skilful obstetrician must be a physician and a surgeon and something more. It would be

reducing us to the level of dental surgeons (also called "persons" in the charter) to demand only a short and easy curriculum on the idea that Obstetric practice can be properly carried on with a partial professional education. We have always maintained that there is no step between the midwife and the medical woman—between one who can manage natural labours and knows when further assistance is required, and another who has by superior mental endowments and instruction passed such examinations as justly place her on a level with fully qualified men and confer upon her all the privileges of registered practitioners. These are two distinct classes of women, and should not be confounded. The midwife's door to practise must be a humble one, and those who would be medical women are mistaken when they endeavour to enter through it a noble and exalted profession. The King and Queen's College of Physicians in Dublin has been passing through the same burning ordeal as the College of Surgeons in England. In Clause 30 of a charter granted in 1692 it is given "full power and authority to examine all midwives and to license and allow all such as they shall find skilful and fit to exercise that profession." Two midwives were thus licensed, one in 1696, another in 1731. After this the Council discovered that "midwives" meant also men, and it began to grant, and does to this day, licences to male practitioners in Midwifery and the diseases of women and children. Two years ago some of the Fellows desired to resume their power of licensing females in Midwifery, and it was at length determined to admit women to examination (not including Obstetric operations) and grant a non-registrable licence "to exercise the calling of a midwife and nurse tender." If the College of Surgeons see fit to follow in the footsteps of their Irish brethren, and grant two certificates in Midwifery, one to medical men and women, and another to midwives, Obstetric medicine might not suffer much by the arrangement; but should they after a pause determine to force upon the profession an inferior class of registered Obstetric practitioners, the voices of all who care

for the honour of Medicine will be raised against their action. At this juncture it behoves obstetricians to be jealous of their threatened honour, and alive to their interests. A critical period in their existence is now at hand. Opportunities will be presented and accepted or lost. The passing of the Conjoint scheme will, at the outset of medical life, render all equal. Those who would afterwards distinguish themselves as physicians must become Doctors of Medicine, or Members and Fellows of the Colleges of Physicians. Those who would be eminent surgeons must be Masters of Surgery or Members and Fellows of the Colleges of Surgeons. What are obstetricians to do? By what title is the public to be made aware of the fact that a medical man has devoted great attention to Obstetrics? Our universities confer no degrees, as on the Continent, in Obstetric medicine. Must we for ever be content with the certificate of the College of Surgeons or of the Irish licensing bodies? Honour will not be thrust upon us. We must win it by hard and constant work. The three Obstetrical Societies are numerous, powerful, and rich; let them now unite, and with the Obstetric Section of the British Medical Association endeavour to improve the Obstetric examinations of our licensing bodies, extend the Gynecic accommodation and instruction of our hospitals, and secure a more complete representation in our Medical Councils. Above all, let us insist upon a higher Obstetric qualification, either by obtaining from universities a special Obstetric degree, or by the establishment of Royal Colleges of Obstetricians.

---

## Abstracts of Societies' Proceedings.

---

### OBSTETRICAL SOCIETY OF LONDON.

*Meeting, January 5th, 1876.*

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., *President,*  
*in the Chair.*

---

#### *On Puerperal Fever.*

#### PRESIDENT'S ADDRESS.

GENTLEMEN,—At the commencement of another year it is usual for your President to take a brief retrospect of the past, to review the work of the session, and report on the general condition of the Society. During the past session the Society has again done excellent work, and several papers of unusual interest, with the discussions upon them, will appear in the forthcoming volume of "Transactions." The event of the session, however, has been the discussion on puerperal fever; and the Society is greatly indebted to Mr. Spencer Wells for taking the initiative in the debate, and for the able manner in which he drew up and illustrated certain propositions, in the form of interrogatories, to elicit the opinions of Fellows. The discussion, indeed, not only proved attractive to large audiences who assembled in this room on each successive evening, but the subject had so profound an interest for medical practitioners everywhere, that the published debates were read with eagerness throughout Great Britain, Ireland, and other countries where obstetric medicine is cultivated. During the progress of the debate, the Society had the advantage of hearing the matured opinions of many of its Fellows resident in London and the provinces, some of them being professors and teachers of world-wide reputation. We were honoured by the presence of the Professor of Midwifery from one of the Scottish Universities, and one celebrated professor crossed the Atlantic to state his views, and combat prevailing ideas which he believed to be erroneous. The Society should also acknowledge its indebtedness to the distinguished surgeons and physiologists who so ably sustained the debate in some of its collateral aspects.

The Society may well be congratulated on the widespread interest attached to its proceedings this session; and among the other benefits accruing, not the least is the fact, that the discussion on puerperal fever attracted for the first time to these rooms two of our honorary resident Fellows, Dr. Arthur Farre and Dr. West. It is well known that both these gentlemen are authorities of the highest order in the department of Obstetric Medicine, and their contributions to the



debate will henceforth enrich the archives of the Society. And surely a topic of such commanding interest and gravity as puerperal fever, or by what other term it may be proposed to call it, well deserves careful investigation. It is a study which may worthily occupy the keenest and best intellects. The acute forms of illness which occur after delivery, and often end fatally, are especially cruel affections ; attacking women, as they commonly do, during the very flower of their age, just at the dawn of fresh hopes, and at the time of their greatest usefulness in life. The anxiety of mind caused to the medical man when such cases occur in his practice, and the damage which, rightly or wrongly, results to his professional reputation, can only be appreciated by those who have had the painful experience. The mental anguish so engendered is reported, indeed, sometimes to have culminated in mental aberration.

Dr. Farr, of the Registrar-General's office, estimates the mortality after childbirth in this country as 1 in 190 women delivered ; but Dr. J. Matthews Duncan, in his address at Norwich in 1874, regards this as much too low a rate, and, after careful computation, states the mortality, within four weeks after delivery, to be 1 in 120 ; while, if the time be extended beyond four weeks, he believes that the number of puerperal deaths reaches as high as 1 in 100. This last calculation is so appalling, that we may vainly hope it is in some way exaggerated ; but, taking the lowest estimate, 1 in 190, we find ample scope for grave reflection, and more especially as a large proportion of the deaths so computed arise from puerperal fever, or the group of post-partum diseases which have been somewhat vaguely designated by the name of "puerperal fever." Moreover, the number of ascertained deaths does not express all the mischief done. Many of those who survive the attack are greatly hindered in recovery, are exposed to much suffering, and possibly remain permanently enfeebled or maimed for life. If reason were required, therefore, for the interest manifested in the late discussion, it is sufficiently forthcoming in the above statistics, and more especially as in and out of the profession there is a growing impression that these deaths are in a great measure preventable.

Disappointment has been expressed by some critics, that after so protracted a discussion, in which so many eminent men have taken part, no more definite conclusions should have been arrived at, no more practical results obtained. It was inevitable in a discussion on such a subject that the most diverse opinions should be expressed ; but, with all due submission, I venture to take exception to such criticism, and I trust I may be able to show that some advances have been made towards a more precise knowledge of the subject. The debate has obviously enabled us to take stock of our knowledge, and, in doing this, it is evident that certain ideas, hitherto fragmentary and vague, concerning the nature and origin of puerperal fever, have assumed a more concrete form than heretofore. The progress of pathological science has in this, as in other departments, been

gradually producing certain definite conclusions and results, which become more and more apparent as time goes on, but hitherto have scarcely been fully appreciated.

Had time permitted, your President might, at the close of the debate, have rendered a service to the Society by presenting a summary of the various arguments used in treating the several propositions laid down by Mr. Spencer Wells. As the occasion did not present itself then, I propose to occupy your time this evening in an endeavour to take up the threads of the discussion, and trace out what may be the general conclusions or practical results arising from it. The task of summing up, however, is by no means an easy one. In view of the many conflicting opinions which have been expressed on some of the points considered, it has seemed like an attempt to educe order out of chaos; and I trust the Fellows who have spoken will pardon me if in the remarks I have to make I in any sense misinterpret their meaning, and if I adopt certain views in preference to others.

In attempting to summarize the debate, I shall venture to make such commentaries as have occurred to me; and, without necessarily adhering to the form or order of the questions proposed by Mr. Wells, I shall pass in review the various points he has raised.

1. *Definition.*—As to definition: what is it that we mean when we speak of “puerperal fever and its relations to the infective diseases and pyæmia?” On this question of definition we had the assistance and heard the opinion of so high an authority as Dr. Arthur Farre, who has defined puerperal fever to be “a continued fever, communicable by contagion, occurring in women after childbirth, and often associated with extensive local lesions, especially of the uterine system.” Dr. Farre, however, has informed us that this definition is only for the purpose of identification, and does not pretend to explain the phenomena of the disease, or to indicate its nature. As an illustration of the extreme difference of opinion expressed in this discussion, I may point out that, even on this preliminary question of identification, Dr. Snow Beck contends that the definition, as published in the Nomenclature of the Royal College of Physicians, cannot be maintained in practice; while, on the other hand, Dr. For-dyce Barker thinks the definition “absolutely perfect.” It is obvious that the definition would include all the forms of infective continued fevers which may attack the lying-in woman, whether they are peculiar to the puerperal state or not; and in this sense it may be faulty, but it is not easy to define what is almost protean in character, and exhibits itself in such varying forms. The definition may well answer the purpose, if only it be borne in mind that in this sense puerperal fever is a comprehensive term, including a diversity of puerperal febrile affections, and that it does not express any theory or doctrine as to their pathological nature. In this respect it differs essentially from the definition of typhus or typhoid fever, and other zymotic diseases, which have characteristic symptoms, well-defined patho-

logical appearances, and are believed to be always produced by the same specific poison.

2. *Is Puerperal Fever a Specific Disease?*—Had I been asked no long time ago if I believed in the existence of a perilous and often fatal form of fever peculiar to the puerperal condition, I should unhesitatingly have replied in the affirmative. Nevertheless, the several speakers who have addressed themselves to this question have with a surprising unanimity denied the existence of a specific puerperal fever, and expressed their conviction that the so-called puerperal fevers are diseases recognised as occurring in other than puerperal patients, but modified and rendered more formidable by the local and systemic peculiarities associated with parturition. Professor Leishman, of Glasgow, indeed, who had taught and written concerning a specific puerperal fever, made a formal recantation of his former views on this subject, and, with certain reservations, stated his belief that a large proportion of so-called puerperal fever had its origin in pyemic or septicemic infection. The only notable dissentient from this view is our distinguished honorary fellow Dr. Fordyce Barker, of New York, who, in a very able speech, asserted the doctrine of a distinct and specific puerperal fever. Dr. Barker's defence of his position was supported by a variety of arguments that were at once ingenious and forcible, and it may fairly be presumed he has put before us the most weighty reasons which can be urged on that side of the question. The fact remains, nevertheless, that clinical investigation and pathological research have both in recent times been pointing in an opposite direction, and teaching that what has hitherto been called puerperal fever or fevers are but forms of blood-poisoning or septicemia similar to those observed in other than puerperal patients, having, perhaps, a diversity of origin, and intensified and modified, it may be, by the peculiarities of the puerperal state, but still essentially the same, and not constituting a pathological genus apart. While the discussion was pending, I took occasion to read the article on Surgical Pyemia in Holmes's "System of Surgery," written by Mr. Callender, the able surgeon who was good enough to come here and tell us something of the surgical aspects of septicemia; and I confess I was struck not only by the identity of symptoms between puerperal fever patients and surgical patients affected by pyemia, but also by the corresponding varieties in each case, whether we look to the origin, severity, or pathological results.

In employing the term "pyemia," Mr. Callender guards himself against being supposed to imply that pus is the active poison producing the symptoms described. Modern pathology teaches that the blood may be poisoned by other ingredients than purulent matter; and those of you who were present on the first night of the debate listened to a lucid exposition on this subject by Mr. Hutchinson, in which he expressed his belief that pyemia, or pus in the blood, was due to inflammation of the patient's own veins, and to suppuration in their interior. Septicemia he defined as blood-poisoning induced



by inflammation of the patient's own tissues other than the veins, whether that inflammation arose spontaneously, or was caused by an irritant from without. It is now clearly established that septicemia, pyemia, and phlebitis are distinct pathological conditions, although all may be coincident in the same patient.

Formerly, two opinions were held concerning the pathological nature of puerperal fever. Puzos, Levret, Hamilton, and White regarded it as an idiopathic putrid fever. Armstrong, Mackintosh, and Campbell maintained that it was essentially a local inflammation, of which the fever-symptoms were merely a sympathetic consequence. Boyer, Le Gallois, and Cruveilhier endeavoured to show the analogy between the condition of a woman recently delivered and any other patient who was the subject of surgical injury, and to point out the similarity of the morbid affections to which both were liable. Cruveilhier, indeed, compared the uterus after delivery to an amputated stump, inasmuch as its muscular walls were laid bare, and at the placental spot were the gaping orifices of divided vessels. Subsequent research has proved that the uterine walls are not so absolutely denuded as M. Cruveilhier supposed; but there is no doubt as to the presence of large open channels at the placental spot, through which deleterious matters may gain a ready access to the general circulation. Following out this idea, Sir J. Y. Simpson in 1850 wrote a memoir on the analogy between puerperal and what he called surgical fever—a term much objected to by some surgeons.

Probably the best essay on puerperal fever which has appeared during the present generation is that by the late Dr. Robert Ferguson, one of my predecessors in the chair of Midwifery at King's College. Dr. Ferguson attributed puerperal fever to a vitiated state of the fluids of the body, and regarded all varieties of local lesion as consecutive to vitiated circulation. He pointed out, what is now generally conceded, that none of the local lesions are constant, and so "none can be assumed as the essence of the malady; and, as all may exist, they may each and all be regarded as the results of a cause capable of producing them singly or conjointly." Whether, therefore, the fever takes the form of the "peritoneal," the "gastro-enteric," the "nervous," or the "complicated," where several organs or tissues are attacked at once, a poison in the blood is the one source and cause. So generally accepted is this doctrine advanced by Dr. Ferguson, that Dr. West was in some sort justified in saying, "We have not advanced far, if at all, beyond the conclusions which Dr. Ferguson laid down." In another sense, Dr. West's assertion stops short of expressing the whole truth of the case; for if we have as yet found no better explanation of the phenomena observed in connexion with puerperal fever than "a vitiated state of the fluids," nevertheless a whole army of ardent workers have been investigating and adding to our knowledge concerning the nature of the poison which produces such fearful results, the various ways in which it finds its entrance into the blood, and the best methods of obviating its effects. A



further impetus has been given to our knowledge of the subject by Virchow, who, after repeating the experiments on purulent infection recorded by Gaspard in 1809, and adopting the term "septicemia" from Piorry as appropriate for forms of blood-vitiation other than by pus, published his researches on thrombosis and embolism. Among obstetricians, the names of Spiegelberg, Schroeder, D'Espine, Barnes, Matthews Duncan, and others, are well known as writers on this subject, who have all contributed something to its further elucidation.

As to the way in which the poison finds its entrance into the systemic circulation, Dr. Ferguson did not overlook the peculiar condition of the interior of the uterus at the placental spot, as affording a ready mode of ingress for what might be deleterious; and he held that all wounds and bruises in the genital canal might create diseased secretions from a traumatic surface, and by absorption give rise to the phenomena of puerperal fever.

Taking, then, the outcome of the discussion in this Society, and the evidence from other sources, it may be stated that puerperal fever, or fevers which are attended by high temperature, and are commonly associated with extensive local lesions, are due to the inception of a morbid poison which vitiates the blood, and which produces a great variety of symptoms in accordance with the nature and intensity of the virus, the amount of the dose absorbed, the state of the patient when attacked, and a diversity of other conditions, which have been alluded to more or less fully by the various speakers.

As to the origin of the poison, whatever may be its nature or chemical composition, Dr. Barnes states that it has two sources: in the one case, the virus is formed in the patient's own body as the result of initial morbid changes in some special part which infects eventually the rest of the system; in the other case, the virus is imported into the body of the patient by contamination from without. To the one class of cases he applies the term "autogenetic," to the other "heterogenetic"—synonyms which correspond to the appellations "auto-infection" and "hetero-infection" of Schroeder.

In the autogenetic or auto-infective form, septic matter is spontaneously formed within the system of the patient, as the result of disorganization and death of tissues by a process which Virchow has termed necrobiosis. Thus, the retention of a portion of placenta, or of membranes, or of blood-clots, may become a source of septic poisoning; or a contusion or laceration of the cervix, vagina, or perineum, may become the seat of unhealthy action, and organic substances in process of decomposition are formed, which, on being absorbed, vitiate the blood. That fluids do pass from the uterine cavity into the uterine sinuses is abundantly proved; and Dr. Snow Beck has stated, as further evidence on this point, that, after the uterus has been injected by the perchloride of iron to arrest post-partum hemorrhage, the uterine sinuses have been found gorged with grumous fluid containing an abundance of iron. Dr. Savage, who has paid particular attention to the anatomy of the uterus, is inclined to doubt if poison is absorbed

by its veins, and thinks the absorbents are the chief agents in this process. Dr. Tilt I understood to hold a like opinion. Dr. Graily Hewitt, Dr. Snow Beck, and others, have enlarged on the great facility afforded to the absorption of septic fluids by an undue relaxation of the uterine walls after delivery, and hence deduce the great importance of thoroughly promoting its contraction upon the expulsion of the placenta. Dr. Hewitt goes so far as to say that he has never seen a case of puerperal pyemia in which this relaxed condition of the uterus was absent. Dr. Beck attributes the influence which has usually been ascribed to hemorrhage as a predisposing cause of septicemia, entirely to relaxation of the uterus as the prime factor; but this, I take it, is a somewhat too extensive statement, as Magendie clearly proved that, with empty vessels, absorption is very rapid in other parts of the body, as well as the uterus. Dr. Tilt attaches great importance to fetid lochia, and their absorption, as factors in producing the febrile affections of the puerperal state. He believes they are not only more frequent than commonly supposed in ordinary cases, but that scarlatina, or other zymotic disease in a puerperal woman, may cause the lochia to become putrescent and virulent: their absorption then adds a fresh peril to the case, and accounts for the serious character of the complication. And if absorption of decomposing or putrescent materials formed in the patient's own body is to be feared when there is no breach of continuity other than that caused by the separation of the placenta, how much greater may become the danger when any laceration has taken place in the genital canal, which exposes a wounded surface to external agencies! Mr. Callender has reminded us of a truth formerly indicated by Benjamin Gooch of Norwich, that there is nothing more irritating or acrid than the fluid of a wound itself immediately after operation, and if this be pent up, it may become a source of auto-infection. Mr. Spencer Wells has become thoroughly conversant with this fact in practising ovariectomy, and he drains off from the cavity of the peritoneum the acrid fluid which would otherwise inevitably poison his patient. But Mr. Callender has further informed us that in surgical practice "a patient may be tolerant enough of decomposition which may be set up by a wound in his own body, but will be intolerant of poison conveyed to him or her from any other wound." If this be true in surgery, *à fortiori* it is true in obstetrics, and demonstrates how essential it is to the safety of his patient that the accoucheur should keep his fingers free from all contamination.

Turning to the heterogenetic or hetero-infective forms, in which the virus is imported from without, the inquiry suggests itself, Whence is derived the poison which, introduced into the system, has such potent results? Is it always one and the same, or are there several poisons capable of producing different diseases in non-puerperal patients, but provoking generically the same results when introduced into the body of a woman recently delivered? It is this part of our problem which seems to me most difficult to solve; and yet upon a correct solution of

it probably depends the reconciliation of many conflicting views, and certainly the adoption of proper prophylactic measures for preventing the spread of puerperal fever. And this brings me to Mr. Wells's second interrogatory, which deals with—

3. *The Relation of Puerperal Fever to Zymotic Disease.*—Concerning the special rôle which the poison of scarlet fever plays in the production of puerperal fever, we have had most valuable evidence brought out by this discussion; and although there has been some conflict of opinion, there are certain practical deductions which are very apparent. In the first place, we have had the evidence of two or three men engaged in large general practice, who have attended labours side by side with cases of scarlet fever, and who are able to tell us from personal experience that a woman recently delivered may go safely through scarlet fever, with the disease so little modified as to retain its distinctive characters, and to be recognisable. Dr. Squire, Dr. Brunton, and Dr. Brown afford conclusive testimony on this point; and Dr. Squire goes so far as to assert that “puerperal fever is not typhus, typhoid, small-pox, measles, diphtheria, nor even scarlatina.” The testimony of Dr. Savage and Dr. Wynn Williams is to the same purpose, at least so far as scarlatina is concerned. Cases have been occasionally recorded like that of Dr. Cordes, of Geneva, and Dr. Jackson, of Notting Hill, where scarlet fever passed through the system of the mother, who was herself protected by a previous attack, and infected her child *in utero*, without serious consequences to the mother. Further, Hirsch and Veit have endeavoured to show by statistics that scarlet fever and puerperal fever are not the same diseases, because there is no coincidence between the prevalence of the two ailments at the same time. On the other hand, are high authorities—Dr. Arthur Farre, Dr. Barnes, Dr. Braxton Hicks—not controverting the statement that a puerperal woman may sometimes safely go through scarlet fever with all its distinctive characters; but asserting besides that scarlatina poison is specially perilous when engrafted on the puerperal state; and that, on account of the constitutional peculiarities after delivery, it is prone to produce what has been called puerperal fever, or a form of scarlet fever so wanting in its usual characters as to be unrecognisable. Dr. Braxton Hicks's paper in the twelfth volume of the *Obstetrical Transactions* is a remarkable exposition on this part of the subject, and furnishes a new starting point. And, in truth, there is nothing very unphilosophical in consenting to both these positions. A small dose of the poison in a perfectly healthy woman after delivery may run through its ordinary course, and be eliminated; while a larger dose, or one from a more virulent source, imported into the blood of a less healthy woman, may produce a more malignant type of disease, not having its ordinary manifestations, just as some of the worst cases in an epidemic of cholera are not attended by the usual symptoms of vomiting and purging.

There is, however, a fallacy in reference to the occurrence of scar-



latina in the puerperal patient which must not be overlooked. As stated by Dr. de Garrequer Griffiths and others, there may be a rash closely resembling the scarlet fever eruption which is due to toxemic poisoning of another kind, and which may be mistaken for scarlatina.

I recollect that it was a current opinion among the medical men in a crowded manufacturing town where I first studied medicine, that scarlatina communicated to a lying-in woman was almost certain death; and I was early impressed with the danger of these cases by knowing a medical man who was believed to have carried scarlet fever to the ladies of three county families, and so caused their death. I have since learned that scarlet fever is not so universally fatal as I then supposed; and the history of Queen Charlotte's Lying-in Hospital, which has been kindly furnished to me by Dr. Blakeley Brown, conclusively proves this. From 1857 to 1871, there were twenty-six cases of scarlet fever in the hospital. Of these, two were removed to St. George's, and the results are not stated. Of the remaining twenty-four, six died—a fourth of the entire number attacked. I still, therefore, cherish a wholesome horror of scarlatina in the lying-in room.

There can be no doubt that lying-in women are prone to be affected by any febrile diseases to which they may be exposed. It matters not whether it be scarlet fever, measles, small-pox, or other zymotic disease; unless they be protected by a previous attack, the affection is apt to break out with more than ordinary severity, with graver complications, and frequently tends to a fatal termination. During pregnancy, the accession of one of the exanthemata will often bring on abortion or premature labour, showing the profound impression made on the system. Dr. Barnes even has suggested that the protection afforded by a previous attack of scarlet fever breaks down under the trial of childbirth. And it is to be remarked that, because certain patients have escaped easily, or, it may be, on one or more occasions it has visited a series of patients lightly, these examples do not vitiate the rule. There is obvious danger in dogmatizing on questions concerning which we are but imperfectly informed as yet, and in arguing only from negative results. By taking too exclusive a view in the case before us, we may fall into pathological error, or, what is worse, neglect precautions necessary for the safety of our patients.

If the evidence be true in reference to the exanthemata, it is even more forcible, and borne out by a greater concurrence of opinion concerning the serious peril encountered by the puerperal woman when exposed to the poisons of erysipelas, diphtheria, and typhus. Mr. Wells and others have mentioned instances in which there could be no doubt that puerperal fever was produced by direct or indirect communication with erysipelas; and other Fellows had painful experiences to relate, had time permitted, of puerperal deaths caused unwittingly by going from patients having erysipelas to the lying-in



room. The connexion between the two has long been noticed ; and observations have accumulated with such force and frequency as to constitute absolute proof of close affinity. Many new-born children have died of erysipelas whose mothers were the subjects of puerperal fever ; and instances have not been rare where the medical man attending a puerperal fever patient has from her contracted erysipelas, and died also.

My friend Dr. Fordyce Barker, who contends strenuously for the specific character of puerperal fever, admits in his larger work, and also in a recently published lecture, that the close relation, or what he calls the "solidarity," between puerperal fever and erysipelas is conclusively settled. He considers, however, that, although they are interchangeable, they are distinct diseases. I fear that logically this acknowledgment of interchange of symptoms is somewhat adverse to the theory that identity of cause will necessarily produce identity of result. In recognition of the same mutual relation, Virchow speaks of puerperal fever as *malignant puerperal erysipelas*.

A connexion has also been remarked between puerperal fever and diphtheria. The late Professor Martin, of Berlin, probably erroneously, came to regard the diphtheritic process in the genital organs of lying-in women as the only essential element of puerperal fever. That there may be a connexion at times seems certain. Dr. Playfair has brought forward some evidence on this point ; and Dr. Fordyce Barker, Dr. Lusk, and others in America, have described the diphtheritic process as occurring in certain puerperal fever epidemics in the hospitals there.

In reference to typhus fever, again, there is evidence that, if communicated to a woman after delivery, it may produce a virulent and fatal form of so-called puerperal fever. Among the examples are those mentioned by Dr. West and Dr. Beck. In one instance, after the introduction of a case of typhus fever into the Dublin Lying-in Hospital, "puerperal fever appeared in forty-eight hours afterwards, although the hospital had been free from it for a year before." Sir Henry Marsh, indeed, from seeing the frequent relation between the typhus and epidemic puerperal fever, remarked that "the true character of epidemic puerperal fever seems to be typhus."

The proclivity to be affected by prevailing febrile diseases, or to initiate them, is no doubt due to the peculiar constitution of the pregnant and puerperal women described at length by Dr. Richardson and Dr. Arthur Farre. The excess of fibrin in the blood, and the diminution of the salts, exist even before parturition. Afterwards, the sudden arrest of the utero-placental circulation, and the consequent effect on the maternal blood-current, with the later initiation of the milk-process, and the involution of the uterus, furnish, as it were, so much combustible material. All constitute so many disturbing forces, which, added to the susceptibility of the nervous system, must render even the healthy woman sensitive to febrile disturbance, and favour the development of rapid changes in the composition of the blood, if

only a small leaven of ferment in any form be added to it. Moreover, from the present artificial habits of society, few parturient women are so healthy as they might be, and, consequently, they become a more ready prey to disease.

A series of predisposing causes common to other diseases as well as to puerperal fever has been enumerated. Dr. Newman, of Stamford, has particularly drawn attention to the baneful effects of sewage-air and depressing mental conditions. All these may so lower vitality, that even the occurrence of a chill may develope febrile conditions. The greater proclivity of unmarried women to puerperal fever has been remarked by Campbell, Home, Leake, John Clarke, and others; and there are some striking illustrations of the effects of mental influences in provoking the disease given by Professor Lusk in the *American Medical Journal* last November.

There seems to be literal truth in the remark made by Dr. West, that "there is not one single solitary cause to which we can refer the symptoms of puerperal fever; that it occurs now from one cause, now from another." With equal truth, also, it may probably be asserted that there is not one puerperal fever, but several, arising from a diversity of causes. The term puerperal fever has been somewhat loosely but habitually applied to all diseases of lying-in women which were attended by febrile symptoms and tended to a fatal termination. In attempting a more accurate pathology, it is well to bear in mind that fever is only a symptom of disease; and that we have to find out what is the underlying disease, of which the fever is but the indication. Perhaps the best practical classification of puerperal fevers which has been suggested, in accordance with our present knowledge, is: 1. The ephemeral forms which depend on some transient cause; and the graver class, divided into (2) the auto-genetic or auto-infective form, and (3) the heterogenetic or hetero-infective, in which the poison is imported from some zymotic or other infective source, and the origin of which can be traced. Notwithstanding this classification, there will always be cases the origin of which it is difficult or almost impossible to detect. These must necessarily form a class by themselves, until further knowledge enables us to assign to them their proper place.

4. *Epidemic and Endemic Characters of Puerperal Fever.*—A warm discussion has taken place as to whether puerperal fever is ever really epidemic; that is, dependent on general atmospheric or other influences which pervade a community, as distinctive from the miasms of unhealthy hospitals or the propagation of the disease from one patient to another by inoculation. Dr. Matthews Duncan is inclined to doubt the evidence of its epidemic character altogether, and to regard it as essentially endemic. There is no doubt, as he suggests, that the word "epidemic," as applied to an outbreak of puerperal fever, has been at times used by authors carelessly, and that "endemic" would have been the more appropriate term; but the annals of medicine still afford undoubted evidence of the epidemic

character of puerperal fever at definite epochs, and, if we turn to the histories by Hirsch, by Dr. Robert Ferguson, and others, beside the speech of Dr. Fordyce Barker in this Society, we shall find that it occurred extensively in certain years among all classes of the community, in hospital and out of it, in town and country. In 1821, for example, it occurred as an epidemic in France, in Holland, in Scotland, and in London at the same time; while, further, it has been noticed that, when puerperal fever prevails epidemically in the human subject, the lower animals die in large numbers of diseases connected with parturition. I confess I cannot see what is gained in argument by those who contend that puerperal fever is essentially a form of pyemia (using the term pyemia with the latitude they attach to it), by denying that puerperal fever is at times epidemic, unless it be desired to prove that the disease always originates in some local inflammation of the generative organs, and that the febrile symptoms are only the natural effect of the intense constitutional effect so engendered. This theory of the localists, as it has been called, was supported by Professor Meigs, Professor Pajot, and others, but has received little countenance in this country. Erysipelas is so undoubtedly and frequently epidemic in its character, has so commonly a spontaneous or idiopathic origin, apart from its association with traumatic injuries, that, if the mutual interchange between puerperal fever and erysipelas be admitted, the epidemic character of both follows as a matter of course.

The influence of a vitiated atmosphere in overcrowded hospitals in producing an endemic form of the disease is only too well confirmed. Whenever a number of lying-in women are aggregated together, there is danger that a miasm may be generated which will develope puerperal fever, and it is by no means easy to define the amount of ventilation and isolation which are necessary to prevent these untoward consequences. Hence a feeling has been gradually growing up that, notwithstanding the economic and educational advantages of lying-in hospitals, poor women are more safely confined at their own homes. In the recent congress on maternity hospitals at Brussels, M. Lefort stated that, in Paris outside the hospitals, the mortality had been one in 212, while in the Maternity Hospital it had been one in 29. Fortunately, the statistics of maternity hospitals in this country are not so unfavourable, but the mortality in them even is much greater than when patients are delivered at their own homes. At King's College Hospital we had a notable instance of the very serious results provoked in puerperal women by their being confined under the same roof with patients in general medical and surgical wards. The mortality in the lying-in wards gradually increased year by year, until at length it reached the fearful maximum of nearly one in every thirteen women delivered, and then it was determined to close the midwifery department altogether.

5. *Contagiousness of Puerperal Fever.*—Turning to Mr. Wells's fourth interrogatory, which bears upon the contagious or infectious



character of puerperal fever, I find there is no question among the various speakers that the disease may be propagated from one patient to another, although there may be some difference of opinion as to the way or means by which it is communicated, and some forms are regarded as less likely to be infectious than others. For a long time, the contagiousness of puerperal fever was disputed, and in Paris writers and practitioners remained unconvinced of its contagiousness long after it had been conceded elsewhere. In our day, the direct proofs of its contagious quality have accumulated to such an extent, that to disregard them in practice would amount, on the part of a medical practitioner, to a plain dereliction of duty. So far as some of the forms are concerned, however the attack may have originated, if it be but a single case, it may become the focus and centre of propagation to other women. And, of the several ways in which the subtle poison may be conveyed to the lying-in woman, none is more certainly proved than that it may be conveyed by the hands of the practitioner in making vaginal examinations. But this is not the only way. It may probably be communicated through the clothes, instruments, respiration, or even the secretions of the medical practitioner, midwife, or nurse. That it may be so spread, we have ample evidence, if we look back to the records of Gordon, Armstrong, Gooch, Ramsbotham, and others. Dr. Roberton, of Manchester, gives one of the strongest instances possible. In the space of one calendar month, a midwife attended twenty cases belonging to a lying-in charity; of these, sixteen died of puerperal fever. Other midwives of the same charity attended 380 cases in the same district and at the same time, and none of these were affected by the disease.

For further evidence on this subject, I may refer to the works of Dr. Tyler Smith, Barnes, Routh, and Semmelweiss; and I can particularly recommend the perusal of the book published by Professor Wendell Holmes, one of the most charming and original of writers on general as well as on medical subjects.

There is very cogent collateral evidence towards proof that the blood of a puerperal patient may be infected by other channels than by the genital canal. It is certain that puerperal fever may begin in a patient before her delivery. Dr. Barnes's statement that he has been conscious of absorbing poisonous and offensive emanations from patients with putrid discharges, and that he has suffered from shivering, diarrhea, and foul breath as the result, affords a very strong presumption that an attendant may go about charged with an infectious disease, and convey an atmosphere about him which shall be most deleterious to susceptible patients. There is a very ingenious exposition in reference to this matter in Dr. Tyler Smith's "Manual on Obstetrics," and he strengthens the position by reference to cases in which pregnant women who have had small-pox in childhood, and are proof against infection, have nevertheless, as in Dr. Cordes's case previously mentioned, on being exposed to the poison, conveyed the disease to the fetus *in utero*.



My friend Dr. Moir, a former President of the College of Physicians in Edinburgh, has sent me the following short history, which seems to indicate that a diseased child *in utero* may infect the mother with a form of pyemia which may be transmitted on to other lying-in women. I quote his own words:—

“Mrs. H., who had had several previous very easy confinements, had threatenings of labour on a Monday. Instead of a rapid delivery, as usual, she had slight pains and much malaise all the week, and was confined latish on the Saturday evening. Child above the average in size, about 9 lbs., fat and healthy-looking. On Sunday forenoon, the child died very unexpectedly and without apparent cause. On examination next day, it was found that one pleural cavity was filled with thick purulent matter, the lung being forced back and, of course, undistended with air. The mother never had milk, as usual, fevered with much general pain, and afterwards had all the symptoms of puerperal fever. Large abscesses formed in various parts of the body, some being opened, which gave partial relief. They were on the legs, arms, chest, and a very large one implicating almost the whole dorsal muscle. This one was not opened. The pyemia in this case was evidently existing before delivery from the symptoms, and from the child's being affected. So much for Mrs. H.

“Two hours after her child was born, I was called to Mrs. W., who had had several children: easy cases. She was ill only two hours; took rigors on the third day, and died about six days after delivery, with all the symptoms of puerperal fever. Two hours after Mrs. W. was confined, I was called to Mrs. F., who also had an easy delivery; had all the symptoms of puerperal fever; but recovered after a hair-breadth escape, not recovering for six months. Observe: the three cases were all over within eight hours. I undoubtedly carried the infection from Mrs. H., whom I had been seeing constantly during the six days she was ailing. She survived the delivery about twelve days; Mrs. W. only about six.”

But are all forms of puerperal disease attended by febrile disturbance equally infectious? This is an inquiry which bears with moment on the necessity for precautions on the part both of doctors and of nurses, and the question is constantly being raised, Should a nurse who has been attending this or that modification of the disease be permitted to pass on at once to the charge of some other expectant patient? On this point, Dr. Hicks says “that the majority are contagious, I have no doubt; whether all are so, I am uncertain. I am inclined to think that those forms derived from the zymotic diseases are the most so; those from the self-generated kinds are least so.” Dr. Routh concludes that some forms are contagious and others are not so; and Mr. Wallace believes that it is “what would be called the pyemic cases which are non-contagious.” Dr. Barnes, again, states that “the autogenetic forms proper do not appear to possess active powers of propagation;” and Dr. Fordyce Barker, concurring in this remark, regards this difference of contagious property as one of the proofs that puerperal

pyemia or septicemia are generally different from true puerperal fever. I take it, then, that opinions coincide pretty generally that some forms are highly contagious and infectious, others are less so; and it is probable that those arising from extraneous sources are most contagious; those from poison generated in the patient's own tissues least so; or perhaps I may put it in a somewhat more practical form, as a broad rule for guidance, at the risk of being less scientific and precise, that the most acute forms, running to a rapid termination, are most likely to be highly contagious; those which run a more chronic course, and are attended by secondary complications, are least so. Nevertheless, as we do not know yet that even the chronic cases may not in a minor degree be infectious, it behoves us to exercise caution in attending these cases, to purify nurses who have been in charge, and, above all, to be careful not to convey any of the discharges to other lying-in patients.

6. *Relation of Bacteria to Puerperal Fever.*—I quite concur in what has fallen from the several speakers concerning the relation of bacteria and other allied forms to puerperal diseases. We are obviously only on the threshold of that important inquiry; and although the researches of Klebs, of Waldeyer, of Mayrhofer, of Lister, of Sanderson, of Heiberg, and Orth, have established a nearly connecting link between bacteria or other allied organic forms and pyemic processes, the field of investigation is rather one for the pathologist and microscopic observer than the obstetrician. The gynecologist would, nevertheless, thankfully receive any information which would enable him to cope successfully with these terrible maladies, either in the way of prevention or cure.

7. *The Prevention of Puerperal Fever.*—I turn now to the last division of the subject, which deals with the prevention of puerperal fever, and with what Dr. Playfair calls "the burning question," the conduct of the medical practitioner in face of perils of this kind. And first I would remark, that I concur with much that is contained in the letter of our distinguished honorary Fellow, Dr. Matthews Duncan, which was read at the May meeting. It would be most disastrous to our profession, and eventually injurious to the public interest, if medical men were liable to vexatious or criminal prosecutions because they were suspected of carrying infection from one patient to another. Members of the medical profession do not need to be reminded by such measures that they pursue their avocations for the public good. I believe no class of men practise more self-abnegation in the exercise of their work than doctors do; and the perils to which they are constantly exposed almost equal the risk of the battlefield. This unselfishness has become, in a manner, traditional, and together with it a courage and fearlessness in doing what they consider right at all hazards. I would, however, earnestly warn my brother practitioners not to be misled by some inferences which may be drawn from parts of Dr. Duncan's letter, however high his authority, and, as a consequence, indulge in a laxity of precautions, when such momentous

interests are at stake. I cannot but think that if any member of our profession had pursued the same course as Mrs. Marsden, under like circumstances, his conduct would have been most reprehensible; and I agree with Mr. Spencer Wells, that he would not be deserving of the sympathy and help of this Society. It is certainly better to err on the side of over-caution than of over-laxity.

The method, therefore, to be pursued for guarding the lying-in woman from noxious influences, consists of all those measures which prevent the formation of poisonous materials in her own system, and which secure her isolation from all contagion from without. In following out the first indication, it is necessary to provide, as much as may be practicable, for a woman encountering her confinement in the best possible condition of health, by impressing upon her the necessity of obedience to natural laws during her pregnancy. If complications occur during pregnancy, they must be combated as the circumstances will permit. Dr. Barnes has informed us that albuminuria at the end of pregnancy is extremely apt to go into puerperal fever. In these cases, therefore, a sedulous watch must be kept, the bowels must be carefully attended to, and other suitable treatment for these cases adopted. The process of parturition should be conducted with the view to the genital canal of the woman being exposed to the effects of irritation, continued pressure, and laceration, as little as may be possible. After the birth of the child, a full and perfect contraction of the uterus should be secured, by seeing that the organ is not emptied too suddenly, and that the hand follows the fundus down from above as its contents are expelled. Subsequently to delivery, the various known methods should be practised to promote the contraction of the walls of the womb and the diminution of the uterine cavity; and any clots forming, should be removed from time to time during the hour after removal of the placenta. It is superfluous to say that the placenta should be extracted in its entirety, when possible; and great care should be taken not to leave any detached portions adherent to the uterine walls. The same precautions should be observed, when practicable, in cases of abortion. If any considerable laceration of the perineum have taken place, the edges of the wound should immediately be united by sutures, not only for the purpose of restoring the perineum, but also to prevent contamination of the wound by putrid discharges. In the case of other wounds in the vagina or cervix uteri, especial care should be taken to keep them clean by repeated injection, and to leave as small a raw surface exposed as may be possible. In all cases where the lochia are in the least degree offensive, the vagina should be well injected with Condyl's fluid and water, or other innocuous disinfectant, twice in twenty-four hours, or oftener if necessary; and the injection may be carried into the uterine cavity, if it be much distended, and there is a suspicion that it harbours fetid contents. The injection of the womb-cavity, however, should be conducted slowly, carefully, and without force. These uterine injections were practised long ago by



William Harvey, and there is concurrent testimony in this and other countries of their marked utility in abating the symptoms even when puerperal fever has apparently set in.

Next in importance is to take care that there is no fecal accumulation in the bowels, and to recollect that the existence of previous diarrhea may be the indication that retained masses are lying in the intestines and producing irritation there. Dr. W. T. Greene has alluded to this during the debate; and it must be within the experience of many how acute febrile symptoms, accompanied, it may be, by delirium or incipient mania, have passed away with a proper evacuation of the bowels. I may put it, in short, that, to promote recovery after parturition, every direct or reflex source of irritation should be removed, pure air and water insured, absolute cleanliness observed; and perfect quietude, mental and physical, should be enjoined. Nursing the child should also be recommended when the mother's condition permits, as puerperal ailments have been observed more frequent in women who have not suckled their children.

To secure the isolation of a lying-in woman from noxious influences which may be communicated to her from without may be difficult, and in some cases perhaps impossible; but as Mr. Callender has informed us that surgeons have learned to banish, for all practical purposes, those affections which may be termed septicemic from surgical wards of hospitals, so I cannot help thinking that the accoucheur will in time succeed in preventing the like affections in puerperal women, so far, at least, as the heterogenetic cases are concerned. We are all agreed as to the absolute necessity of preventing, directly or indirectly, any communication between cases of erysipelas and puerperal patients. We are agreed also as to the necessity of the midwifery practitioner avoiding all post-mortem examinations. His hands should avoid all contact with specific poisons or septic materials; and if perchance his fingers have touched anything suspicious, he should at once carefully and thoroughly disinfect them. The necropsies which seem most baneful are of those bodies which have recently died of erysipelas, peritonitis, zymotic disease, or any other inflammatory and febrile affections; and in such instances the accoucheur should not even be present in the room when the dissection is made, as, though he decline to touch, yet his person and clothes may become infected by the poison. There may possibly be a doubt whether one who dissects bodies altered by decomposition or antiseptic injections, as they are commonly found in the dissecting-room of medical schools, is so liable to contaminate a puerperal patient as he who makes a necropsy soon after death. Semmelweiss, however, believed that animal matter in a state of putrefaction would cause puerperal fever, and immediately lessened the mortality, during the fearful epidemic of puerperal fever which raged in the Vienna Hospital in 1846 and 1847, by obliging all dissecting students to wash their hands in chlorine or chloride of lime before making vaginal examinations. Impressed by Semmelweiss's views and



opinions, I endeavoured, during my occupancy of the chair of Obstetric Medicine at King's College, to prevent students frequenting the dissecting-room and post-mortem theatre from attending midwifery cases, and the result was a very small mortality in the outdoor maternity charity.

This care about absolute cleanliness of the hands in attending labours should extend even to occasions when a medical man has had digitally to examine patients suffering from offensive discharges either proceeding from the surface or from the interior of the body. We have heard in this discussion that the discharges from cancer may produce fever in a lying-in woman ; and this is probably true of other putrescent discharges. I have had grave anxiety, extending over some weeks, about a patient after her delivery ; because I had gone straight from an instrumental case where labour had been so protracted that febrile symptoms had set in, and the passages had become influenced, so as to pour out an irritating muco-purulent discharge.

When a medical man has a bad case of puerperal fever in his own practice, or is required to see one in consultation, he should certainly not go direct to another midwifery patient without first changing his clothes, besides careful ablution of his hands ; and he will be all the safer in his ministrations if he adopt some of those precautions mentioned by Dr. Swayne and others, such as using a Turkish bath, or the more radical measures for disinfection recommended by Dr. Wynn Williams, and for the details of which I may refer to his speech.

Many chemical substances have been recommended to be added to water for purifying the hands : iodine, chlorine and its compounds, sulphurous acid, cyanide of potassium, carbolic acid, and the permanganates. It matters not which agent is employed, so long as it is used carefully and efficiently ; and if it be necessary to disinfect clothing, this is readily done by exposing it in an oven to a high temperature, for which many upholsterers have a suitable arrangement. With all deference to Dr. Matthews Duncan, I have no doubt that every medical practitioner who earnestly desires to promote the welfare of his patients and to keep himself blameless should at once abstain from attending fresh cases of labour when one or more of those recently delivered by him have died with acute febrile symptoms, or are still so ill as to require his constant visits. When, indeed, there is the remotest suspicion that he may convey the poison in his own person, he should not incur the risk of disseminating it. How long he ought to absent himself from midwifery practice, is yet a moot point. Dr. Swayne thinks a medical man should seclude himself for a week only ; others think a month hardly sufficient. The remarkable experience of Dr. Huntley of Yarrow led him to believe that he actually generated poison in his own body, as the result of some faulty process, which he communicated to his patients. Taking all the circumstances into consideration, I am disposed to recommend a week's seclusion after regular attendance has ceased on a single puerperal fever case. When a series of cases have occurred in the

practice of any one medical man, he should absent himself from midwifery practice for a month at least. During either the short or the longer interval of seclusion, the means for disinfection should be fully carried out.

Finally, I have to say a word or two as to the propriety of attending patients suffering from scarlet fever or other zymotic disease, and lying-in women, at the same time. After what has been said in this debate, I cannot expect the same accord of opinion as on other parts of the ground I have gone over; but I would earnestly beg those who have as yet had so favourable an experience, when attending the two sets of patients conjointly, to ponder well what has been said by others on the reverse side of the question. It cannot be expected that men in general practice, who may be in charge of a scarlet-fever or small-pox patient, shall at once relinquish all midwifery practice for the time being, because zymotic diseases are so prevalent that this would practically preclude their attending confinements altogether, or make their attendance on midwifery patients so irregular as to be unreliable. Nevertheless, in view of the dangers which have been indicated by various authorities, albeit their experience may seem to point to different conclusions, they are bound to exercise vigilance, lest perchance they slip into a pitfall unsuspected by them. In seeing an ordinary case of illness, a medical man rarely stays sufficiently long in the sick-room to concentrate any considerable dose of contagium about him, and the after-exposure to fresh air in passing from house to house no doubt usefully dissipates any smaller quantity. I would suggest, however, to those treating infectious cases, and liable at the same time to be summoned to cases of midwifery, or who may be in attendance on puerperal women, that they should not stay long in the sick-chamber; not undertake the duties of nurses, as Dr. Duncan puts it; that they should see puerperal patients before seeing infectious cases, and never go from the infectious case direct to the lying-in room, without changing clothes and the most careful ablutions. Some doctors change their clothes and wash with a disinfectant whenever they have seen infectious cases. Whatever either will, or may, conduce to the well-being of patients, will, I am sure, not be regarded as either too irksome or troublesome by any member of our profession.

---

Since the last annual meeting the Society has to lament some grievous losses among its Fellows. Foremost among those who have been taken away from us by death, I have to mention the name of our revered honorary president, Sir Charles Locock. Sir Charles was born at Northampton in 1799. His father was a general practitioner in good position, and from him Sir Charles learned in early life the technical details in dispensing and combining drugs, which caused his *formule* in after life to be regarded as patterns of accuracy and appropriateness conjoined. He studied medicine at Edinburgh, mainly under the then celebrated Hamiltons, and took his M.D. degree there

in 1821. Coming soon after to London, he attracted the notice of the eminent Dr. Gooch, who earned for himself a reputation which still remains as the most accomplished and elegant of obstetric writers. Sir Charles often repeated to his friends how Gooch, on their first acquaintance, had sent him in the night to an urgent case, which proved to be one of rupture of the uterus. Locock delivered the woman, and gave an opiate. Next day the patient was found by Dr. Gooch so comparatively well that he doubted the diagnosis of his junior, and told him so. Locock, however, adhered to his opinion, and its correctness was proved by the woman's subsequent death and examination. This was the turning-point in Locock's career. Dr. Gooch formed so high an opinion of the young man's capabilities, that he soon put work in his way, and as his own health was declining, he began by degrees to transfer his entire practice to him. Dr. Cumberbatch, who knew him intimately, and to whom I am indebted for many details of his life, assures me, on Sir Charles's own authority, that at the age of thirty he was making an income of 3000*l.* a year, which went on steadily increasing. At one time in his early career Locock was supposed to be phthisical, and was condemned as a "poitinaire." This tendency he outgrew, and he has repeatedly told me he attributed his recovery to driving in all weathers in an open carriage. Besides being one of the most busy and popular of physicians-accoucheur in private practice, he lectured on midwifery, first at St. Thomas's Hospital and then at St. Bartholomew's. At St. Thomas's he lectured in the winter at the early hour of 8.30 A.M., and was not absent from a single lecture during the whole session.

He increased his practical experience by diligent attendance to his duties as physician, first to the Westminster Lying-in Charity, and later to the General Lying-in Hospital at Lambeth. His medical writings were not numerous, but he contributed gynecological articles to the "Cyclopedia of Practical Medicine," and afterwards to the "Library of Medicine," edited by Dr. Tweedie. Some of the best known and esteemed of these are the essays on amenorrhea, dysmenorrhea, leucorrhea, and menorrhagia. All are marked by acuteness of observation, and bear evidence of careful clinical study. There was a remarkable practical faculty observable in all Sir Charles Locock did—in his writings and in his daily work. He had a most extensive and versatile experience, which served him in good stead, but besides this he had a rapid power of generalizing details, which often made his diagnosis seem as rapid as it was generally correct. His conscientious fulfilment of what he considered his duty to his patients may serve as a model of its kind. However pressing and urgent other summonses might be, he adhered to his rule of not leaving a parturient patient until an hour after her delivery, and he achieved a great reputation for preventing after-pains, his method consisting simply of keeping up pressure over the fundus uteri, and removing coagula so long as they formed in the os uteri. I am informed also that he was the first to give a dose of ergot before the last expulsive pains in



cases of threatened post-partum hemorrhage ; and in 1857, when President of the Royal Medico-Chirurgical Society, he drew the attention of the profession to the utility of bromide of potassium in certain forms of nervous disorder, associated with excitement in the reproductive organs.

Sir Charles was singularly free from strong prejudices, and even late in life was ready to welcome any innovation in practice which gave promise of genuine advantage. When chloroform was introduced into midwifery practice, Sir Charles was one of the first to make use of it and testify to its utility. But the most characteristic features in Sir Charles were his unfailing kindness and his perfect straightforwardness. Beneath a somewhat undemonstrative and cold exterior there was the most sympathetic nature, and those who knew him best were the most warmly attached to him. To his patients he was friend as well as physician, and few men exercised the influence he did, so long as his activity continued. There was a certain directness and clearness in the way he expressed his views and in the directions which he gave to his patients which showed that he had confidence in himself and was master of his subject. This necessarily inspired confidence in others and insured the obedience of his patients. Notwithstanding his overwhelming practice, he had unfailing animal spirits, and his sense of humour even in difficulties was unflagging. He was one of the men that prosperity did not spoil, and who cherished old friendships as well as created new friends by acts of kindness. I bear in grateful memory the recollection of his helping hand extended to me when coming a comparative stranger to London with a letter of introduction. Sir Charles not only wrote me a testimonial for a public appointment, but actually came and voted for me personally. When the Obstetrical Society was founded, he was so pre-eminently the head of the obstetrical department of practice that he was chosen by common consent its honorary president. It is well known that Sir Charles was first Physician-Accoucheur to the Queen, by whom he was greatly esteemed. He attended her at the birth of all her children. Her Majesty, indeed, showed her great regard for him by visiting him during his illness, and expressing her sorrow for his loss. After a most useful and exemplary career Sir Charles died at Binstead Lodge, in the Isle of Wight, on the 23rd of July, 1875, full of years and full of honours.

Sir Charles was a Fellow of the Royal College of Physicians ; a Fellow of the Royal Society ; a D.C.L. of Oxford, and a member of several other learned societies. He was created a baronet in 1857, and he was a Deputy Lieutenant of the county of Kent.

The next death I have to record is that of one of our honorary fellows, Professor Edward Martin of Berlin, and I have to acknowledge the courtesy of Prof. Otto Spiegelberg of Breslau in furnishing me with materials for a short biographical sketch.

Prof. Martin was born at Heidelberg in 1809, and was first a law student, but eventually elected to study medicine, and became a pupil of the celebrated Naegele. He graduated at Göttingen. In



1837 he began to lecture on midwifery at Jena, and soon after was appointed director of the newly erected Clinic and Policlinic Institute for Midwifery in the same university. In 1858 he was advanced to the chair of Midwifery and Diseases of Women in Berlin, in succession to Prof. Busch. Among his principal works I may mention essays on "External Version," "On Transfusion in Puerperal Hemorrhage," and "On Diseases of the Ovaries." He was one of the first who practised ovariectomy successfully in Germany, and who followed extra-peritoneal treatment of the pedicle. Besides, he published a "Handbook on Midwifery, for Midwives," a "Gynecological Atlas," a work on "Flexions and Versions of the Uterus" (two editions), and formerly he was one of the editors of the *Monatsschrift für Geburtskünde*, which ceased to appear after the issue of the *Archiv für Gynäkologie*. In the last year of his life he published the first number of the *Zeitschrift für Geburtshilfe und Frauen Krankheiten*. Dr. Martin was a good type of the hardworking, painstaking German professor, who pushed himself to the front by dint of sheer industry and merit. He was for some time the leading obstetric authority in Berlin, and was selected to attend the Crown Princess of Prussia (Princess Royal of England) in her first confinement. He was a kind-hearted, genial man, and many Fellows of the Society will look back with pleasure to having made his acquaintance when he paid a visit with his son to this country some ten years ago.

Among our ordinary Fellows we have eleven deaths to lament—viz., Joseph Dickson, M.D., Jersey; James Clark, M.D., Regent's Park Road; Philip Rowling Heeman, F.R.C.S., Clifton; George Shaw, L.R.C.S. Edin., Battersea; J. Myers Briggs, Coeymans, New York; Joseph Maldon Dempsey, M.D., Charterhouse Square; John Ward, M.R.C.S., Penistone, Sheffield; Edward Leech, M.R.C.S., Chichester; Thomas Brown, M.D., 236, Kennington Park Road; Edward Lorenzo Desmond, M.D., Liverpool; and Dr. Squary, London. All except the two last were, I believe, engaged in general practice, and did good service to the commonwealth in their several capacities. Dr. Desmond was a prominent obstetrician and gynecologist in Liverpool, surgeon to the Lying-in Hospital, and was greatly respected for his high professional character and geniality.

The general condition of the Society is highly satisfactory. The number of Fellows continues to increase, and the funds are in a most prosperous state. The property belonging to the Society has so increased that its present location is no longer able to contain it, and it has become a serious question with the Council whether more extensive premises should not be acquired—opportunity serving—for a more commodious library, museum, and permanent home for the Society. The munificent donations of pelvis and gynecological casts, by our distinguished Honorary President, Dr. Arthur Farre, are in the meantime almost an embarrassment, but it is hoped ere long that some solution will be found for the difficulty, and that the preparations will be displayed in a manner worthy of their importance.

## OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, November 17th, 1875.*Dr. MATTHEWS DUNCAN, *President, in the Chair.**Sarcoma Uteri.*

By ALEXANDER RUSSELL SIMPSON, M.D.

It is one of the many services which Virchow has rendered to pathology, to have rescued the term Sarcoma from the somewhat vague senses in which it used to be employed, and to have applied it to a group of tumours whose source he has carefully traced, whose structure he has elaborately described, and whose relation to neighbouring groups of tumours he has clearly defined. They spring from a connective-tissue basis, and may therefore be found in all parts of the body where areolar tissue, bone, cartilage, skin, mucous membrane, and the cognate textures exist. In structure they have an affinity with the tissues from which they take origin, consisting of cells, and an intercellular substance freely traversed by blood-vessels. But they differ from the simpler homologous growths—such, for example, as a fibrous tumour—in that the development and increase of the cells predominate over the development of the intercellular substance, whilst the cells tend to assume distinct forms; and from the heterologous growths, the carcinomata, they differ in that there is a distinct formation of intercellular tissue, though of rudimentary type, and not a mere infiltration of cells among the pre-existing tissues. The cells present either, 1st, a fusiform or oat-shaped outline, when we have the spindle-celled sarcoma; or, 2nd, they are more distinctly circular when we have the round-celled variety. Sarcomata with stellate cells and with large myeloid cells are found more rarely, and only in special situations.

Such tumours have long been occasionally noticed as occurring in the uterus. In his chapter on Fibro-plastic or Sarcomatous Tumours, Lebert<sup>1</sup> gave the history of a case, with microscopic drawings, where the disease sprang from the cervix uteri. Hutchinson recorded a case of intra-uterine sarcoma very fully in the *Transactions of the Pathological Society of London* in 1857,<sup>2</sup> under the designation of Recurrent Fibroid Tumour of the Uterus. In the following year<sup>3</sup> another case was brought before the same Society, under the like designation, by Callender; and the same cases, with a more complete clinical history, are related by West,<sup>4</sup> who had the care during life of the second patient, and to whom belongs the merit of giving the affection a place, though not clearly defined, among the diseases of the uterus. In his

---

<sup>1</sup> "Physiologie Pathologique," ii. 154, 1845.

<sup>2</sup> Vol. viii. p. 287.

<sup>3</sup> Vol. ix. p. 327.

<sup>4</sup> "Lectures on the Diseases of Women," 3rd edition, 1864, p. 328.

lecture on Sarcoma,<sup>1</sup> Virchow described its occurrence in the uterus from instances that had come under his own observation, and gave reference to previously-recorded cases. But it was not till after the appearance of Gusserow's essay<sup>2</sup> on sarcomes of the uterus that the subject received due attention, and gynecological literature began to be enriched by the observations of Hegar,<sup>3</sup> Winckel,<sup>4</sup> Spiegelberg,<sup>5</sup> Chrobak,<sup>6</sup> Leopold,<sup>7</sup> Paul Grenser,<sup>8</sup> Ahlfeld,<sup>9</sup> and especially by a more elaborate thesis by Kunert, an abstract of which is given in the *Archiv für Gynäkologie*<sup>10</sup> in 1874. So late as 1873 the subject is meagrely treated by Barnes,<sup>11</sup> and it is only in the works of Schroeder,<sup>12</sup> published last year, and in the last edition of Thomas,<sup>13</sup> which came into my hands two days ago, that a distinct chapter is accorded to sarcoma of the uterus. Let me add, that I have looked through the index of the *Transactions* of our own Society, and that of the *Transactions* of our younger but more prolific sister in London, without finding a reference to the subject, unless an interesting case recorded by Hall Davis<sup>14</sup> as one of intra-uterine fibro-plastic tumour belongs to this category. Let this be an apology for bringing four cases under your notice, and offering a few remarks regarding them.

The first case is one where we have probably to do with a uterine myoma or fibro-myoma undergoing sarcomatous transformation.

CASE I.—*Probable Fibro-myoma—Repeated Removal and Recovery—Development of Myo-sarcoma—Removal and Recovery—Development of Sarcoma—Removal of Portion, and Death of Patient.*

I first saw Mrs. B., a widow, of forty-six years of age, mother of several children, in consultation with Dr. David Gordon, of George Square, on 12th July, 1870. She was pale from great anemia, with a weak, quick pulse, and complained of great pelvic distress. The evacuation of the bladder especially was accomplished with great distress and difficulty. A tumour, the size of a child's head, was found rising above the pelvic brim, firm and fixed, and having all the characters of a uterine fibroid. On vaginal examination the pelvic cavity was felt to be completely occupied by a soft, fleshy, elastic mass, which the combined examination showed to be of a piece with the

<sup>1</sup> "Die Krankhaften Geschwulste," B. ii. p. 350, 1864-65.

<sup>2</sup> "Archiv für Gynäkologie," B. i. p. 240, 1870.

<sup>3</sup> Ibid., ii. 29, 1871.

<sup>4</sup> Ibid., iii. 297, 1872.

<sup>5</sup> Ibid., iv. 344, 1872.

<sup>6</sup> Ibid., iv. 549.

<sup>7</sup> Ibid., vi. 493, 1874.

<sup>8</sup> Ibid., vi. 501, 1874.

<sup>9</sup> Ibid., vii. 301, 1875.

<sup>10</sup> Ibid., vi. 111.

<sup>11</sup> "Clinical History of the Diseases of Women," p. 825. At p. 752 he quotes Hutchinson and Callender's cases of "recurrent fibroid."

<sup>12</sup> "Krankheiten der Weiblichen Geschlechtsorgane," p. 284. He gives the history with drawings of two cases observed by himself.

<sup>13</sup> "Practical Treatise on the Diseases of Women," 4th edition, p. 539, 1875. Has met with four cases, but their histories are not recorded.

<sup>14</sup> "Transactions of the Obstetrical Society of London," ii. 17, 1860.



supra-pubic tumour, and which occupied the whole anterior wall of the uterus. The os uteri could, with difficulty, be detected high up towards the promontory of the sacrum; and the pelvic excavation was so completely blocked up that catheterization of the bladder was almost impossible. The loop of a straight chain-écraseur was applied as high up on the growth as possible, and a nearly circular portion was removed, measuring  $4\frac{1}{2}$  inches in diameter, and  $2\frac{1}{2}$  inches in thickness. On section it presented a uniformly smooth surface of pale pinkish colour, with two islands in it presenting the familiar cotton-ball structure, and clear white glistening aspect seen on section of an ordinary fibroid tumour of the uterus, and separated from the softer surrounding tissue by a connective-tissue capsule. The larger part of the tumour was composed of fusiform nucleated cells, with an intercellular matrix having a fibrillated appearance, and running for the most part in small sections in parallel directions. It presented a marked contrast with the irregular wavy arrangement of the fibres and connective-tissue corpuscles seen in the fibromatous nodules. In some places among the bundles of spindle-cells there were to be seen rounded nucleated cells, rather larger than white blood-corpuscles.

The patient's earlier history, as Dr. Gordon informed me, presented nothing remarkable; her elder children having been born without any difficulty. But on 30th November, 1865, the birth at the full time of her sixth child was impeded by a soft elastic swelling, which was then, for the first time, discovered in the right side of the vagina, and which necessitated the use of the long forceps, which was applied by Sir James Simpson, Dr. Keiller also being present. Within a year, on 22nd October, 1866, having again carried a child to the full time, the delivery had to be terminated artificially by Dr. Keiller, who turned and extracted the infant by the feet. Except in connexion with her labours the patient had not complained of special distress arising from the tumour; but in the following year it began to be attended with a copious serous discharge, and to produce pain and considerable difficulty in micturition. In June, 1867, accordingly, a portion of it was removed with the écraseur by Sir James Simpson, with the effect of relieving the patient for a time from her discomfort. By November, 1868, however, her distressing symptoms had returned to such a degree, that removal of another slice of the growth was attempted. The tumour at that time must have been very dense and tough, for though the portion removed was of smaller dimensions than that which came away in my hands subsequently, Dr. Gordon tells me that the chains of two different écraseurs with which the tumour was seized snapped asunder, and Sir James had to cut through the constricted part with strong scissors. Again the patient was for a time relieved; and again the mass, which had been felt in the abdomen, seemed to lessen considerably. But on 3rd February, 1870, the vagina having again become blocked up, a third lobe of the tumour was removed; and on 24th February a fourth portion. The relief



experienced by the patient lasted till the following year, when she was fain to submit to an operation for the fifth time—the amputation in July, 1870, of the mass I have described.

After the operation the patient rallied speedily and satisfactorily, and the relief afforded by the operation tided her over three more years. In the spring of 1874, however, her old symptoms began to trouble her; the watery discharge became more profuse than ever; the dysuria returned; and in addition, in April she had a severe attack of menorrhagia, and her health began to deteriorate under repeated uterine hemorrhages. I saw her again in October, 1874, and found her greatly reduced in strength, with a waxy colour, and anxious expression of countenance, more pronounced than one sees in cases of mere anemia or marasmus. There was no marked elevation of the fundus uteri above the pubes; but the vagina was occupied by a tumour the size of the fist, with a smooth surface, and having a soft, sodden feeling, bathed with a copious, faintly putrescent discharge, and bleeding easily when somewhat roughly handled. We deemed it desirable to give her the chance once more of a temporary relief and rally, and on 31st October, with the *écraseur*, I removed the projecting mass, Dr. Ogilvie Will of Aberdeen being also present. The chain of the instrument cut through with great ease, and with something like the sensation one experiences in cutting through a cauliflower excrescence. The structure of this last amputated mass much resembles that removed in 1871, except that there are now no traces of fibroid tissue; the round cells are more numerous, and are found running in parallel rows in a fibrillated but gelatinous-looking matrix, and the spindle cells are plumper and shorter. There was a slight tendency to hemorrhage from the raw surface, which was controlled by perchloride of iron. The patient seemed to do well for two days, but was attacked with acute and extensive phlebitis in both the lower extremities, and on 13th November she died. A post-mortem examination was not obtained; but when I saw her shortly before her death, I found in the vagina, and springing from the wound surface, a fresh sarcomatous sprout about the size of half the fist, having the feel, and when exposed by separating the labia, the exact appearance, of a mass of proud flesh.

This, as I have said, appears to me to be a case where, after removal of a large portion of a myome or a fibro-myome, there has sprung up a neoplasm, which became more and more decidedly sarcomatous in its texture.<sup>1</sup> But the greater proportion of cases which have been recorded are sarcomata *ab origine*, and they present themselves under one or other of two forms. Either, 1st, they are circumscribed, and more or less solid bodies projecting polypoidally from some spot on the inner surface of the uterus; or, 2nd, they are more diffused and soft, and spread along the uterine

---

<sup>1</sup> Not unlike what occurred in a case recorded by P. Müller. *Archiv für Gynäkologie*, vi. 126, 1874.

mucous membrane. The next two cases are illustrations of the former.

CASE II.—*Intra-uterine Sarcoma—Removal and Recovery on two occasions—Death after five years and four months—Autopsy.*

Mrs. M., aged seventy-two, married forty-six years, and mother of five children, with some intercurrent miscarriages, was placed under my care in the year 1869 by Dr. Ritchie of Glasgow, who had instructed her to wear a vaginal ball-pessary, kept in position by a wire fixed to an abdominal band, to give relief from down-bearing symptoms, from which she had suffered for many months previously. When I first saw her she was a well-nourished, healthy-looking old lady, and I found, on inquiry into her symptoms, that besides having had for a long time a feeling of pressure and discomfort in the pelvis, she had more recently been surprised to notice occasional escapes of blood, menstruation being for many years at an end. On making a vaginal examination, I found the genital canals moist and relaxed, the os round and patulous, the cervix and body enlarged and softened; and when the fundus was pressed down with the left hand above the pubes, the finger of the right passed easily and painlessly into the cavity of the uterus. Within the os internum, I could thus feel very distinctly a soft polypus, which I judged to be of the size of a walnut. Not being prepared to attempt its removal, I prescribed ergot, with the view at once of checking the attack of hemorrhage, which had been the immediate occasion of my present visit, and of inducing uterine action to favour the further descent of the intra-uterine body. Two days afterwards the hemorrhage had ceased, but, on examination, I found the os firmly closed, and the cervical canal quite impervious to the finger. An examination with the sound shortly afterwards, however, showing me that the uterine cavity was enlarged to three inches in length, and occupied still by the foreign body, I dilated the canal with sponge-tents, and partly with forceps and partly with the finger-nail, broke down and removed the morbid growth which I found springing from high up in the cavity. It was too soft and friable to be removed in any other way, and I applied solid nitrate of silver to the ragged bases. She had no bad symptom after the operation, and for some months was in the enjoyment of good health. About eight months subsequently, however, there was a recurrence of the escape of blood; and she began now to complain more of leucorrhœal discharge, and as the os relaxed occasionally when the bleedings came on, so as to permit the passage of the finger into the uterine cavity, a new mass could be detected pressing into the os internum. In August, 1870, exactly a year after the first operation, I again dilated the canal, and removed, with the assistance of Dr. Munro, into whose care the patient was about to pass, a portion of the diseased mass the size of an egg; but it was felt to have acquired a more extensive attachment along the back wall of the uterus, and its com-

plete extirpation, because of the bleeding set up in the very friable substance, could not be effected. Again, for nearly a year, she enjoyed comparatively good health, although the thin, pale, watery discharge soon reappeared, and by-and-by became very profuse; and for the relief of her pressure symptoms, which were sometimes accompanied with frequent micturition, and even retention, she had to wear a Hodge's vaginal pessary. After this time the hemorrhages recurred, and the profuse watery discharge became offensive. Early in 1872, I saw her along with Dr. Munro, when the uterus was found considerably enlarged, the orifice freely open, and occupied with a portion of the neoplasm hanging through in a sloughy condition, and giving rise to a profuse dirty discharge, having such a fecal odour as to give rise to the suspicion that a fistulous communication had formed between the rectum and vagina. Her general health was now greatly impaired, her pulse weak, and her skin waxy, and it seemed that any attempt to remove more of the tumour might be likely to lead to a fatal collapse. Means were therefore used simply to keep up the patient's strength, and a portion of the tumour having been expelled, she rallied to a remarkable degree. From time to time, however, as Dr. Munro has informed me, she suffered from increase of the watery discharge, and under a good deal of sickness and constitutional distress, the os uteri became more expanded, and pieces of the tumour were thrown off, sometimes as large as a pear, and invariably with some degree of hemorrhage. These attacks came on every six or eight weeks; and on several occasions Dr. Munro removed the protruding mass when it was hanging into the vagina. The strength was gradually undermined, and the blood disintegrated: and in the end of 1873, and again in 1874, she had attacks of phlebitis in both legs, but most severe in the left. She sank at last, and died on the 29th November, 1874, five years and four months from the date of the first detection of the sarcoma.

Dr. Munro was allowed to remove the uterus, which he kindly gave me. It is enlarged in size, and presents the general appearance of a uterus at the fourth month of pregnancy, except that the os is expanded to the size of a two-shilling piece, and occupied by the projecting growth. It measures from fundus to os externally 5" 9", from side to side at the level of the Fallopian tubes 5" 5", and from before backwards 3". The Fallopian tubes are seen to be enlarged to a corresponding degree, each having a thickness of from half an inch to 1" 1", and from their free fimbriated extremities there project rounded masses, having the appearance of the thrombus projecting from a small vein into a larger trunk. That on the right side pouts out to the extent of half an inch from the tube. They are evidently of the same nature as the growth protruding through the os uteri, though somewhat paler in colour. On cutting open the anterior wall of the uterus the morbid mass was found adhering to the whole surface, but not so firmly as to hinder its being easily detached with the handle of the scalpel. In the two lower thirds of the posterior wall, however, the



union is much more intimate. The walls of the uterus were everywhere thinned to a remarkable degree. In the middle of the anterior aspect they measure only from 1" to 2" in thickness. An incision running at right angles to this at the level of the Fallopian tubes cuts through a portion where the wall is thicker, but only because of the development of a few sarcomatous nodules within the layers of the muscular fibres. The mucous membrane is smoothed out, and presents a cribriform appearance from the processes thrown into it from the surface of the tumour, and a scraping of it placed under the microscope shows it to be deprived of its epithelial covering. In the posterior wall the sarcomatous tissue has firmer hold of, and passes deeply into the whole thickness of the walls—so much so, that at three spots nearly at the level of the os internum it has grown right through the muscular wall, and formed slight projections on the peritoneal surface the size of a half-cherry, covered with a thin investment of serous membrane. The walls of the Fallopian tubes are similarly atrophied till there seems to be little more than the serous covering left. On microscopic examination the tumour presents many nucleated cells, both round and spindle-shaped, the latter in by far the greater proportion. But they are for the most part filled with fatty granules and molecules, and the fibrillated tissue around them is also dimly granular.

This case is remarkable clinically, because of the frequency and extent of the sloughy masses which were expelled; and pathologically because of the manner in which the growth, while rooting itself specially in one part of the uterine wall, yet laid hold eventually of its entire mucous membrane, not only in the body but also in the cervix. In its invasion of the Fallopian tubes, and finding in them a new seat of development, the case is, I believe, unique.

CASE III.—*Intra-uterine Sarcoma—Inversion of the Uterus—Amputation with Ecraseur—Death—Description of Tumour.*

J. P., aged forty-one, unmarried, came from the country into my ward, in the Royal Infirmary, on 2nd September, 1875. She was a stout, well-built, anemic-looking woman, and with a somewhat worn expression of countenance. She complained of being very weak, and becoming increasingly weak, from the occurrence of frequent bloody discharges. The first had appeared at a menstrual period, ten months previously, when the amount of flooding alarmed her; and to the recurrence of her courses at shorter intervals than usual and in increased amount she attributed her weakness. During the intermenstrual periods there was a copious leucorrhœal discharge, which had only latterly begun to be offensive. She had no pain till two weeks before her admission, when she began to have distress in the pelvis, more particularly felt in the bladder, micturition becoming difficult, and then impossible, so that for three or four days her only relief was obtained by the use of the catheter. Since then she passed urine of herself,



but with some difficulty and in rather small amount. She stated that some months before she had swelling of the left leg from the knee downwards, and both legs were occasionally swollen at night. The urine had a deposit containing pus and a few blood-corpuscles, but was free from albumen. Palpation over the hypogastrium elicited some degree of tenderness ; otherwise nothing unusual was to be felt through the abdominal parietes. On examination per vaginam, a large tumour was felt distending the vaginal walls, and filling up the pelvis. It was rough and ulcerated at its lower aspect ; the sides felt smooth and fleshy, and in the roof of the vagina it narrowed towards a pedicle, which seemed to come through the os uteri. Because of the pain and bleeding excited when exploratory attempts were made with the sound, and by combined internal and external examination, the exact relation of the morbid growth to the uterine cavity could not at first be ascertained. The patient was ordered to be kept at rest, to use ergot and a tonic internally, and to be syringed with an astringent and disinfectant injection. Her general condition not improving, and occasional attacks of vomiting coming on, on 18th September I had her brought under the influence of chloroform in the presence of Dr. Edmonstone Charles of Calcutta, Dr. Halliday Croom, and the resident physician and clerks, with the view of more fully examining, and, if possible, removing the growth. It was now discovered that the fundus uteri could not be touched through the abdominal walls, that the larger globular portion of the body occupying the vagina was of softer texture than the pedicle, which came through the circle of the os uteri indeed, but felt as if it were attached to the cervix within a few lines of the orifice. It was impossible to get through the os with the finger except at one point, but the absence of the fundus at its usual level leading me to suppose the uterus might be inverted, by means of the sound I ascertained that such was the case. The tumour, rather firm to the feel as it was packed in the vagina, proved to be too friable to be easily seized with forceps or vulsella. With the four fingers of the right hand, I broke off about a half of it, and then the remainder, with the completely inverted uterus, was easily brought down to the vulva. The loop of a chain-écraseur was applied around the base, but as it involved the whole of the fundus the amputation had to be carried through a plane nearly at the level of the orifices of the Fallopian tubes. Notwithstanding that it was slowly cut through, there was some jetting of blood from the wound surfaces, and as the patient was already much reduced by loss of blood, I brought the raw margins together with four metallic stitches, which completely checked all further hemorrhage. The patient did not seem to be much affected as to her general condition immediately after the operation, and on the following day seemed to give fair promise of recovery. On the third day, however, there was a rise in the temperature, and in the evening she died very suddenly, and with some symptoms, such as breathlessness, that led Dr. Croom (I was myself out of town, and did not see her) to suspect that she

might have had a pulmonary embolism. A post-mortem examination was not permitted.

On examination of the amputated fundus uteri, with the adherent growth, it is easy to see how intimate is the union of the latter with the uterine wall. The union, indeed, is so intimate that one cannot tell where the transition takes place from the tissues of the neoplasm to those of the uterus. The growth is found, on microscopic examination, to be composed of large spindle-cells, each with a clear nucleus lying close together in parallel bundles, and close up to the serous coat of the uterus some of the well-marked bundles of spindle-cells can be traced among the strata of muscular fibres and connective tissue of the middle coat.

The most interesting feature in connexion with this case is the inversion of the uterus. It is now the fourth case of sarcoma uteri with this complication. The first case on record<sup>1</sup> will be found in the Lectures on Pathological Anatomy by Dr. Wilks (p. 404), published in 1859,<sup>2</sup> but the clinical history is wanting. The next case was brought under the notice of the Berlin Obstetrical Society, in January, 1860, by Langenbeck;<sup>3</sup> the patient died without being operated on, and being already very much reduced, the death seemed to be hastened by the bleeding caused by the examination made to clear up the nature of her malady.

In the third case, by Spiegelberg,<sup>4</sup> the entire inverted uterus was amputated with the *écraseur*, and the patient died in thirty hours with acute peritonitis. The frequency of inversion of the uterus as a complication of sarcoma uteri is surely somewhat remarkable. If we add<sup>5</sup> to the thirty-nine cases collected by Kunert<sup>6</sup> the case above referred to by Wilks, the two given by Schroeder in his text-book, one by P. Müller,<sup>7</sup> one by Ahlfeld,<sup>8</sup> and the four which form the groundwork of this communication, we have a total of forty-eight cases of uterine sarcoma, and in four there was complete inversion of the uterus. Besides that Spiegelberg notes a tendency to inversion in two of his other cases, the fact that in one out of every twelve cases of intra-uterine sarcoma the organ had become completely inverted is sufficiently striking, for we find no such frequency of inversion in the cases of the common intra-uterine myomata. But this complication becomes more remarkable when we consider that all the three patients with inversion, whose history we know, were nulliparous females, or, if one

<sup>1</sup> Apparently overlooked by Kunert, &c., though it is referred to in a note in Virchow's "Onkology."

<sup>2</sup> Under the heading "Recurrent Fibroid Disease." In the recent edition—"Lectures on Pathological Anatomy," by Wilks and Moxon, 1875—it is given, p. 561, under the heading "Sarcoma."

<sup>3</sup> "Monatsschrift für Geburtskunde," xv. 173.

<sup>4</sup> Loc. cit., p. 351.

<sup>5</sup> I leave out of account cases, such as are recorded by Leopold and P. Grenser, of sarcoma springing from the cervix uteri, for in such inversion of the organ was not likely to take place. Perhaps some of Kunert's thirty-nine cases are instances of cervical sarcoma.

<sup>6</sup> Loc. cit., 116.

<sup>7</sup> "Archiv für Gynäkologie," vi. 125.

<sup>8</sup> Ibid., vii. 301.

of them (Langenbeck's case) had given birth to a child, it was twenty-three years before the occurrence of the sarcomatous inversion. There are two points, however, in connexion with the development of intra-uterine sarcomas in which they differ from the common myomas, and which prepare us to expect the production of inversion. In the *first* place, the sarcoma springing, say, from the fundus uteri, is in intimate union with the walls from which it grows, whereas the myoma is surrounded by a capsule, and so more easily separable from the bed in which it lies. The result of this difference in the relation of the two kinds of tumour to the tissues where they originate is, that when the uterus begins to contract for the expulsion of the neoplasm, the sarcoma drags down with it its seat, in the wall of which it forms an integral part; whereas the myome gets gradually detached from its loose connexions and becomes pediculated, and perhaps is at last expelled from the cavity of the uterus. Doubtless, also, the condition of the layers of the muscular fibres between the neoplasm and the serous surface differs; in my case, certainly, there must have been some degree of paralysis at the site of the tumour, whilst a myome in the same situation would probably have been covered with a layer of healthy muscular fibres, which would concur equally with the rest of the uterine muscles in pressing the tumour towards the cavity. But, in the *second* place, there is in these sarcomatous cases an unusual relaxation of the walls of the genital canals, more than is to be accounted for by the moistening with the frequently profuse discharge. I was particularly struck with this in the second of the cases which I have related. There were times when though at the first touch of the finger the os was too small to admit it, yet on continued pressure it yielded and opened up; and, as I have stated, there were times when it opened freely and widely to permit of the escape of sloughy portions of the tumour. It was as if a relaxation and dilatation took place at those times when the uterus was making efforts for the birth of the neoplasm, parallel to the relaxation and dilatation that accompany the contractions of the uterus in ordinary labour. And it is to be remembered that that kind of vital dilatation is not confined to the cervical canal. Though the patient whose case is under discussion was unmarried, and the vulva in a virginal condition, so that the full examination could only be conducted after she was anesthetized, it was remarkable to what an extent the genital orifice became dilated during the operation, so that first three fingers, and then the whole hand, could be passed into the vagina without lacerating the mucous membrane. This point was the more impressed upon my mind from the circumstance that less than three weeks previously I had seen, with Dr. Brotherston of Alloa, a case of a large fibro-myomatous polypus coming down from the interior of the uterus of the size of two fists, and hanging by a somewhat narrow neck into the vagina. The patient in that case also was an unmarried female of about forty years of age, and after I had succeeded in detaching the tumour from its uterine connexion by torsion, I was almost baffled in



my attempts to extract it through the firm and unyielding vulva, and only succeeded at last by cutting the tumour into pieces within the cavity, the patient being kept under the influence of chloroform for upwards of two hours. The singular dilatability of the canals, and the intimate connexion between the sarcoma and the part of the wall to which it is attached—at once giving it a strong purchase upon its seat and impairing the action of the muscular fibres in that portion of the wall—seem to me to afford a fair explanation of the marked proclivity of the sarcomatously affected uterus to become inverted.

In the next case we have an example of the diffuse sarcoma.

*CASE IV.—Intra-uterine Sarcoma—Patient still under Observation.*

Mrs. C., aged forty-five, admitted to Ward XIV., 4th October, 1875, has been married twenty-five years, and is mother of ten children, the eldest born twenty-three, the youngest nine years ago. She had always menstruated regularly and enjoyed good health till eighteen months ago, when she began to suffer from floodings. Within the last four months the pains, which remind her of labour pains, have begun from time to time to distress her. Four weeks ago one of her floodings set in, and continued for seven weeks, large clots sometimes escaping. For a fortnight the bleeding ceased, but she had then a profuse watery fetid discharge, and a week before her admission the bleeding returned. She was greatly reduced in her general health, with a weak, quick pulse, and a cachectic expression of countenance. There was nothing to be felt on abdominal palpation. On vaginal examination the uterus felt heavy; the os was soft and patulous, and easily admitted the tip of the finger as far as the os internum, at which a substance with the feel of a soft clot could be touched. The cervical canal having been further dilated with a tent, the cavity of the uterus was felt greatly expanded; the front and back walls were covered with soft, ragged, irregular patches of tissue, a larger and more prominent portion of which grew downwards from the fundus. But when I attempted to make a combined external and internal examination, the walls of the uterus gave me so much the impression that they were ready to tear, and there began to flow from the already enfeebled patient such a stream of blood, that I was fain to desist, and to arrest the hemorrhage by a free application of perchloride of iron into the uterine cavity and the introduction of a vaginal plug. She soon recovered from the effects of that bleeding, and went home, as further operative interference gave no hope of any good result.

She has had less pain since the uterus was dilated, and but little hemorrhage; but the watery discharge is more profuse and very offensive. A small fragment of the excrescence removed with the finger-nail shows the characteristic appearance of the spindle-celled sarcoma. Some large epithelial cells with double nucleus which appear in the field of the microscope may have been derived from



the vaginal canal during the abstraction of the minute fragments of tissue. At least there were no similar groups of cells collected in interstices of connective tissue, such as we see in cases of carcinoma.

The narration of the cases, with the epicritical remarks, has extended to a greater length than I had anticipated. What further general observations I have to add must therefore be brief.

*Etiology.*—My cases do not confirm the impression made by statistics already published, that nulliparity favours the development of sarcoma uteri. In one of them, as I have said, the patient was unmarried; but the other three patients were married women, who had each been mothers. As for age, they were all approaching, or had passed, the menopause. The youngest was forty-one, the oldest seventy-two years of age at death. Their social circumstances were comfortable, even the hospital cases not being drawn from the poorest class of society; and so far as could be ascertained, their family history was good. On looking over the ward statistics for the last four years, with the view of comparing the proportion of sarcomata to the more common uterine neoplasm, I find twenty cases of myoma or fibro-myoma, and thirty-two cases of carcinoma, two of which were intra-uterine, whilst there were only the two cases above recorded that could be regarded as sarcomatous.

*Symptoms and Diagnosis.*—In most cases these growths give rise to a marked degree of *hemorrhage*. The usual monthly discharge becomes exaggerated, or bleedings come on in the intermenstrual interval, or they are set up in women who had passed the menopause. These hemorrhages are easily intelligible when we bear in mind that the tumours are always vascular, sometimes channelled with large and thin-walled blood-vessels; and that from the softness of their texture they are easily broken down. But, in certain cases, the tendency to hemorrhage is not pronounced. There is usually, however, a marked degree of *leucorrhœal discharge*. The succulent and sometimes suppurating surface of the tumour itself, and the thickened and expanded mucous membrane lining the cavity in which it lies, furnish a free secretion of a pale, rice-watery, or more yellow fluid, which, in any case, may for a time disappear, though it is almost never absent (only in Leopold's case, where the tumour developed in the cervix) throughout the entire history of any case. This discharge will, for a long time, be odourless, but it acquires something approaching to the disagreeable odour of the cancerous discharges in the more advanced stages of the disease, when fragments of the tumour are squeezed into the vagina, and lie within the canals till they decompose. We have seen that the mischief may develop in women who have borne children; but after it has appeared, the *reproductive function* of the organ is at an end. For I know of no case of the concurrence of pregnancy and sarcoma uteri, such as we sometimes meet with in myoma or carcinoma: probably from the circumstance that the sarcomatous growths almost always affect the interior of the uterine cavity. There has been

some diversity of opinion as to whether this affection is attended with *pain* or not. My own impression is, that pain is not a usual symptom of it, any more than we can speak of pain being a usual symptom of a fibrous polypus. In both cases we may have suffering from the enlargement, or from intercurrent inflammation in the uterus or around it, or, what is still more common, we may have pain from the muscular contractions set up from time to time in the walls of the uterus for the expulsion of the neoplasm. But all this is different from the uncertain, unprovoked, sharp stinging pain to which a cancerous patient is subject. More particularly I have been struck with the difference between the pain complained of by these patients who are the subjects of intra-uterine sarcoma, and the intense paroxysmal attacks of suffering, coming on sometimes<sup>1</sup> with a marked degree of periodicity, which Sir James Simpson used to point out as characteristic of intra-uterine cancer. It was the absence of this symptom, which I had seen in several instances, that first led me to think that, in the case No. IV., we should find, as we did, not the hard, rough surface of an epithelial, but the soft, velvety, friable surface of a sarcomatous growth. In addition, there is *disturbance* in the functions of *neighbouring organs* differing in degree in different cases according to the size and situation of the neoplasm and its bed. Sooner or later the repeated losses of blood or the continued drain of watery discharge begin to tell on the patient's general health; and when the disease has advanced towards its final stages, it usually develops in the sufferer a condition of *general cachexia*.

The *physical examination* of a patient with such a train of symptoms would discover some degree of uterine enlargement, varying according to the size of the morbid mass occupying its interior. It is rarely so large as to form an abdominal tumour, except where we have to do with a myo-sarcoma, or a fibroid tumour undergoing sarcomatous degeneration, when we may find a supra-pubic mass of considerable dimensions, with the vascular bruit and other characteristics of such growths. Where the growth springs from the cervix, or the uterus has become inverted, a polypoidal body will be felt occupying the vagina. The uterus, though enlarged, remains freely moveable, unless the tumour be so great as to cause some impaction in the pelvis, or inflammatory adhesions have formed among the pelvic organs—conditions which are both comparatively rare in the true sarcoma. The sound will pass without difficulty into the interior of the uterus, and detect the extent of increase of the cavity and the presence of a foreign body in which bleeding is easily excited. But it is only when the canal is dilated with a tent—and, as I have shown, the textures are so soft and expansible that a single good-sized sponge-tent may suffice for the dilatation—that the size, consistence, and attachments of the new growth can be discovered, and that a

---

<sup>1</sup> See an illustrative case in a Clinical Lecture by Thomas, in the *American Journal of Obstetrics*, v. 703, 1873.

fragment of it can be obtained for microscopical investigation. It is in this way alone that an exact diagnosis can be made out.

*Prognosis.*—The comparative degree of malignancy to be attributed to this neoplasm may be gathered from the significant circumstance, that West treats of it in the same lecture with fibrous polypi, whilst Barnes gives it a place among the cancers. Although the disease shows a distinct tendency to remain localized in the uterus—those cases being but few where secondary sarcomata were found in other organs—there seems to be only one recorded case (Winckel's first) where removal of the growth was followed by a radical cure; and it is to be noted in regard to it, that it was a polypoidal myo-sarcoma, and not a pure sarcoma, and that the patient's history does not extend beyond two years from the date of operation. According to Kunert, "in 14 out of 30 cases death ensued within a year after the first appearance, in 1 after 2 years, in 3 after 4–6 years; in 3 cases the patient was still suffering after 2 years; in regard to the remaining cases, partly observation is awaiting, and partly the patients enjoyed tolerable health under continuous medical treatment." One case is recorded<sup>1</sup> where an intra-uterine sarcoma was the seat of carcinomatous degeneration, and proved rapidly fatal. My own cases confirm the general verdict that we have here to do with a form of disease which, though it is not the cause of such intense suffering, nor of such rapid constitutional deterioration, nor of such speedy death as cancer, yet has a vicious tendency to recur after apparent complete removal, and to lead sooner or later to a fatal issue.

*Treatment.*—Clearly, however, patients who are the subjects of it are not to be abandoned to a hopeless do-nothingism. Where the mischief is in an early stage, and in any case where we have it in a polypoidal form, it should always be removed, to give immediate relief from suffering, and with the justifiable expectation that, for a time—it may be for years—the progress of the malady will be arrested, and the patient restored to comparative health. It may even be of service to her to repeat the operation more than once. In the pediculated forms the growth may be removed with the écraseur or galvano-caustic wire; but sometimes, as we have seen, it is too brittle, and requires to be broken down, and removed piecemeal. In such cases, and in cases of the diffuse sarcoma where the extirpation can be attempted, it may be carried out with the curette, fenestrated like Recamier's, or cup-shaped like Simon's. In any case, it is desirable to lessen the immediate hemorrhage by free application of perchloride of iron and hypodermic injection of ergotine. The profuse and sometimes fetid discharge can be controlled by astringent and disinfectant injections; and the patient's general health will be kept up by a generous diet, and the administration of tonic remedies.

<sup>1</sup> Rabl-Rückhard, in *Beiträge zur Geburtshülfe*, i. 76, 1872.



Dr. KEILLER thought the paper one of value and novelty. He could, on looking back, remember a number of such cases, one of a somewhat suspicious character he would refer to. He had reported it to the Society as a fibroid growing from an inverted fundus uteri, and had removed it as such; the woman recovered, and still survives with the uterus inverted. In this case he pulled the tumour down to the vaginal orifice, and saw the inverted fundus with the tumour attached to it. After removal of the tumour by the *écraseur* all efforts to re-invert the uterus failed, though great force was employed. The patient for a long time subsequently had severe periodic menorrhagia, but little or no pain or tenderness. Some other cases he regretted having interfered with, and now thought he would have done better to have left them alone. He had often injected the perchloride of iron in such cases, and had found much benefit from it; the longer he lived the more he felt disposed to use palliative rather than radical treatment in cases of a malignant nature, which were often aggravated by interference, and seldom or never cured by operative treatment; although it must be admitted that the knife and other so-called radical measures are occasionally called for in order to diminish hemorrhage, or to relieve other urgent symptoms.

Dr. GORDON was struck, in Mrs. B.'s case, with the very slight tendency to bleeding in spite of the great size of the surfaces which were cut through. He was quite satisfied that her life was much prolonged by the repeated operations. He considered that they added several years to her life.

Dr. DUNCAN had found it very hard to satisfy himself about the classification of the various malignant diseases of the uterus; he called them all malignant disease. No doubt Virchow's definition and description of sarcoma were of great importance, but we must remember that they were denied by another school of pathologists. Clinically he found the greatest differences in their histories and symptoms; and that not only in tumours growing from the uterine wall, but in those affecting the os uteri also—some were distinctly cauliflower, others were spindle-celled sarcoma; between them there was an infinite variety, so that he could come to no conclusion as to classification. Dr. Simpson's cases were all sarcomata according to some definitions, but they differed materially in their history, course, and symptoms. The Society was much indebted to Professor Simpson for his valuable paper.

Professor SIMPSON, in reply, observed that in the cases he had related the disease certainly varied in character; in Case I., there was a distinct descent from fibroid to sarcoma, while Case IV. may have been one passing from sarcoma to carcinoma. At the same time, two of the cases were typical illustrations of a definite form of tumour. In attempting to classify the mixed forms of growth, not only the anatomical structure, but also the clinical history must be taken into consideration. In Dr. Keiller's case the tumour removed was not a sarcoma, but a fibroid tumour, and was, in fact, an illustra-



tion of the rare complication which he thought he had referred to in his communication, of which there are several instances on record, where a uterus containing a fibroid polypus had become inverted. Its complete removal without recurrence certainly was in favour of the view that it was a fibroid. The patient had lived a long time since the operation; whereas one of the great features of sarcoma was its tendency to recur.

### Obstetric Summary.

#### *Uterine Decidua expelled without Metrorrhagia, and with the signs of Internal Hemorrhage, probable Extra-uterine Fotation.*

Dr. Féréol (in *Archiv. de Tocologie*, January, 1876) relates the particulars of a most interesting case of this nature. The patient, a sempstress, aged twenty-five, was confined of her first child when twenty, and of her second when twenty-three, after which she had a serious attack of pelvi-peritonitis, lasting three months. The catamenia had not appeared the last two times. On July 20th she was perfectly well. During the day, whilst seated at work, and without any appreciable cause, she experienced severe pain in the lower abdomen, necessitating her going to bed, where she remained two days. On the 23rd, she went out, and on her return the pain became extremely intense, and she fainted and vomitted.

When taken to the hospital on the 26th, she was exsanguine, of a deathly pallor, in an almost incessant state of syncope, vomiting everything that was given her. The pulse was small, thready, 120. The abdomen was distended, tympanitic, extremely tender. There had been no external hemorrhage. *Per vaginam*: The uterus was found to be immobile, the cervix patulous, an intestinal swelling being detected in the right posterior cul de sac.

Brandy and ice were administered in small quantities at a time, and the patient rallied somewhat.

On the 28th, two days after admission, on examining *per vaginam*, membrane was found free in the vagina, there not being the least trace of blood.

The membrane was triangular, thick villous on one of its faces, presenting upon the other well marked glandular apertures, evidently decidual.

The patient convalesced very tardily, but ultimately did well, an intercurrent attack of pleurisy on both sides, necessitating thoracentesis on the left side, retarding recovery.

The author, in some accompanying remarks, dwells upon the singularity of the expulsion of the membrane without a trace of uterine hemorrhage, either before, during, or after its expulsion.

## Gynecic Summary.

### *Accidental Penetration of a Hairpin into the Bladder.*

Monsieur Bardeux (in *Le Progrès Médical*, January 1st, 1876) gives an instance. A lady, aged thirty, just as she was leaving home perceived the catamenia coming on, and not having time to protect herself as usual, fastened her chemise with a hairpin. Arrived at her destination, she seated herself by some others, when a particular sensation made her rebound from her seat, and, on examination, the hairpin was found to have passed by the rounded extremity into the bladder. After several ineffectual attempts to remove it, gradual dilatation of the urethra was resorted to, and the pin removed.

---

## Pædiatric Summary.

### *Dog's Milk for Children.*

Dr. P. Luzun (*Bordeaux Médical*, No. 43, and *Gazette Hebdom.*, November 5, 1875) relates the particulars of three cases in which he employed dog's milk. In the first, a girl between six and seven years old, affected with rickets, who was unable to walk. Within twenty-five days she became vigorous and able to walk. He states that dog's milk contains as much again of butter as human milk or that of the cow, and seven or eight times more than that of the donkey. It is also of all the milks which are employed by man, save that of the sow, the richest in casein.

---

## News.

### *The Obstetrical Society of London.*

Probably few scientific Societies have achieved in so short a number of years greater distinction and success than the Obstetrical Society of London.

In numbers, influence, and valuable work its development has been remarkable; and we learn with pleasure that a continuation of this progress is still aimed at by those who are responsible for the conduct of its affairs. The past work of the Society, and especially of late the important discussion on Puerperal Fever, has illustrated its great usefulness, and how powerfully it may aid in advancing professional knowledge.

We are glad to see that further steps advantageous to our important department of medicine are contemplated by the Society. A powerful Committee has been appointed by the Council to take cognizance of the recent and impending action of the College of Surgeons in reference to its midwifery licence, and we may rely on the Society

to prevent degradation of this branch of the profession by means of an inferior qualification.

It will be recollected that ten years ago the Society gave a most instructive conversazione at the Royal College of Physicians, when a large number of Obstetrical instruments, ancient and modern, were exhibited; and the Society very wisely published an illustrated catalogue of these. We learn that it is again proposed to give a conversazione, and that through the kindness of the authorities it will once more be given at the College of Physicians. We believe that there is every promise of a very brilliant gathering; and we are glad that so widespread and influential a department of practice as Midwifery will be worthily represented at a time when it is necessary that its influence should be asserted and felt in official quarters.

We are informed that at the next meeting of the Obstetrical Society the discussion on Dr. Meadows's note on the Post-mortem Diagnosis of a Nulliparous Uterus will be resumed, and we believe that an important debate is likely to take place. Dr. Braxton Hicks and others are expected to show specimens.

#### BOOKS, PAMPHLETS, AND PAPERS RECEIVED.

Ziemssen's "Cyclopædia of the Practice of Medicine." Vol. V. Diseases of the Respiratory Organs. By Prof. Juergensen, Prof. Hertz, Prof. Ruehle, and Prof. Rindfleisch. London: Sampson Low and Co. 1875. Pp. 712.

"On the Development of the Ova and Structure of the Ovary in Man and other Mammalia." By James Foulis, M.D. Edinburgh. 1875.

"The Address in Obstetrics and Diseases of Women and Children." By W. H. Byford, M.D. Philadelphia. 1875.

"The Genesis of an Epidemic of Puerperal Fever." By W. T. Lusk, M.D. New York. 1875.

"Geburtsbehinderung durch grössere Fibromyome in der Wand des Mutterhalskanals." Von Eduard Martin.

"Herpes et Erythema iris und Scarlatina vei Wöchnerinnen." Von A. Martin.

Communications have been received from Dr. Priestley, Dr. Ashburton Thompson, Dr. A. Henry, Dr. Edis, Dr. A. Wiltshire, Dr. John Williams, and Mr. Cullingworth.

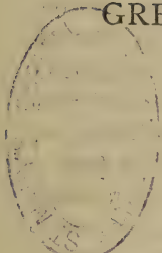
*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.

# THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.



~~~~~  
No. XXXVI.—MARCH, 1876.  
~~~~~

Original Communications.

---

## ON THE WEIGHT OF NEW-BORN CHILDREN.

By E. INGERSLEV,

Clinical Assistant in the Lying-in Institution in Copenhagen.

*(Continued from p. 715.)*

EXAMINATIONS of the weight of the child in the earliest periods after birth soon show that its growth does not proceed continuously, but that the increase is preceded by a period of decrease, which, according to its greater or less duration, may not inconsiderably reduce the weight found at birth. The earliest investigators of this subject (Quetelet, Hoffmann, Bartsch, Breslau, &c.) asserted that there is after birth a loss of weight, which is nearly recovered at the end of the first week. The amount of material for investigation was in most cases very small, and more comprehensive researches on this point were first made by E. von Siebold, whose examinations comprised forty-nine children, of whom thirty-five showed a loss of weight amounting to a quarter or half a pound within the first two or three days. According to his view, the weight of the child remains stationary until the fourth, or even the sixth day, when an increase begins. But as Siebold weighed the children only on every other day, this conclusion is scarcely justified; for when the child shows a decrease in weight on



the fourth day, and the same weight on the sixth, there may have been a further fall on the fifth day followed by an increase on the next. That fourteen children showed neither regular increase nor loss of weight on the earliest days, while an increase was evident from the sixth to the eighth day, was probably due to the circumstance that Siebold took premature children into account, and also unhealthy children born of unhealthy mothers, in whom irregular oscillations readily occur. Later observers all find a loss of weight in the first days ; and Siebold also declares that he never met with a distinct increase during this period. He finds the causes of this loss, partly in the character of the colostrum—which acts as an evacuant rather than as an aliment, perhaps through its greater richness in salts, or perhaps because, taken as the first food, it acts as an irritant on the intestinal canal ; partly in the inability of the child to suck at first.

Haake's researches (*Monatsschrift für Geburtsk.*, vol. xix.) were made on 100 mature children, who were weighed by himself immediately after birth, and then daily at a fixed hour until the ninth day. That the first weighing should take place immediately after birth is of no small importance to a trustworthy result, for even a few hours after birth the original weight may be not inconsiderably diminished by the evacuation of meconium and urine. The children were also all healthy, and the mothers in the normal conditions of the puerperal state. The result of his researches was, that every child loses weight on the first days of extra-uterine life, and that the third day is generally the turning-point when the increase begins. The total decrease of weight (a little over 6 ounces) was in boys from  $\frac{1}{16}$ th to  $\frac{1}{17}$ th of the weight of the body ; in girls,  $\frac{1}{15}$ th to  $\frac{1}{16}$ th ; there being thus in the former a small average loss, just as the subsequent increase was greater. Winckel's researches (*Monatsschrift*, vol. xix.) comprised also 100 children ; and he likewise found a loss of weight up to the third or fourth day amounting to about 7 ounces, six-sevenths of it falling within the first two days. In the first twenty-four hours all, without exception, showed a decrease amounting on an average to  $3\frac{1}{2}$  ounces. In this number of 100 children, were both mature and premature

ones, unhealthy and healthy, artificially and naturally fed. Hence, if we would compare Winckel's results with those of Haake, we must only take notice of the fully developed healthy children, born of healthy mothers. These were sixty in number, and their average loss of weight during the first two or three days was 6 ounces, while the mean increase up to the tenth day was  $7\frac{1}{2}$  ounces—36 having on this day exceeded the weight at birth. The loss of weight was also found to be less in boys, and the increase greater. While the results taken as a whole thus agree, they vary in one respect—as regards the cause of loss of weight. Winckel seeks the explanation of this in the great outgoing and small income during the first days of the child's life: the evacuation of meconium and urine may make the child an ounce or an ounce and a half lighter two hours after birth; to this are now added cutaneous transpiration, muscular movements, and change of nourishment. The child may learn to suck, but it takes often an insufficient and perhaps evacuant milk (colostrum); and finally the umbilical cord has to be thrown off, to which Winckel endeavours to attach no slight importance. For, as this was found in three-fourths of the children to be thrown off on the third or fourth day, and also three-fourths of the children fed with their mothers' milk showed an increase of weight from the third or fourth day, Winckel regards the detachment of the umbilical cord as a condition of the commencement of increase of weight, as, in his opinion, the bile-secreting and blood-forming functions of the liver cannot be normal until the necessary obliteration at the umbilicus is completed. Haake (like later observers) finds no such connexion between these two factors, and rejects the detachment of the umbilical cord as having any influence on the increase of weight; for while 75 children began to increase in weight on the third day, in only 21 of them was the cord detached at this time; in the greater number the detachment was not completed before the fourth or fifth day. The explanation of the loss of weight, according to Haake, must be sought either in the small supply of nourishment, or in the want of relation between the child's power to assimilate what it takes in,

and the discharge of excrementitious matters. Scantiness of the mother's milk, the qualitative changes in it during the first days, and the frequently defective power of the child to suck, appear to give great support to the view that deficient nourishment is a cause of loss of weight ; but as children fed during the first days either with cow's milk properly prepared, or with drawn-off mother's milk in proper quantity, or immediately suckled by nurses, show just as great a loss of weight, Haake attaches essential importance to the imperfect functional power of the intestinal canal during the first days of life—that is to say, when the secretion of gastric juice has not begun, and the peristaltic action of the intestines is imperfect, the assimilation of the food taken in can be but insufficient ; and to this must be added the great loss by respiration, transpiration, defecation, and the active oxidation processes in the tissues, which commence immediately at birth. He therefore compares the child during the first days of life to an animal subjected to inanition and supplying excreta from its own tissue-elements. The loss of weight he would then regard as a necessary inanition, not a pathological condition, but a physiological process which cannot be escaped from by giving the child any other food than its mother's milk during the first days of life ; for indifferent materials will not prevent the loss of weight, and more strongly nutritive articles will not be absorbed or be of use to the child.

Like Haake, Gregory (*Archiv für Gynäkologie*, vol. ii.) and Kezmarsky found no connexion between the commencement of increase of weight and the detachment of the umbilical cord ; according to Gregory, this very rarely takes place within the first three days, and frequently on the fourth or fifth day, and after the fifth day more frequently than before the fourth : the loss of weight he finds to cease on the third day—that is, before the cord is detached. Gregory fixes the average loss of weight at 139 grammes on the first day after birth, and 64 grammes on the second day, the whole loss amounting to  $\frac{1}{16}$ th or  $\frac{1}{17}$ th of the weight of the body. Of mature children fed with cow's milk, twelve were examined, and in these the increase was found to begin

a day later ; and the increase was very irregular, alternating with decrease, so that on the ninth day the children did not weigh so much as when the increase commenced. Of immature breast-fed children, some were weighed, and in these the loss of weight during the first days was found to take place about the same as in mature ones ; but the increase was more irregular, though not so defective as in those artificially fed. The conditions were naturally unfavourable in eight immature children fed by bottle, where there could scarcely be said to be an increase, this only appearing as an interruption to the progressive loss. In order to ascertain how far improved feeding of the mother had an influence on the child, Gregory fed ten lying-in women with richer food ; in these cases the increase began in their children on the second day, and was more considerable and more regular ; all with one exception on the eighth day exceeded the weight at birth. He concludes from this, that improved feeding of the mother has a favourable influence on the child, since the period of loss of weight is shortened and the increase is greater.

This view had been already referred to by Kehrer (*Archiv für Gynäkologie*, vol. i.). In inquiring whether the loss of weight in the new-born child was a physiological process peculiar to man or occurred also in animals, he found on examining dogs, cats, and rabbits, especially when the mothers were sufficiently fed, that the young increase uninterruptedly, though irregularly after birth ; he is of opinion, however, that there is a short period of arrest in which loss of weight takes place, but which is so rapidly compensated by the supply of food, that the first weighing after birth shows an increase. That, however, the condition is not so completely different in mammalia and in man is proved by Edlefsen's researches on porpoises (*Archiv für Gynäkologie*, Band i.), in which there was almost constantly a loss of weight during the first thirty-six hours, amounting to about 4 per cent. of the total weight. This sudden appearance of increased weight in animals is connected by Kehrer with the regular presence of the colostrum at birth, while in man the secretion begins comparatively late, though earlier in proportion to the



woman's health and to the sufficiency of the diet during the later period of pregnancy. In the next place, the child has to learn to suck, while the animal can suck instantly with great readiness. These two conditions he regards as the decisive ones ; for the loss by meconium and the other excretions are common. The practical conclusion from this is, that the child should be early applied to the breast and the woman well fed, so that the period when the increase of weight sets in may not be delayed too long. While the above-named observers regard a loss of weight in the first days after birth as the ordinary rule, there were among the 54 children weighed in the Maternité by Bouchaud, five in whom there was an increase without subsequent decrease ; and the loss of weight found by Bouchaud is on the whole less than that noticed by other observers, as he estimates it at 100 grammes for the first two days, and ascribes it simply to the discharge of meconium. All the strong children increased from the third day ; and when this was not the case, some unfavourable influence on the part of the nurse or child could be found. In continuing daily weighings during the first year of the child's life, Bouchaud found that the early increase during the first five months was 20 to 25 grammes, and in the following months 10 to 15 grammes. At the fifth month the weight was doubtful ; and in the next seven months the whole increase was not so great as from birth to the end of the fifth month. Odier (*"La Loi d'Accroissement des Nouveau-nés"*) estimates the daily increase at from 25 to 10 grammes ; but he regards this as a minimum, and believes that the smallness of the figure is due to the place where the researches were undertaken, since, in a hygienic aspect, the Maternité must be regarded as a very unfavourable place for children. The variations of weight are set forth by Odier in graphic curves, and the loss of weight during the first days (*"dépression normale"*) is attributed by him to the meconium, urine, and transpiration alone.

Professor Ritter von Ritterstein has endeavoured (*Jahresbericht der k. böhmisch. Findenanstalt*, 1866) to disprove the assumption that a loss of weight is normal, and a kind of physiological necessity. But it is difficult to draw any con-

clusion from his statement. Of 144 children only a small number were weighed immediately after birth, the second weighing being made between the third and seventh days, generally on the fifth day. Of these 76 had increased, 12 had the same weight, and 56 had lost weight ; and this is regarded as evidence that the assumption of a constant loss of weight is incorrect.

As the weight was not taken accurately at birth, but only when the child was brought into the foundling institution, that is to say, during the period of decrease ; and as the second weighing was performed on an average on the fifth day, the whole period of loss of weight is left out of account, and the second weighing (on the fifth day) will readily give an increase in comparison with the first. In his Report of the Foundling Hospital for 1865-69 (*Oesterr. Jahrb. für Pädiatrik*, vol. ii.) he admits that a decrease in weight on the first and second days may be normal, and believes that this is a result of the child not sucking, or sucking very little ; but he sees, in the fact that the loss is not great, a proof that the proper tissue-growth is most energetic in the earliest days, while it may be regarded as very favourable when the child remains of the same weight or increases a little during the first days after birth. A hundred boys and a hundred girls (healthy and unhealthy, mature and immature) were weighed daily until the seventh day, the first time from two to six hours after birth ; and when the child had its nurse changed frequently during the seven days on which it was weighed, a natural explanation of the oscillations in weight was given, no regular increase or decrease being shown. As regards the boys, the average loss of weight on the second day was 25 grammes, after which there were alternate risings and fallings till the seventh day. In the girls, the loss on the second day was about 3 grammes, after this there was a decrease until the sixth day, on which, and on the seventh, there was an increase. As an institution where children are taken from the mothers and often change the nurses is not an example of normal feeding, and as the loss of weight and the variations are so small during the first days, especially

when the amount of excreta is considered, he thinks that the weight in reality makes greater advance during the first days than afterwards, and therefore maintains the opinion that growth is greater the younger the child; if this do not take place, it is to be attributed to the circumstance that the child is not in the condition corresponding to perfectly natural and sufficient nutrition.

To investigate this matter I have daily weighed 96 children born in the lying-in institution here, the weight being first determined immediately after birth, when the child was cleansed and washed. The daily weighings were performed at a fixed hour up to and including the tenth day, with intervals of twenty-four hours, with the exception of the first, of which the time varied from a few hours up to twenty-four. The children were weighed quite naked. Of the 96 children, 90 showed an average loss of 86 grammes at the first weighing after birth, four remained stationary, and in two there was an increase. The exceptional condition in the six children was interrupted on the second and third days, when a loss of weight showed itself; and of the two children which showed an increase at the first weighing after birth, one was a breech presentation, and the other was in a state of intense asphyxia: both evacuated a large quantity of meconium during birth, and both sucked immediately afterwards. Of the four which were stationary, in two there was no discharge of meconium on the first day, and all had sucked before being weighed for the first time. In this way the loss of weight is, perhaps, naturally compensated; but the compensation is but of short duration and of secondary importance. On the second day, 91 showed an average loss of 98·3 grammes, making up a loss of 184·3 grammes, corresponding to  $\frac{1}{17}$ th or  $\frac{1}{18}$ th of the body weight. The greatest loss of weight on the first day was 210 grammes, and a loss of more than 150 grammes was observed in 14 cases. In order to investigate the weight of the children during the first ten days under as similar and as normal conditions as possible, I put aside all the children who were ill, also those with quite transient disorders, such as dyspepsia, the influence of which on the conditions of weight is

generally very considerable, and also the children whose mothers had febrile puerperal diseases or other complications, or who had fissure of the nipple, which may have no small influence on the nourishment of the child. There remained then 50 healthy mature children, born of healthy mothers, the conditions of whose weight during the first ten days after birth are shown in the following table, where the averages of loss and increase are given.

TABLE XII.

	DECREASE.		INCREASE.		STATIONARY.
	Grammes.	Number.	Grammes.	Number.	Number.
First day .	90·2	46	10	1	3
Second day	88·7	47	45	2	1
Third day .	63·7	27	56·5	23	—
Fourth day.	52·1	14	64·2	33	3
Fifth day .	40·8	13	59·1	34	3
Sixth day .	41·8	11	45·6	39	—
Seventh day	27·8	9	57	37	4
Eighth day.	32	5	66·8	41	4
Ninth day .	24·3	7	46	35	8
Tenth day .	30	12	51·3	35	3

The increase of weight began on the second day in 1 child, on the third day in 20, on the fourth in 14, on the fifth in 6, on the sixth in 6, on the seventh in 3. On the average, the time when an increase appeared was 4·1 days, which indicates a longer duration of the period of loss than was shown by most of the earlier investigations, which gave on an average the third day as the turning point.

The interruptions and irregular variations which occur in many children after the increase of weight has commenced depend on accidental circumstances, such as the full or empty state of the bladder or intestines when the child is weighed, or of the stomach, according as it has sucked immediately before the weighing or fasted some time.

When dismissed to the foundling institution or to their homes on the tenth day, 30 children had made an average



increase of 178 grammes since birth, one was stationary, and 19 showed an average loss of 149 grammes.

If we take into consideration the condition on the tenth day, and the time when the loss of weight was at the highest, it is seen that the 30 children who surpassed the weight at birth had gained an average of 344 grammes from the cessation of the decrease till the tenth day; and that in the 19 whose weight on the tenth day was less than at birth, there was an average increase of 142 grammes in the similar period. The daily average increase between the cessation of loss and the tenth day was 23 grammes in the case where the increase began on the fifth day, and about 42 grammes where the increase began at about 3·5 days. So far, we may certainly agree with Professor Ritter's assertion, that the child at no period of its life grows so much as in the first period after birth—that is, if we reckon from the cessation of the decrease in weight, and not from the moment of birth. The whole absolute loss of weight during the first days was on an average 222·6 grammes, corresponding to  $\frac{1}{14}$ th or  $\frac{1}{15}$ th of the body weight, or 6·86 per cent. ; which, on the whole, is a somewhat higher figure than that of the earlier investigators. The loss of weight exceeded 300 grammes in nine cases, the highest figure being 510 grammes in a child weighing more than seven pounds, in whom the increase began on the sixth day and then proceeded regularly and rapidly. No indication of disease could be observed in the mother or child, nor was there any deficiency of food.

The relation between boys and girls is shown in the following table.

TABLE XIII.

	BOYS.		GIRLS.	
	Grammes.	Number.	Grammes.	Number.
Loss of weight . . .	228·7	24	217·3	26
Increase on tenth day .	200	14	160	16
Decrease on tenth day .	140	9	158	10
Stationary on tenth day .	—	1	—	—

The researches of Haake, Winckel, and Gregory show a less loss of weight in boys and a more rapid and greater increase. According to the preceding table, this is not the case here. The loss of weight is absolutely greater in boys, and also relatively : their average weight was 3270 grammes, and the loss was  $\frac{1}{4}$ th or 6.94 per cent. ; the average weight of the girls was 3190 grammes, and the loss  $\frac{1}{5}$ th of the body weight, or 6.8 per cent. ; and the time of the increase of weight was in boys 4.2 days, in the girls 4. In what this different and unfavourable condition for the boys examined here lay, I cannot say. A number of the children examined by Kezmarsky in the institution at Pesth showed a similar condition as regarded the boys, in that their relative loss of weight was greater than that of the girls. On the tenth day, the number of those who had increased was nearly the same in both sexes ; but the condition was so far more favourable in boys, that the increase was greater and the loss compared with the weight at birth less than in girls, so that the loss of weight appears to be more rapidly compensated in them.

In Table XIV. the comparison between the children of *primiparæ* and *multiparæ* is shown.

TABLE XIV.

—	PRIMIPARÆ.		MULTIPARÆ.	
	Grammes.	Number.	Grammes.	Number.
Collective loss of weight	226.4	28	218	22
Increase on tenth day .	190	17	163.8	13
Decrease on tenth day .	171.8	11	118.7	8
Stationary on tenth day .	—	—	—	1

The loss of weight was less in *multiparæ*, both absolutely and relatively, for the average weight of the children was 3362 grammes, and the loss of weight  $\frac{1}{5}$ th or 6.48 per cent. In *primiparæ* the average weight was 3123 grammes, and the loss of weight between  $\frac{1}{5}$ th and  $\frac{1}{4}$ th, or 7.2. The

increase of weight also began somewhat earlier in *multiparæ* than in *primiparæ* (after 4 and 4·2 days respectively). On the tenth day, however, the condition does not appear to be so favourable as might be expected; for a slightly greater number of firstborn children show an increase up to the weight at birth, and in those who have not reached this point on the tenth day, the diminution is greater in *primiparæ*; but, on the other hand, also the increase is greater among them than among *multiparæ*. This proportion does not quite agree with former researches, which, besides the smaller loss of weight and the earlier beginning of increase in the children of *multiparæ*, also as a rule indicated that several of these had on the tenth day exceeded the weight at birth in a higher degree than the children of *primiparæ*.

To ascertain the influence of the original weight on the variations, I have divided the 50 children into three classes in the following table. (The pounds are Danish.)

TABLE XV.

	Under 6 lbs.		Between 6 and 7 lbs.		Over 7 lbs.	
	Gr.	No.	Gr.	No.	Gr.	No.
Collective loss of weight	208·6	14	223·5	21	235·3	15
Increase on tenth day .	212	4	161·3	15	188·1	11
Decrease on tenth day .	131·1	9	143·3	6	200	4
Stationary on tenth day .	—	1	—	—	—	—

According to this table, the loss is greatest in the stronger children, but only absolutely: for, in relation to the body-weight, it is greatest in the first division, the least developed children, in whom it is  $\frac{1}{13}$ th or 7·7 per cent., and in the second class it is  $\frac{1}{14}$ th or 6·9 per cent. of the body-weight; and it is least in the third class, the strongest children, being in them  $\frac{1}{15}$ th or 6·3 per cent.; and on the tenth day a large proportion of these weighed more than at birth.

It might be expected that immature children would show the most unfavourable conditions as far as loss of weight is

concerned. As from the first I left these out of calculation, I only weighed seven, born from two to six weeks before term. Their average loss of weight was 185 grammes, an apparently low figure in comparison with mature children (222·8), but their average body-weight was 2600 grammes, and the loss 7·1 per cent., being less than in mature children under 6 lbs. ; but with a greater number of immature children the proportion would be probably changed, for the absolute loss of weight might be expected to exceed 185 grammes. On the tenth day, four were on an average 155 grammes heavier than at birth, and three were 263 grammes lighter. In regard to the absolutely small loss of weight this condition is very unfavourable, as from the cessation of loss up to the tenth day there was a relatively smaller increase or even a further fall.

The collective loss of weight in the 50 healthy children born at full term of healthy mothers, the average of which was 222·8 grammes, agrees pretty closely with what has been shown by other observers.

In considering the conditions on which the loss of weight is supposed to depend, I will first take the detachment of the umbilical cord. In 96 children the average period of the fall of the cord was 5·6 days. In 50 healthy children it fell off in one on the third day, in 11 on the fourth, in 22 on the fifth, in 12 on the sixth, and in four on the seventh day. The average was 5·14 days ; and as the average time when the increase of weight began was 4·1 days, no connexion between these two processes can be seen to exist.

The two conditions to which may be awarded the chief influence on the loss of weight during the first days of life, are, on the mother's side, the changes in the quality and quantity of the milk, and on the child's side, the relatively great amount of evacuation, especially from the bladder and bowels. To ascertain the influence of the first of these conditions, I had sixteen of the children suckled immediately after birth, and during the first two days by women who had been confined four or five days previously, and whose secretion of milk was in full activity. In connexion with this subject, I have often become convinced that as a rule the child shows



a marked aptitude for sucking immediately after birth, and that the alleged inability is certainly to be attributed more to the mother's inexperience than to that of the child. The following table shows the conditions of weight in the children treated in the way described, compared with those who were suckled by the mothers alone.

TABLE XVI.

—	Collective Loss.	Day of com- mence- ment of Increase	Increase on Tenth Day.		Decrease on Tenth Day.	
			Grms.	No.	Grms.	No.
16 children fed by mothers and nurses	236·2	4·6	205	8	147·5	8
34 children fed by mothers alone . .	216·5	3·8	169	22	160·3	11

In one of the 34 children the weight on the tenth day was the same as at birth.

The result is quite opposite to what might be expected, for the loss of weight was both absolutely and relatively greater (7·4 per cent. in the first and 6·6 in the second class), and the time at which the increase began was later in the children placed in the first category; and on the tenth day a comparatively smaller number (50 per cent.) exceeded the weight at birth, while 64 per cent. of those suckled by the mothers alone had reached this point. As has been already mentioned, Haake found in children who were immediately put to the breast quite as great a loss of weight as in others; and it seems to me that the essential cause of loss of weight which he sees in the defective assimilating power during the first days of life, and the consequent uselessness of giving the child stronger nourishment during this time, is quite valid in so far as the loss is a physiological process which cannot be evaded. It may be objected against researches made in a lying-in institution that the conditions are not normal, as the persons who seek admission are in many respects below the healthy standard; but to this it may

be replied that the loss of weight under conditions as similar as possible, the mothers and children being healthy, was pretty constant, and not liable to great variations ; and the greater number of the mothers of the children which were examined were young and apparently strong individuals, nor have I observed any difference between legitimate and illegitimate children. The circumstance that children who, immediately after birth, were put to the breast of nurses having a good supply of milk, showed quite as great a loss of weight as the others, appears to me to be opposed to the view that defective nourishment is a cause of the loss.

In a treatise on the placenta (*Archiv für Gynäkologie*, Band iv.) Winckler expresses the opinion that a modification of the usual manner of managing the umbilical cord would produce a favourable result as regards the loss of weight after birth. According to Winckler's representation, the aortic pressure falls considerably at the first inspiration, and the arteries of the umbilical cord and chorion contract, the blood in the placenta is driven through the capillaries into the veins, and so back to the child ; and this movement is favoured by the suction-action of respiration on the blood in the umbilical vein. But this blood becomes useful to the child, and is of importance to it ; the more so as, after the sudden filling of the pulmonary circulation, the child is for a moment in a state resembling that produced by rapid arterial hemorrhage (and Winckler says that he has not unfrequently seen dyspnea corresponding to this anemia in children ; that is to say, the first tranquil respirations suddenly changed their character to that of extreme dyspnea, with convulsive contractions of the cervical and pectoral muscles, strong heaving of the thorax, and violent reflex movements of the extremities—symptoms which first begin to disappear when the circulation is equalized). When the umbilical cord is immediately tied, according to Winckler, a certain quantity of blood—namely, that which is in the placental capillaries and veins, and which would go back, is cut off from the child ; the amount of blood which at the same time goes from the child to the umbilical arteries is much reduced as soon as respiration has begun, by reason of the

contractions of the vessels. He therefore advises that the cord should not be tied until the placenta is expelled, or at least until the cord is relaxed and pulsation in it has ceased ; and he expresses the opinion that this *plus* of blood will be advantageous to the new-born child, which will thus possibly more easily compensate the consumption during the first days after birth.

In ten children the umbilical cord was treated in the manner recommended by Winckler, the cessation of pulsation being waited for, which took place in from two to six minutes. The pulsation was observed to cease gradually from the placental end of the cord to the umbilicus. In most of the cases, the placenta was in the vagina when the cord was tied.

As regards weight, these children showed just as much loss as all the others previously examined. The average loss was 223·3 grammes, nearly the same as was observed in the fifty children. The loss in proportion to the average weight of the children was 6·50 per cent., which is less than in the other children (6·84 per cent.). Setting aside the fact that the number is too small for drawing conclusions, this slightly more favourable condition may be explained by the circumstance that all the ten children were well developed, their average weight being 3430 grammes, four of them weighing seven pounds, and six between six and seven pounds ; they thus belong to the second and third divisions in Table XV. The duration of loss of weight was certainly unusually brief in these ten children, inasmuch as the increase began at 3·2 days, which is earlier than in the other children of corresponding weight (4·1 and 3·9 days respectively for those weighing from six to seven and above seven pounds). On the tenth day, six had gained an average of 212 grammes over the weight at birth, one was stationary, and three showed a decrease of 86 grammes : a result more favourable than in the other children with which these are compared.

As regards the child's evacuations, it has been attempted to ascribe to them the loss of weight during the first days. The vernix caseosa is not taken into account, as the children were not weighed before its removal ; besides the

loss by respiration and perspiration, the urinary and alvine discharges come extensively under consideration, and special importance is attached to the meconium. Allix (*"Etudes sur la Physiologie de la Première Enfance,"* Paris, 1867) estimates this at about 60 grammes in the first two or three days; on the following days, according to Bouchaud, the feces are estimated to amount to 80 or 90 grammes daily. It is naturally very difficult to determine the quantity of urine in the new-born child. Allix gives it as 12 to 36 grammes daily, until the lacteal secretion is established; when this takes place, the quantity rises to 100 or 200 grammes, or even higher, in proportion to the amount of milk taken in.

In order to ascertain the relation between the evacuations and the loss of weight during the first days, I endeavoured to determine the amount of the former in twenty-four children in as similar conditions as possible. It is difficult to accurately determine the quantity of meconium, as there is a gradual transition between the dark-green meconium and the natural excrements; and therefore I have endeavoured to determine the amount of excreta during the first days by collecting in napkins, which were previously weighed when quite dry, the amount of urine along with the meconium and the excrements mixed with meconium. The napkins were weighed after removal from the children; and although I admit that an accurate idea of the excreta is not obtained in this way, I yet believe that the comparison of the weight thus obtained with the child's loss of weight in the corresponding time, gives a result indicative of the relation between the two. The following table shows this with regard to twenty-four children. The time within which the loss of weight and the weight of the excretions is compared is up to the weighing on the second day; that is, in some cases the beginning, in others the end, of the second day of the child's life.

The average weight of the excreta was 79·2 grammes, and the average loss of weight 185·8 grammes; in several cases, the latter was three or four times as great as the weight of the urine and meconium. It is then incorrect to simply ascribe the loss of weight to these evacuations; for, even if we take into account the difficulty of collecting all the excretions,



there still remains the fact that not more than half the loss of weight corresponds to them.

TABLE XVII.

No.	Weight of Excreta.	Loss of Weight.	No.	Weight of Excreta.	Loss of Weight.
1	50 grms.	140 grms.	13	60 grms.	130 grms.
2	70 "	390 "	14	90 "	100 "
3	40 "	190 "	15	130 "	190 "
4	130 "	140 "	16	70 "	200 "
5	70 "	150 "	17	60 "	90 "
6	90 "	150 "	18	80 "	200 "
7	40 "	230 "	19	40 "	180 "
8	100 "	260 "	20	120 "	320 "
9	40 "	130 "	21	100 "	170 "
10	50 "	100 "	22	50 "	140 "
11	70 "	160 "	23	190 "	370 "
12	80 "	130 "	24	80 "	200 "

As regards the amount of discharge by respiration and perspiration during the first days of life, there are no fixed data. From researches on large and small animals, on adults, and on children six or seven years old, Allix comes to the conclusion that the amount of oxygen taken in, and of carbonic acid and water discharged by the lungs and skin, is greater in proportion to the weight of the body the younger the child is (according to Bouchaud, the loss by respiration in a new-born child during twenty-four hours is estimated at 45 grammes, and that by perspiration at 55 grammes). The lowering of temperature through the skin in the child is in direct proportion to the extent of surface of the body, and this is relatively greatest in the new-born child. Now, as its temperature is not remarkably lower than that of the adult, it must, to preserve the same temperature, produce more warmth, and the change of tissue must proceed with greater energy. The child may take in relatively more oxygen and discharge more carbonaceous and nitrogenous substances than the adult, and this relatively greater discharge may be derived

either from its own tissues or from the food. As regards the latter of these, it is naturally very difficult to determine accurately the quantity of milk which a child normally takes during the first period after birth. According to Bouchaud's researches, a child usually takes the breast ten times a day ; and he gives the amount as 30 grammes on the first day, 150 on the second, 400 on the third, and 550 on the fourth day ; 650 grammes a day in the first month, 200 in the second, 850 in the third, and 950 in the fourth.

In an article by Dr. Krüger of Dresden (*Archiv für Gynäkologie*, Band vii.), who attempts to determine these quantities, the first inquiry is, how often the child sucks. He found (in 150 cases) that the child does not suck on the first day in 44 per cent. of *primiparæ*, and in only 10 per cent. of *multiparæ*. Among the first, the child sucks on an average once ; in the second, twice, in the course of the day. In both alike, he found that the child sucks on the second day six times, on the third and fourth days eight times, and on the following days up to the tenth nine times a day. In order to determine the quantity of milk used by the child, he has weighed the children before and after sucking every day up to the tenth, under conditions normal and as similar as possible. As the child was not weighed at each sucking, but once a day, the quantity of milk ascertained to be taken being 20 grammes, and the child being applied to the breast eleven times, so that the quantity during the day was 220 grammes, and as the quantity was, in like manner, on the following days estimated at 1000 to 1100 grammes daily, it must seem to be an arbitrary conclusion that the child must suck the same each time. Krüger gives the normal quantity of milk taken by the child as a little over 1 gramme on the first day in *primiparæ*, and 15 grammes in *multiparæ* ; in both indiscriminately, on the second day, 96 grammes ; on the third, 192 ; on the fourth, 234 ; on the fifth, 363 ; on the sixth, 441 ; on the seventh, 501 ; on the eighth, 518 ; on the ninth, 621 ; on the tenth, 648 ; and on the eleventh, 705. The last is regarded as the normal quantity in the child's first month. The numbers for

the first days are smaller than Bouchaud's, who calculated that the child was applied to the breast ten times daily, but for the rest used the same method, that of multiplying together the number of suckings and the quantity of milk ascertained at one time. Krüger believes that the loss of weight is only apparent, depending only on the amount of excretion, and that a mature healthy child increases in weight immediately after birth ; and that the deficiency is not produced because the mother has no milk, but because the child does not know how to suck the milk which exists. He adduces two examples in which the child was applied to the nurse's breast five or six times daily for the first two days ; one child took more than 400 grammes, and the other more than 300 grammes of milk ; the respective loss of weight up to the end of the second day was 120 and 190 grammes. If we follow the author in ascribing the loss of weight to the excreta alone, it must still appear surprising that with so abundant a supply of food the child did not make up the loss ; if the excreta alone were the cause, we might at least expect that the child would remain of the same weight.

I have therefore examined some children who were applied to the nurse's breast in the earliest period after birth a greater number of times than those referred to in Table XVI.

1. Boy, weighing 2970 grammes. Fifty-seven hours after birth he had been applied to the breast fourteen times, and apparently sucked well ; he was weighed twice, before and after sucking ; the quantities of milk taken were respectively 10 and 20 grammes. In the same period the loss of weight was 170 grammes, and the quantity of meconium and urine 95 grammes.

2. Boy, weighing 4000 grammes. Forty-four hours after birth he had been put to the breast ten times, and sucked well. Loss of weight, 340 grammes ; meconium and urine, 90 grammes.

3. Boy, weighing 4080 grammes. Thirty hours after birth he had sucked eleven times. Loss of weight, 190 grammes ; meconium and urine, 100 grammes.

4. Girl, weighing 3800 grammes. Fifty hours after birth had been put to the breast twenty times. Two weighings showed an intake of 15 and 20 grammes of milk. Loss of weight, 160 grammes ; meconium and urine, 120 grammes.

5. Girl, weighing 2730 grammes. Twenty-four hours after birth had sucked nine times. Two weighings showed 15 and 20 grammes of milk to have been taken. Loss of weight, 80 grammes ; meconium and urine, 50 grammes.

In these children the quantity of milk taken in was certainly greater than in the children who were first put to their mothers' breast when the lacteal secretion had become active, and certainly not less than on the third or fourth day ; and yet in all there is seen a loss which is not in proportion to the amount of excrement and of food taken in. If we now admit that the loss is greater on the first and second than on the third and fourth days, the difference can yet certainly be scarcely accounted for alone by the circumstance that an abundant supply of food in the first days did not have a somewhat better result, while the supply on the third and fourth days, though certainly not greater, produced a compensation of the loss and an increase of weight. If the child's excretions are relatively greater the younger it is, the amount of ingesta should also be greatest in earliest period after birth in order to produce compensation. This cannot be the case, as the appearance of the lacteal secretion in parturient women shows. One might certainly attribute the loss of weight to defect of food during the first two or three days, in considering the different conditions between the new-born child and the young of mammalia ; but if this were the only condition, we might still expect that children who were put to the breast of nurses with an abundant flow of milk seven or eight times a day would show a condition varying from the normal with regard to the loss ; if this be not the case, it must be due to the fact that the child does not receive benefit from the ingesta, partly in consequence of the deficient assimilating power of the digestive canal, and that the loss of weight is a physiological process which neither can nor will be superseded.

The normal loss of weight, which in healthy children



suckled by their mothers under the normal conditions of the puerperal state is so marked that scarcely 60 per cent. on the tenth day exceed the weight at birth, becomes naturally of great importance when the loss, in cases of disease of the children or mothers, is not compensated, but is both great and of long duration. I will, in conclusion, bring forward a few facts which show this.

In the cases of 19 women suffering from puerperal disorders, the children showed an average loss of weight amounting to 233·3 grammes, apparently not greater than in the normal condition; but the puerperal disorder generally began after the end of the period of loss, and the unfavourable influence on the mother's side was shown especially in the conditions on the tenth day. At this time, of the 19 children only 4 were on an average 142 grammes heavier than at birth, while in 15 there was an average decrease of 166 grammes.

As regards the diseases of the children, dyspepsia and diarrhea especially, which during one month occurred to a great extent among the children under examination, exercised an extraordinary influence on the weight, though the duration of the illness was rarely more than three or four days, inasmuch as the loss of weight from day to day exceeded 200 or even 300 grammes in some cases. In 14 children with dyspepsia or intestinal catarrh the average loss of weight was 317 grammes, corresponding to  $\frac{1}{11}$ th of the body-weight, which was 3500 grammes; the loss was so far from being compensated on the tenth day, that only two exceeded by 75 grammes the weight at birth, the remainder showing an average deficiency of 228 grammes; a proof that an acute disorder of the intestinal canal, even though quickly removed, has an important influence on the growth and state of health of the child.

The collective results of these researches on the loss of weight in infants are, in short, as follows. Every child loses weight during the first days of extra-uterine life. An increase may occur on the first day, if the meconium have escaped during birth, or if it have not been discharged before the first weighing and the child have sucked in the meantime; but this increase is only transient, and the loss of weight shows

itself at the second and third weighings. The loss corresponds to  $\frac{1}{14}$ th or  $\frac{1}{15}$ th of the body-weight, and is both absolutely and relatively greater in the children of *primiparæ*. Contrary to most other investigations, it was found to be greater in boys than in girls, but was more rapidly compensated in them. With regard to the original weight of the body, the loss is greater the less developed the child is, and in the same proportion the increase is more delayed ; premature children show a corresponding relation. The increase begins as a rule on the fourth day, while other observers have named the third day as the turning-point. As regards the conditions having an influence on the loss, no connexion can be found between the detachment of the umbilical cord and the commencement of the increase ; but the chief importance must be attached to the relation between the egesta and ingesta. We cannot explain the loss by the discharge of meconium and urine alone, for not more than half of these in a given time correspond to the loss of weight. The remainder of the loss must be ascribed to a consumption on the part of the child, and at first sight too small an amount of ingesta, or defective nourishment, might indeed be regarded as the cause of this ; but as abundant a supply of food as the child can use does not compensate the loss. The food administered in the earliest period after birth cannot benefit the child (*i.e.*, by increasing its weight), but the consumption of tissue and the consequent loss of weight are a physiological necessity.

---

## REMARKS ON AN UNSUSPECTED CAUSE OF DEFECTIVE HEARING AND OF DEAF- MUTISM IN INFANCY AND CHILDHOOD.

By JAMES PATTERSON CASSELLS, M.D., M.R.C.S. Lond., &c.  
Surgeon to, and Lecturer on Aural Surgery at, the Glasgow Dispensary for  
Diseases of the Ear.

THE object of this communication is to call the attention of those of the profession whose province it is more especially to treat the diseases of early life, to what in my special ex-

perience is a common but unsuspected cause of defective hearing and of deaf-muteness in infancy and childhood, and to its appropriate and successful treatment.

In selecting the pages of this journal as the more fitting medium by which to convey to my professional brethren the above information, I am constrained to do so because I feel satisfied, after a not inconsiderable experience, that the form of ear disease and its consequences, to which I shall presently more fully refer, are curable—indeed, only curable—by treatment of the simplest kind, at a period when such cases do not usually come under the notice of the aural-surgeon.

The cause to which I refer is that form of simple nasal catarrh,—coryza—so common an affection among infants and children, and named by nurses and other non-professionals “the snuffles.”

The mode in which this simple and apparently harmless disease brings about so serious a change as defective hearing, and in not a few cases deaf-muteness, will be considered by-and-by; meantime, I may state that the examination of several hundreds of cases of ear disease in children under four years of age, among whom I include upwards of thirty deaf-mutes, confirms me in the opinion, that the initial step in the causation of the tissue changes in the ears leading to such dire results as are mentioned above, was an attack of this form of simple nasal catarrh.

The history of these cases of defective hearing or of deaf-mutism, when they come into the hands of the specialist, is pretty similar the one to the other. Usually the age of the patients is about three years, and the parents, hitherto, have regarded them as “inattentive,” or “late in speaking,” notwithstanding their lively and active habits and utterance of mono-tones. The family history is unimpeachable; if there are other children in it, they hear and speak quite well; the child, moreover, has always been healthy, and no cause can be assigned by those in charge for the defective hearing, or deaf-muteness, as the case may be. If, however, the history of such cases is closely inquired into, it will be found that, at some period or other, the child suffered once, or perhaps several times, from a “trifling cold in the head,” and the

nose "ran with water;" that the child, if suckled, could not grasp the breast of the mother, or, if weaned, was unable to breathe through the nostrils, and when asleep lay with open mouth, and snored loudly. With such a history, the suspicion which is awakened in the surgeon's mind, even though it has not been already aroused by the expression of the child's face, is made a matter of certainty after he has investigated the condition of the naso-pharyngeal mucous membrane; there, he finds evidence of the most conclusive character, that the "trifling" cold in the child's head has been an attack of nasal catarrh.

How, one may inquire, does this form of catarrh of the nasal passages damage the apparatus of hearing so as to cause the serious consequences already named? To understand the explanation I have to give in reply to this question, I have, in the first place, to ask the reader of this paper to perform the following experiment upon himself:—Let him close the anterior orifices of the nasal passages by compressing them with the finger and thumb of one hand, and while they are so closed, let him perform the act of deglutition several times in succession, then note his sensations in regard to his ears.

His perception of external sounds will be considerably impaired; indeed, it may be completely lost for the time, and, in addition, there is a perception of internal sound—subjective tinnitus—of a disagreeable and distressing kind, and other symptoms not needing to be named here. Now, so long as the above closed state of the nostrils remains unchanged, these symptoms continue unrelieved, but the instant that the nostrils are released from the compression, and their orifices are opened, one or two acts of swallowing suffice to make the symptoms above named disappear.

Now, what has taken place in the above experiment to disturb the normal function of the organ of hearing is this; the normal intra-labyrinthine tension, necessary for perfect hearing, was increased by the disturbance in the balance that existed between the intra- and extra-tympanic air-pressures before the experiment was instituted, and the result of this disturbance was, and always is, impairment of the function



of the organ, as I have indicated ; in other words, the air within the tympanum has been sucked out of that cavity by the successive acts of swallowing with the nostrils closed, hence the increased pressure upon the labyrinth, and the defective hearing and other symptoms. As all that I have stated above in connexion with the experiment is capable of the most conclusive demonstration and proof, I do not consider it necessary to take up space here by entering further upon this part of the subject. It is sufficient for the purpose of this communication to state that the changes brought about in the ears, in a case of coryza, so soon as the nostrils become closed by the tumefied tissues, are precisely similar to those which were produced in the above experiment ; with this difference, however, that the former are not relieved at will as the latter are, but they intensify themselves with every act of swallowing, and thereby more surely become permanent.

If the catarrh is severe enough to close completely the nasal passages, so that breathing through them is rendered impossible for many days or weeks, as is frequently the case, such a state of matters is fitted to excite our liveliest fears, for it may cause deaf-muteness at once in a young child, while in one somewhat advanced it is certain to cause great impairment of the function, and probably subsequent loss of speech, if the disturbance in the balance of the tympanic air-pressures is unrelieved. That Nature is frequently unable to effect this relief unaided is a matter of common experience and every-day observation on my own part.

Now, what is the most rational mode of treatment which such a state suggests ? Besides the measures that are necessary to remove the congestion of the tissues of the nasal passages, it suggests undoubtedly the pressing need of restoring the normal balance that existed between the air within the tympanum and the external atmosphere, previously to the onset of the disease. But this indication of treatment, simple though it is to execute, to be effective must be used as I have already stated at the very outset of the disease, after the more acute symptoms of the catarrh have

subsided, otherwise it may fail. It cannot succeed if, as in many of the cases which I have seen, the defective hearing or deaf-muteness has lasted one or two years, or it may be even longer. Then, though you may overcome adhesions and obstructions in the Eustachian canals, you fail completely to relieve the imprisoned stapes, and to make mobile the stiffened joints of the *ossiculæ*; the patient remaining defective in hearing or deaf-mute, as the case may be, during life.

The means which I have to recommend, in addition to the local measures usual in such cases, is as follows:—supposing that you have a little patient with closed nostrils, from whatever cause, and that the function of hearing has become impaired, blow a current of air forcibly through the nostrils two or three times daily till you feel satisfied that such a proceeding is no longer required, by reason of the complete restoration of the function. This performance is easily effected by means of an ordinary india-rubber enema bag (No. 6 size), the nozzle of which can be introduced into the anterior orifice of either nostril. Compressing the nostril into which the tube is inserted with moderate firmness, and closing the opposite one with the finger and thumb of one hand, with the other grasping the bag itself, you squeeze it sharply, but not too violently, and so force the air which is contained in the bag along the nasal passage and into the tympanum; of course it is necessary to see that the mouth of the little patient is kept firmly closed during this act of inflation. If the child is of an age that it can give expression to the sense of relief which the above simple operation deftly performed is capable of affording in such cases, its remarks will be pleasant music to the ear of the medical attendant, or if too young to do so at the time, the lively expression of countenance which follows after two or three days of this treatment is no less pleasant to him, if it is less demonstrative.

As I have already said that these cases are only curable at their outset, and as the simple means above described suffice to accomplish this desirable result, I am not without the hope

that the information which I have given in this paper, free from special technicalities, may enable those with whom the responsibility rests to treat promptly, and therefore successfully, a class of cases which for good and regrettable reasons do not turn out well in the hands of the aural-surgeon.

---

## ON LACERATION OF THE GRAVID UTERUS.

By J. ASHBURTON THOMPSON,

Fellow of the Obstetrical Society of London, and Surgeon-Accoucheur  
to the Royal Maternity Charity.

(Continued from p. 729.)

I PROPOSE to conclude this paper with a few remarks upon the medico-legal aspect of rupture of the uterus during labour.

Writers on medical jurisprudence do not discuss this subject at length ; even Casper has but little to say upon it. No doubt the same reason which that writer assigns for the brevity with which he refers to it—that each case must be decided upon its own merits judged by the history of the patient, of the labour, and of the attendant's action during the latter—has influenced other authors. But a just decision cannot be arrived at without a more special knowledge of the subject than is given in any one work, and I propose to recapitulate very briefly the causes which may result in rupture. These, in the absence of direct and very distinct evidence of violence inflicted during delivery by the operator, must be excluded before it can be affirmed without hesitation that he is responsible for the laceration. Just as the foregoing analysis shows that rupture of the uterus is an injury to discover which during life often requires as much vigilance and acumen as many diseases ordinarily accounted obscure, so a consideration of the numerous circumstances which may cause or contribute to rupture shows at once the difficulty which exists in proving the responsibility of the accoucheur for its production. Unless the latter has obviously caused or contributed to the injury (as by cutting off prolapsed intestine),

or has inflicted direct injury with an instrument (as by thrusting the forceps into the abdomen through the vaginal roof), it will almost always be difficult to say that the laceration did not arise from natural causes. And even if it should be proved to reasonable satisfaction that the symptoms first and suddenly arose during extraction, there may be, even in that case, many circumstances which would render it unjust to condemn the operator; and these circumstances can only be ascertained by a more complete post-mortem investigation than is usually made. I shall occasionally notice the case to which I referred in the first part of this paper, since it affords a striking example of the justice of the foregoing observations.

Save under exceptional circumstances, such an inquiry as this is most difficult to be carried to a satisfactory conclusion; and it may even be asserted that in the absence of positive evidence it is impossible to be certain of the cause of rupture. Positive evidence may tell either way—for or against the accused; and that it may be derived both from the history of the case and from the dissection of the body indicates a conclusion which will presently be drawn—that the history and the results cannot be considered separately, but that the one illustrates the other. Thus positive evidence for the accused is offered, if from the history of the case it should be apparent that collapse supervened before he had interfered with the patient, and if he should be able to affirm that on account of it he hastened delivery; but if it should appear certain that during the operation of version the patient suddenly experienced the characteristic pain of rupture and immediately became collapsed, then there can be no room for doubt that the laceration occurred during the operation. So in the dead body, should the uterus be found seriously diseased at or about the seat of injury, that may render it unnecessary to inquire further of the conduct of an accoucheur of ordinary care and ability; but the wounds found may obviously be stabs inflicted by *spiculæ* of bone in course of removal during the performance of craniotomy; and that is evidence of the contrary sort. Thus it seems that most often it will be only by a process of exclusion



that in the more doubtful instances the best opinion can be formed. In such cases, in order to establish the culpability of the attendant, one has a right to demand that it should be shown first of all that no predisposing cause of rupture existed ; and only in the second place will it be necessary to show that the determining cause was some other than the contractions of the uterus itself. These two points involve a variety of considerations which, if the reports already referred to be consulted, appear as a rule to meet with very little attention.

The most remote predisposing causes of laceration in point of time are laceration at a former delivery, Cesarean section, and peri-uterine inflammations. Of these the former are necessarily rare and can scarcely escape observation ; the latter may involve a somewhat minute examination of the past history of the patient and the condition of the abdominal viscera. If there have been inflammation of very old date, then it may have given a predisposition to rupture by destroying the structure of the part affected, and rendering it incapable of development during gestation. But if it be more recent then it may alter the affected part in the contrary way—by softening it, and rendering it unfit to withstand the strain of labour. But such a spot may occupy a small area only, and it may be necessary to search carefully for it ; and on that account no search of this kind can be deemed complete unless the borders of the laceration be not merely included in it, but their condition made the subject of special inquiry. The laceration may have begun in a small spot thus weakened by disease, and have subsequently extended itself through the whole length of the uterus ; the whole border of the wound (but perhaps the angles or extremities most particularly) must therefore be examined both generally and microscopically. Such an inflammation may be the result of direct injury to the abdomen inflicted during pregnancy ; but when such injury is more recent it may by bruising or even lacerating some of the uterine fibres constitute a predisposing cause of complete rupture. The uterus may thus become so softened at a particular spot that its own contraction shall be sufficient to complete or deter-

mine laceration. In that case not only will the uterus offer signs which may be detected with care, but the abdominal wall itself would probably still offer some evidence of the injury. But inquiry into injury inflicted during pregnancy should not stop at such as is the result of direct violence or of actual assault, for many cases of rupture are recorded in which from the apparent insignificance of the cause the idea of rupture from violence would, *à priori*, be negatived. Thus in Case No. 12 and in another related by Perfect it seems possible that the pressure which sudden action of the abdominal muscles may exert upon the gravid uterus may constitute a sufficient predisposing cause of this accident. In the first case the patient stumbled—it is not said she *fell*—in crossing a railway; and in suddenly saving herself from actual prostration no doubt a partial rupture was effected which only needed labour to complete it. Case No. 1 is similar to this, although the patient did actually fall; for she slipped downstairs without striking the abdomen, and did not offer the symptoms of rupture for some days afterwards, when they were manifested during labour. Emotional contraction of the uterus without any direct violence is an adequate cause of complete rupture during pregnancy; as in Case 5, in which the patient sustained laceration during a thunderstorm which very much alarmed her. Last among the predisposing causes operating during pregnancy comes that assigned by Denman and called by him attrition. By this is intended a continued pressure exerted upon the uterus by pressure of the fetal head against some unduly prominent part of the pelvic brim. But though this might possibly cause atrophy of the part implicated, yet to that wasting must be added the peculiar tension to which a continuance of the same condition must expose the organ during labour, and to which more particular reference will be made below. Various diseases experienced during pregnancy may also act as predisposing causes of rupture. In the first place general disease may modify the uterine tissues so as to unfit it for the strain of its contractions. But from the readiness with which the uterus relieves itself of its contents in the course of wasting diseases, it does not seem probable that it is often

seriously interfered with in this way. It is, however, subject to some special diseases which need to be looked for; they are, thinning of its walls either general or in patches; degeneration of its tissue of a kind similar to that which it undergoes during the normal process of involution; and a species of gangrene such as is described in Case 11. All of these may be detected easily enough upon a proper search; but the point before noticed seems to me important enough to be repeated here—that the whole border of the wound should receive a special and minute examination over and above any general examination of the uterine walls; and that this is all the more important when the rest of the uterus appears healthy. Lastly, it is not to be forgotten that the healthy uterus may spontaneously rupture. Dr. Barnes has very emphatically stated that in more than one case of spontaneous laceration he has submitted the organ to careful examination himself, and has corrected his observations by those of other experienced microscopists upon the same specimens; and that it was not possible in any of those cases to consider the uterine tissue other than healthy.

Such is a brief review of the more important conditions which, occurring during pregnancy, conduce to rupture when labour sets in. It shows that it is not sufficient to examine a suspicious case in the light of facts revealed by the post-mortem alone; the patient's history must be inquired into, and even if the occurrence of any of the accidents named be denied (and it must be borne in mind that the patient is quite unable to form any estimate of what would be a sufficient cause), yet it will be due to the accused to make the narrowest search for the results of such accidents. With this precaution it may appear that the inquirer might be independent of the history furnished by the patient or her friends. But that is not so; for should the injury received be a partial laceration from emotional causes or general compression of the abdomen, the subsequent completion of the rupture may easily hide the comparatively trifling tear in which it began.

The history of the pregnancy having been examined, and

the narration checked by comparison with the actual condition of the womb, the history of the labour will next engage attention. Of the manner in which that should be regarded, more need not be said than the foregoing part of these pages indicates. Yet it will be as well to repeat the general and most important conclusion to which that section points—viz., that with regard to particular symptoms negative evidence is of no value whatever. To be able to assert that collapse did not occur—that the labour pains did not cease—that no pain was experienced, and so forth, affords no presumption that the condition to which these symptoms are owing *when they do show themselves* had at any particular time not supervened. No symptom, unless it be that of collapse, can be considered essential to rupture of the uterus ; and to pretend that the time of appearance of collapse bears any relation to the time at which the injury occurs, save that it happens after it, the foregoing analysis shows to be absurd. On the other hand, positive evidence may be obtained from the history of the labour, should the case have been a tolerably clear one, which is of course of great value. When we are informed that a person was taken in labour at three o'clock in the morning ; that she suffered from trifling pains until seven o'clock ; that she then had two strong pains ; that from that time she had no pain, but was soon afterwards found by the accoucheur, who then arrived, to be in such a state of depression that immediate delivery was deemed necessary ; and that with all this there was a considerable discharge of blood from the vagina before delivery, it is tolerably plain that the case was one of rupture of the uterus from natural causes, to relieve the effects of which delivery by version was at once resorted to. In that case the history of the labour should have been sufficient to exonerate the accused. But had the circumstances been somewhat different—had the history, instead of being remarkably clear, been deficient or obscure, or had there actually been no symptoms until after the forcible delivery, it would have been necessary to inquire further into the circumstances of the labour, with a view of discovering any of those conditions which are known to favour spontaneous



laceration. These circumstances are composed of facts derived from the condition of the mother, of the child, and from the relations existing between the two.

Without doubt the most important point to ascertain is the proportion existing between the fetal head and the maternal pelvis, supposing the presentation to be cephalic. It has long been acknowledged that two of the more common causes of laceration of the uterus are hydrocephalus in the fetus on the one hand, or a moderate contraction of the pelvis on the other. It is not in the higher degrees of contraction that laceration most frequently occurs, for it is not by obstruction to the exit of the child, and the consequently futile efforts of the uterus to expel it, that the injury is caused, though no doubt laceration does occasionally occur in that way too. It is that, when there is a trifling contraction, the lowest zone of the uterus is carried down by the fetal head ; it passes upon it through the brim of the pelvis ; it becomes fixed between the head and the sacral promontory, for example ; and then the uterus, in continuing its contractions, instead of pulling upon the circumference of the os and pushing the fetus through it, is obliged to expend its force upon the fixed point in its own wall, which, partly from pressure and partly from actual violence, presently gives way. The same explanation may be given in cases of moderate enlargement of the fetal head. The maternal pelvis must, therefore, above all things, be accurately measured, and should it be found to be of average capacity, then the fetal head must be examined in the same way. Should the pelvis be so contracted as to afford an effectual bar to the exit of the fetus by the natural efforts, or, similarly, should the presentation be found unusual, whether cephalic or of other parts, then the history of the case will call for consideration, with a view of determining what steps should have been taken by the accoucheur to effect delivery. And in this case it may be borne in mind that the present case shows that two spasmodic or violent contractions of the uterus are sufficient to rupture it ; and that, therefore, this accident might happen to a person of irritable constitution even while the necessary preparations for delivery are being

made, and before such delay in interference had been permitted as is usually necessary to set up spasmodic action. From the same case, indeed, it appears that (from causes unfortunately not ascertained in it) these spasmodic pains may occur without any extraordinary delay whatever. Attrition has been referred to above as a predisposing cause of rupture. This operates only when the inlet of the pelvis is either deformed, as by an exostosis, or is the subject of undue development, as when the promontory of the sacrum is too much advanced, or the ilio-pectineal line sharper and more prominent than usual. These are points to be observed in examining the pelvis, for when labour sets in the uterus will be liable to be fixed between the prominence and the fetal head in the same manner as when there is a general disproportion between the two. In such cases as these the uterine tissue may show signs of destruction by pressure if the labour have been prolonged ; but should the throes have been active from the first the tearing may take place at too early a period to allow of such evidence as that.

The part of the child which has escaped into the abdomen should be noted. It may be found that an arm only or a leg has passed through the uterine wall ; and in that case it must be carefully ascertained that the uterus at this point is healthy and of the normal thickness. For, even if such a laceration have occurred during version, disease of the uterus at the injured point must be taken to exonerate the attendant, provided he is a person duly qualified to undertake an obstetric operation. It would scarcely be possible to show, and it would be quite unnecessary to suppose, that he had acted otherwise than with due caution.

Although the position of the laceration is not entirely independent of the circumstances which conduce to it (disease of a particular portion of the wall of the uterus being competent to determine laceration at that part for example), yet no argument as to the manner in which it was immediately caused can be maintained by reference to its situation. When this injury occurs from natural causes any part of the uterus, and even the vagina as well, may be its site ; but the neck or the parts about it seem to be most frequently implicated

in that case. Of the twenty-three cases above enumerated, in three the position of the wound is not noted, while in one it is left doubtful. Nineteen cases remain, in five of which the injury was traumatic. Case 10 (version), the posterior wall was lacerated. Case 15 (attempted version), a lower extremity had perforated the anterior superior part of the uterus. Case 19 (version), a perforation by the finger, and a laceration just above the cervix. Case 20 (craniotomy), laceration by fragments of bone. Case 21 (by fall downstairs), in the right side of the uterus three inches in length. In fourteen the laceration was the result of natural causes. Case 1, cervix to fundus (undelivered). Case 3, longitudinal, several inches (undelivered). Case 4, transverse in front, close to cervix, several inches. Case 5, above the os, about three inches in length. Case 6, transverse, posterior, about the cervix. Case 6A, left side of anterior aspect, perpendicular, and involving the vagina. Case 7, left side of posterior aspect from cervix to fundus. Case 7A, in the median line on the posterior surface, involving uterus, vagina, and rectum. Case 9, posterior wall to the right side. Case 12, posterior surface, longitudinal, upwards from cervix. Case 13, anterior wall longitudinal, five inches. Case 14, left side at the junction of the cervix and body. Case 17, posterior wall. Case 18, posterior wall, transverse. From this analysis it appears that when the rupture is spontaneous it is more frequently placed near the neck (in which case it may either be transverse or longitudinal), but in some instances occupies the wall of the organ, and, so far as the reports go, is confined to that part; the latter cases in this respect resembling two of those in which the injury was traumatic. These cases bear out the commonly received opinion, that when laceration occurs spontaneously during parturition, it is most common for the cervix to be involved; and certainly in cases of doubt that well known fact may be permitted to influence an opinion upon the cause of rupture in a case in which this part of the uterus was torn. It is important to note that in Case 1 the laceration extended from the fundus down to the cervix although delivery was not effected. It is thus possible for the uterus to be ruptured

throughout its whole extent entirely by its own efforts ; and, although in delivering by version it is very likely that a pre-existing rent may be extended, yet the accoucheur cannot possibly be held responsible for such extension.

There is one other predisposing cause of rupture to be added to those named above (from which direct reference to that which consists in cross, or other abnormal presentations has been purposely omitted)—viz., the death of the fetus. It has been pointed out by Dr. Barnes that the body in losing its vitality loses its resiliency. No doubt a living child offers some resistance to the contractions of the uterus which the latter gradually overcomes ; and it is this “buffer”-action of the child which is lost upon its death. The state of the child is, therefore, an important consideration in forming an opinion upon the cause of rupture, and should be directly referred to.

Most of the above-mentioned points are well enough known, and plainly enough stated in most obstetric works ; although few, if any, offer a complete or connected account of them. They chiefly concern certain physical conditions of which the bearing they have upon the point under consideration is plain. They are, therefore, not liable to the same confusion of statement or misconstruction of import as may beset accounts of symptoms as they have appeared to various observers. Hence it would be unpardonable if the expert employed for the prosecution in a criminal trial were, for example, to omit to state the proportions which existed between the fetal head and the maternal pelvis, the condition of the uterine walls in the neighbourhood of the laceration especially, or the condition of the fetus with regard to its vitality. The evidence to be given upon these points consists of a simple statement of facts. So with the history of the labour when that comes under discussion. Some latitude of opinion must necessarily be allowed upon the exact value of symptoms gathered from the confused accounts of ignorant, and often vindictive, witnesses. Some latitude of opinion must be allowed, too, upon the interpretation which may be put upon such of those symptoms as it may be agreed are ascertained. But that latitude will



narrow almost in proportion to the special knowledge of the subject which the expert possesses. The position of a witness in cases touching the practical skill, and through it, the reputation of a member of his own profession, must always be one of grave responsibility—but to this (owing to the exigence of circumstances) is sometimes added the anxiety which is entailed when he is called as an expert who neither possesses, nor professes to possess, a special knowledge of the subject in discussion. However distressing such a task may be, the witness will see that he is called upon to discharge a duty which is due neither to the profession nor to the public, but to justice ; and in order to discharge it to the best of his ability he will demand such opportunities of research and consultation as shall at least enable him to avoid assertions which are not so grounded upon fact as to be incontrovertible. To this end he will reflect that text-books, containing as they do classical descriptions alone, cannot afford the data necessary to judge of a particular case which may present any kind of exceptional peculiarity. He will, therefore, refer to such monographs as may have been published upon the subject ; but even from them it will be evident, however complete the information they contain, it will be scarcely possible to derive all the necessary facts. Finally, therefore, he will compare them with those clinical details which his own experience may not afford, but which may be gathered from the reports of isolated cases which appear from time to time in the various journals. Thus prepared, he will be ready to avoid those errors into which, under the special circumstances, it is inevitable mere classical descriptions would lead him. He will be ready to admit, for instance, that complete laceration of the uterus during labour is not immediately, or even very shortly, followed by any degree of prostration at all approaching collapse, except occasionally ; or, that it is not unusual for the accident to be attended by no symptom but what may much more easily and probably be accounted for in some other way, until a period of hours has passed over ; or that laceration of the neck of the uterus is not only *not* presumptive evidence of a traumatic origin, but is, in fact, in

favour of the opinion that it is of spontaneous origin. And hence that accoucheur who has failed to recognise laceration of the uterus immediately upon its occurrence will appear to him to be not so much culpable as the victim of an unfortunate concurrence of circumstances, to which the most careful and far-seeing practitioner is liable.

---

## Reports of Hospital Practice.

---

### ST. MARY'S HOSPITAL, MANCHESTER.

---

- I. *Retroflexion of the Gravid Uterus—Retention of Urine—Miscarriage.*
- II. *Retroversion of the Gravid Uterus—Retention of Urine—Edema of Legs—Albuminuria—Disappearance of Edema—Pregnancy Uninterrupted.*

*Under the care of Mr. CULLINGWORTH.*

I. ELIZABETH B., aged forty-one, the wife of a labourer, was admitted as an out-patient August 10th, 1875, and gave the following history. She has borne seven children at full term, and has miscarried twice. It is upwards of three months since she menstruated normally; a slight sanguineous discharge has, however, occasionally occurred at irregular intervals. Nearly a fortnight ago she felt a "great straining" in the vagina, which sensation has gradually increased. On the 2nd August the urine dribbled during the whole day; on the 4th it was passed voluntarily; on the evening of the 8th she found herself scarcely able to pass any, though the desire was urgent, and repeated efforts were made; and on the 9th she noticed some abdominal enlargement, which led her to consult a medical man. She was examined, prescribed for, and told to apply to a hospital; a catheter was not used.

On the 10th, the day of her admission, the abdominal

enlargement had increased ; she had passed a quiet night, and had twice been able to pass a little urine.

A smooth, oval, fluctuating tumour was found in the middle line, immediately above the pubes, dull on percussion, and measuring  $6\frac{1}{4}$  in. long by 5 in. wide. The posterior cul-de-sac of the vagina was occupied by a smooth elastic tumour, pressure upon which caused a little urine to flow from the urethra. The posterior lip of the os uteri felt soft, flaccid, and swollen, and projected downwards into the vagina, so as to give at first the impression that it was a pendulous morbid growth. The anterior lip was hard and fixed, and from it rigid converging folds passed upwards into the funnel-shaped cervix.

By means of a gum-elastic male catheter forty-five ounces of clear urine were withdrawn. The abdominal tumour entirely disappeared. The uterus, now distinctly felt to be retroflexed, was gently replaced by the fingers, and a Hodge's pessary was introduced to retain it in position ; after the replacement the os and cervix assumed their normal condition.

*Aug. 11th.*—No pain or discomfort ; has been careful to preserve the recumbent posture ; urine has been passed three times since the catheterism, voluntarily. The uterus retains its normal position. On passing the catheter, to insure thorough emptying of the bladder, only six ounces of clear urine were withdrawn.

*13th.*—Position of parts normal ; no discomfort of any kind.

*16th.*—The patient miscarried.

II. Sarah S., aged twenty-eight, married, was admitted an in-patient, December 14th, 1875.

She had borne three children at term, and had once miscarried, eighteen months ago. She last menstruated nearly four months since.

Three weeks ago she experienced a sudden and severe pain in the lower part of the abdomen, producing an urgent call to stool ; nothing, however, passed. A few days after

this she noticed that the abdomen was enlarging, and urine has been dribbling for the last fortnight.

On admission she looked very ill ; the face was of a waxy paleness, the appetite poor, and rest impossible. The abdomen was greatly distended by a fluctuating tumour, dull on percussion. The following measurements were taken :—Girth at umbilicus 37 in., length from pubes to umbilicus 8 in., and from umbilicus to upper limit of dulness 5 in. ; width of the area of dulness at the level of the umbilicus  $13\frac{1}{4}$  in. (7 in. on right,  $6\frac{1}{4}$  in. on left). The patient complained of pain in her back and loins ; there was some edema of both lower extremities. The circumference of the left ankle was eleven inches, that of the right ten. The posterior wall of the vagina was prolapsed, and protruded beyond the vulva ; its mucous membrane had become dry and tender. The external orifice of the urethra was displaced downwards so as to lie just within the vaginal opening. The uterus lay almost horizontally in the pelvis ; the fundus (enlarged to the size usual about the fourth month of pregnancy) occupying the upper part of the hollow of the sacrum, while the cervix was wedged so tightly against the upper part of the pubes that it had become acutely flexed near the os, which looked directly downwards.

A catheter was introduced, and 105 ounces of pale, turbid urine were withdrawn, having a specific gravity of 1004, and a purulent sediment after standing. The bladder was washed out with warm water, and, the patient having been placed in the knee-elbow position, pressure was made by means of two fingers in the vagina. As the uterus did not readily yield, the patient was put to bed, and the catheter ordered to be used every four hours.

During the next twenty-four hours 198 ounces of urine were withdrawn—namely, at 7.30 P.M. on the 14th, 68 oz. ; at 11 P.M., 50 oz. ; at 4 A.M. on the 15th, 24 oz. ; and at 10 A.M., 56 oz. The last specimen had a specific gravity of 1006.

15th.—Noon. The retroversion remained unreduced. An attempt was again made to rectify the position by means



of two fingers in the vagina, the patient on this occasion lying on the left side. This did not succeed ; the whole hand was therefore passed into the vagina, and the fundus gently pushed upwards. No difficulty was experienced in thus restoring the organ to its natural position. A Hodge's pessary was introduced to prevent a recurrence of the displacement.

During the next twenty-four hours the total quantity of urine could not be ascertained ; that it was still large will be seen from the following :—At noon on the 15th, 17 oz. were withdrawn ; at 4.30 P.M., 30 oz. ; at 9.30 P.M., 31 oz. ; the quantity passed voluntarily on three separate occasions was not measured.

16th.—The catheter is no longer used. The urine this morning is slightly turbid, of sp. gr. 1006, and moderately albuminous. The edema of the legs is so far reduced that the circumference of the left ankle is now  $8\frac{3}{4}$  in., that of the right, 8 in. The girth of the abdomen is now 31 in., being less by 6 in. than on admission.

From 11 A.M. on the 16th to 11 A.M. on the 17th the quantity of urine was  $33\frac{1}{2}$  oz., besides a little that was passed during defecation.

17th.—Patient is still pale, but cheerful and without pain : the uterus retains its position ; the urine is rather more albuminous, turbid, pale, of sp. gr. 1010, and deposits a considerable quantity of pus on being allowed to stand.

From noon on the 17th to noon on the 18th 100 oz. of urine were passed.

18th.—Patient feels well, and is losing her extreme pallor. All edema has disappeared, both ankles having now a circumference measuring 8 in. The urine is pale, of sp. gr. 1008, and still somewhat albuminous.

The quantity of urine passed from midday on the 18th to midday on the 19th was 104 oz. ; during the next twenty-four hours, 59 oz. ; and from the 20th to the 21st,  $83\frac{1}{2}$  oz.

22nd.—Pessary remains in good position. The bladder required to be relieved only three times in twenty-four hours ; quantity, 65 oz.

24th.—Pessary escaped during defecation ; it was not thought necessary to replace it.

31st.—Fetal heart heard for first time, the number of its pulsations being 132 per minute. Measurements of abdomen as follow:—Circumference at level of umbilicus, 30 in.; length from umbilicus to pubes,  $6\frac{1}{2}$  in.; from umbilicus to ensiform cartilage,  $5\frac{1}{4}$  in. Position of uterus normal. No difficulty or frequency of micturition. Urine contains more albumen than is accounted for by the quantity of pus present. Under the microscope the sediment is found to consist of pus-cells, both fresh and granular, squamous epithelium, large nucleated cells from deeper stratum of epithelium, and a few cylinder-cells. No casts are discoverable.

Patient left the hospital this day.

*Feb. 11th, 1876.*—Patient presented herself at the out-patients' room. She expresses herself as feeling very well and able to attend to her household duties without difficulty. The uterus is in normal position, and the pregnancy progressing satisfactorily. Urine still turbid from the presence of pus, of sp. gr. 1010, and somewhat more highly albuminous than before. There is no edema, or unnatural frequency of micturition.

---

## THE OBSTETRICAL JOURNAL

OF

GREAT BRITAIN AND IRELAND.

MARCH, 1876.

---

### THE DEVELOPMENT OF OBSTETRIC MEDICINE.

THE Obstetrical Societies of the United Kingdom have at the present moment great responsibilities. Besides their ordinary scientific work, they have entrusted to them the task of directing the vigorous growth of an important branch of Medicine. To their dexterous training is committed the form which this branch shall take, and their successors will award to them praise or blame in proportion to the value of

their action. It is gratifying to see that the Obstetrical Society of London is showing symptoms of being alive to its duties. Special meetings of its Council have been held, and resolutions passed. One has been sent to the College of Surgeons of England, and to the General Medical Council, to the following effect:—That Obstetric science is an integral part of Medicine, and cannot be taught or practised with advantage to the community apart from that full degree of knowledge and skill in Medicine and Surgery which is held to be essential for the safe practice of the healing art; and that the Society regrets that the College of Surgeons is legally required to admit persons to examination for the midwifery certificate who are not otherwise medically qualified, and points out how injurious must be the admission of persons to the medical register who are imperfectly qualified for medical practice. If the Fellows be in earnest in making this protest, it will be difficult for any of them consistently to aid and abet the College, by acting as examiners, in carrying out that which they have condemned and declared as “injurious to the general interests of the public and the profession.” It is to be hoped that the Society will see it in this light, and throw upon Surgeons themselves the responsibility of doing that which would be as distasteful to the general profession as it would be degrading to Obstetricians. Besides this important subject, the Council of the Obstetrical Society has been again considering the wide question of how best to insure the instruction and registration of midwives. The proposals which had already been drawn up for this purpose have been carefully revised, and it has been determined to submit them by deputation to the Duke of Richmond and Mr. Sclater Booth. The regulations concerning midwives abroad, moved for last session, at the suggestion of Dr. Aveling, by Mr. W. H. James, M.P., are now printed, and contain much valuable information for the guidance of those interested. A difficult point connected with this question, and one which should receive the earnest attention of Obstetricians, is, By whom shall midwives be examined and licensed? In all large towns examining boards will be required. Are these boards to be

appointed by the State, the General Medical Council, or by a corporate body? We incline to the belief that the examination and licensing of midwives might be best performed by a special Obstetric corporate body. The necessity for the existence of such a corporation has been long felt. It has several times been under the consideration of the Obstetrical Society of London, and has each time met with the approval of its wisest members. All action, however, in this direction has been delayed until the settlement of the conjoint scheme. This obstacle is now about to be removed, and the time seems most appropriate for reconsidering the subject. A strong necessity, it is believed, exists that there should be a special examination for those who wish to practise or distinguish themselves in the Obstetric department of the profession; and that in the interests of Obstetrics, and for the promotion of Obstetric knowledge a special examining board is required. This opinion has been opportunely endorsed by the examiners and professors of the University of Dublin, who are unanimously agreed that the defect in their midwifery examination for the degree of M.B. should be met as in Surgery by the institution of a degree in Midwifery, this degree to be conferred upon Bachelors of Medicine only after a special examination in midwifery. If Obstetricians would appreciate the signs of the times, and unite in each division of the Kingdom for the purpose of establishing Royal Colleges of Obstetricians, they would probably meet with little resistance. One of their subordinate duties might be the examination and licensing of midwives. They would have no opposition from existing medical corporations, which have always unwillingly listened to their importunities and given them scant assistance. The time has come for Obstetricians to return the crutches which Physic and Surgery have lent them, and boldly walk alone. They have the strength to do it, if they lack the courage they will justly fall to a level, the humiliation of which will be commensurate with their apathy and want of prescience.



## Abstracts of Societies' Proceedings.

---

### OBSTETRICAL SOCIETY OF LONDON.

*Meeting, January 5th, 1876.*

WILLIAM OVEREND PRIESTLEY, M.D., F.R.C.P., *President,*  
*in the Chair.*

---

#### *New Preparation of Ergot.*

Dr. WILTSHIRE exhibited a new liquid extract of ergot, prepared by Mr. Martindale, of New Cavendish Street. It was made with ammoniated alcohol, and was a remarkably active preparation of the drug.

---

#### *Forceps.*

Dr. AVELING presented a pair of his curved-handled short forceps.

---

#### *Cyst of Larynx in an Infant*

Dr. EDIS exhibited a specimen removed post mortem, where death ensued thirty-seven hours after birth, on November 30th, 1875. The mother was delivered at 9.30 A.M. of a living, apparently healthy, well-nourished female child, weighing 8 lbs., and 20 inches in length. The duration of labour was eight hours. The child seemed to experience difficulty in breathing and sucking, and gave vent to a more or less continuous kind of feeble wail. Nothing abnormal could be detected on the most careful examination. It died within thirty-seven hours of its birth, and on examination post mortem a small tense cyst, the size of a small hazel nut, was discovered attached to the side of the larynx, and nearly closing up the aperture. No other abnormal condition was detected.

Dr. BARNES thought the specimen of sufficient interest to be figured in the Society's Transactions.

Dr. SAVAGE seconded the proposition.

---

#### *Carneous or Fleishy Mole.*

Dr. WILTSHIRE exhibited for Dr. Bathurst Woodman a specimen of this nature sent up by Mr. Robert Hughes, of Woodbridge. The patient from whom it was removed was one of fifteen, aged twenty-nine. She had miscarried a twelvemonth before, at about the fifth month. On August 15th, 1875, she was delivered of a full-grown, healthy male child. On examination a large tough substance, about

twice the size of the fist, attached to the placenta, was detected and removed, together with the latter. It closely resembled a large kidney. The points of interest in the case were the hereditary proclivity to plural birth, the patient's mother having had two twin births in succession, and the absence of any known cause for the death of one twin, the other reaching to full maturity. There were no symptoms of syphilis in the parents.

Dr. WILLIAMS inquired if there was any history of threatening hemorrhage or abortion.

Dr. WILTSHIRE said he knew nothing more of the case than had been read. There was evidently an attempted formation of the anus and one or more eyes.

The PRESIDENT remarked that the specimen had somewhat the appearance of a placenta succenturiata. He suggested a further examination of it by Dr. Barnes, Dr. Williams, and Dr. Woodman.

---

*Fibroid of Uterus.—Gastrotomy.*

Dr. ROUTH communicated the particulars of a case of extra-uterine fibroid of large size removed successfully by gastrotomy from a patient aged thirty-two. The tumour weighed over 17 lbs. Five days after the operation the temperature rose to 105° F. in the axilla, the vaginal temperature being 106°·4. She was then placed bodily in an ice-water bath, and kept in for three-quarters of an hour. Consciousness returned, and the temperature sank to 100°, the axillary temperature a quarter of an hour later being 97°. A week subsequently, the temperature having again risen to 104°, the ice-water bath was repeated, which reduced the temperature to 99°. Eighteen days after the operation, about five ounces of intensely fetid, grumous, bloody pus was removed with the aspirator per vaginam, and the cavity washed out with weak iodine. The pulse fell from 112 to 86 the same day, and the temperature in the vagina from 104°·2 to 102°·4, it being 99°·7 in the axilla. The abscess was subsequently opened, per rectum, and a drainage-tube kept in. After rather a protracted illness the patient recovered perfectly.

Dr. ROGERS, who had assisted at the operation and during the subsequent treatment of the case, remarked upon the after treatment being most successful, and thought great credit was due to Dr. Routh for his management of the case throughout.

Dr. BARNES regarded the specimen as one of a fibroid tumour springing from the uterus. In many instances they retained a close union with the uterus. In some cases they seemed to be of ovarian origin, and it was a question whether they could be regarded as uterine.

The PRESIDENT said that Dr. Routh's case was remarkable in more than one aspect, and he trusted that so interesting a communication would not be allowed to pass without full discussion. The points he thought of special practical importance were the success of the treat

ment when the temperature was high and the symptoms threatening, by the ice bath, and the obvious abatement of septicemic symptoms when irritating fluid was tapped from the peritoneal cavity through the rectum.

---

*Malignant Tumour of the Omentum Removed Post Mortem.*

Mr. G. D. BROWN (of Ealing) exhibited a specimen weighing over 17 lbs., which, taken in connexion with Dr. Routh's case, had many points of collateral interest. The patient, a lady, aged thirty-six, had observed the growth of the tumour for at least two years. Opinions had varied as to the nature of the case, but Dr. H. Smith, who had also seen the case, and Mr. Brown, both regarded it as malignant disease of the omentum. She had been under Dr. H. Smith's care, at Soho, where an exploratory incision had been made, when the growth was found to be so extensive and deeply seated as to make its removal a practical impossibility. The pain during the growth of the tumour was at times very distressing. The tumour was firmly adherent to the peritoneum, at the back, but had no other attachment, and shelled out without cutting. In appearance it was nodulated and brain-like in colour. It weighed 17 lbs. 10 oz., and was 24 inches in circumference. The uterus and ovaries, as well as the other organs, were healthy.

Dr. H. SMITH explained that he saw the case when the tumour was quite small. An exploratory incision had been made to see if it could be removed, but he was unable to find any attachments or adhesions.

The Annual Meeting then took place. The List of Officers for the ensuing year recommended by the Council was adopted by the Meeting. The Secretaries', Auditors', and Librarian's Reports were then read and adopted. Votes of thanks to the retiring officers having been proposed and carried, the President delivered his Annual Address.

---

*Meeting, February 2nd, 1876.*

The following gentlemen were elected Fellows of the Society:—Charles J. Brookhouse, M.D. (Deptford), Martin Brunges, M.R.C.S., J. H. Crowdace, L.S.A. (Beverley), I. N. Jakins, M.R.C.S., Leslie Jones, M.D. (Blackpool), Charles Knott, M.R.C.S. (Portsmouth), W. H. Leighton, M.D. (Lowell, Mass.), John H. Parsons, M.D. (Toronto), G. Sirignani, M.D. (Naples), F. Wells, M.D. (Cleveland, Ohio), and J. C. Wilkinson, L.S.A.

*New Chloroform Inhaler.*

Dr. CROMBIE exhibited an apparatus for enabling patients to inhale chloroform by themselves in safety. A spirit-lamp filled with cotton-

wool was used to absorb the chloroform, and so render it practically solid, so that it could not be upset. An india-rubber ball and tube acted as a spray-producer, the patient ceasing to pump as soon as faintness was induced.

The PRESIDENT thought it might be of use to soothe nervous patients in the early stage of labour.

Dr. BANTOCK doubted whether it was safe to allow the public generally to be their own administrators.

Dr. COOPER ROSE, whilst expressing his admiration of the ingenuity exhibited by the inventor, thought apparatuses of all kinds were more or less objectionable. He was in the habit of saturating a few folds of blotting paper in the bottom of a tumbler and allowing the patient to hold it over her mouth and nose. Before anesthesia was induced the glass fell harmlessly into the bed. The bottle he retained possession of himself.

Dr. SAVAGE acknowledged that patients accustomed to take chloroform should have a safe apparatus, but he thought that both chloral and chloroform were doing much harm, and should not be administered without medical supervision.

---

*Fibroid Tumour of the Uterus.*

Dr. DALY exhibited a specimen removed post mortem, which had been diagnosed during pregnancy, and had caused serious flooding at the time of parturition. She, however, progressed well for a fortnight, when swelling of the right calf, with pain, occurred, and she ultimately died on the twenty-seventh day.

Dr. SAVAGE inquired as to where the temperature had been taken, to which

Dr. DALY replied in the axilla and mouth.

The PRESIDENT inquired if the specimen had been immersed in a disinfecting fluid. He recommended that all specimens relating to women who had died shortly after delivery should be thoroughly disinfected before being brought to the Society, lest, perchance, the Fellows in handling the preparations should convey poison to other patients.

Dr. CLEVELAND inquired if any injections had been made into the uterine cavity.

Dr. DALY, in reply, stated that he had employed Higginson's syringe, the tube being carried up to but not within the os uteri.

---

*Extra-uterine Fetation.*

Dr. CHALMERS exhibited a specimen removed post mortem that had not been discovered during life. The patient, aged fifty-four, had eight children, youngest twenty-four years old. Eight or nine years ago she believed she was pregnant. About the fifth month she became ill, and passed something. She never menstruated after-



wards. Lying between the uterus and rectum was a piece of bone resembling a portion of vertebra, and the wall of the tumour was formed by a portion of bone resembling some portion of the fetal skull.

The PRESIDENT thought the case interesting, and suggested Dr. Gallabin should examine the specimen in conjunction with Dr. Daly.

---

*Double or Dicotyledonous Placenta.*

Dr. GREENE exhibited a specimen of two *placentae* enclosed in one membranous bag. The separation was more distinct when the specimen was fresh. Each placenta has a distinct plexus of veins terminating in a funis of about four inches in length; these then coalesce, and the remaining portion in nowise differs from an ordinary funis. There was only one healthy child born.

Dr. HAYES questioned whether it was a fair specimen of a double placenta. There was an atrophied portion in the centre which gave it that appearance.

The PRESIDENT thought it of sufficient interest to preserve in the museum.

---

*The adjourned Discussion upon Dr. Meadows' previous Communication,  
"Note on the Post-mortem Diagnosis of a Nulliparous Uterus."*

Dr. J. BRAXTON HICKS brought forward the uterus that had been the subject of much discussion recently. There were certain points of a medico-legal bearing, chiefly two—1st. The effect of drying decomposition had produced loss of tissue and thinning of the walls, which subsequent drying had intensified. Its relative shape when filled out was the same as when it was dry. Some experiments he had made upon fresh uteri by drying and again moistening had given similar results. The second point, which was the principal one under discussion just now, was as to the possibility of saying whether any given uterus was that of a woman who had borne children. It would doubtless be conceded that the uterus of a woman who had been delivered within a twelvemonth would be very distinguishable from that of a woman who had never conceived, but after senile atrophy had set in, it might be difficult to distinguish it from a virgin uterus.

Dr. SAVAGE thought the question for consideration was whether in any given uterus it was possible to decide whether it had ever borne children. Until it could be determined what might be the standard measurements of a pattern uterus, no table of measurements could be of any avail. The normal uterine variations in this respect were infinite, and bore no constant relation with "parity," or the contrary. The higher arching of the fundus and greater distance between the insertions of the Fallopian tubes, relied upon as one of the leading signs of previous pregnancy, was hardly to be depended upon.

Dr. EDIS exhibited several specimens of uteri, both nulli- and multiparous. Their weights and measurements had been accurately determined, and their general condition noted as regards the arching of the fundus, form of the cervix, shape of the os uteri, &c., and in addition the appearances presented on section of the ovaries. In the case of one uterus, the subject of a recent delivery, the appearance of the vessels in the walls on section were very distinct; but as regards forming a definite opinion in doubtful cases further research seemed necessary.

Dr. BANTOCK alluded to two cases of atrophy where from the appearance of the uteri it would have been impossible to say whether they had borne children, although in one instance this had been the case.

In reply to Dr. Williams, Dr. BANTOCK said the patients were still living.

Dr. MEADOWS stated that the object of his previous note had been simply to raise the question whether there were any definite signs by which previous pregnancy could be diagnosed; as a general rule he thought no absolutely certain opinion could be given.

Mr. BOND explained that in the case that had originated this discussion, he had not attached any importance to the state of the uterus. When first removed from the body it was excessively small, and the walls as thin as stout brown paper; all fibrous structure had disappeared, and it had a soapy feel; when soaked in weak spirit and water it enlarged to three times its original size, and assumed the flaccid condition it now presented. He thought from the large size of the cavity, compared to the thinness of the walls, that it was multiparous, the great thinness of the wall being due undoubtedly to the general drying-up of the body, but no one could possibly give a positive opinion regarding it.

Dr. BARNES confessed that he saw no positive grounds free from fallacy by which we could say a uterus had borne a child. We could not insist upon the weight or size, as the uterus might be enlarged from hyperplasia. One condition had not been sufficiently insisted upon, and that was the arching of the fundus externally between the broad ligaments. The condition of the os uteri too might throw some light upon the question, but this was open to fallacy, as the split edges and lobed appearance of the cervix might be due to incision and not to parturition. If there were no appearances of the breasts or abdominal walls to guide us, the condition of the uterus alone must be received with very great caution. The arching of the uterus, the split condition of the cervix, and the size of the uterus, might afford fair presumptive evidence of pregnancy.

Dr. JOHN WILLIAMS remarked that the diagnosis of the existence or non-existence of a previous pregnancy could not be made from the shape and size of the body of the uterus, thickness of its walls, form of its cavity, and deformities in the os and cervix, for all the changes which are brought about by pregnancy and labour in the

above characters may be produced or simulated by other causes, as polypus, fibroid, and other diseases. There were, however, two appearances which were of greater value in deciding this point, and one of them when found was absolutely diagnostic of pregnancy having previously existed—viz., the condition of the sinuses in the walls of the uterus under the placental site. These become filled with clots, which become organized, and are said to have been seen six months after parturition. The second point was the condition of the vessels in the walls of the uterus. They form small patent tubes with thick yellowish-white walls, and give the tissue a worm-eaten appearance. This is, so far as his experience went, permanent. It has been well marked fifteen years after parturition, and so far as he had been able to ascertain it was not simulated by any disease of the uterus.

Dr. BARNES stated that the condition of the cervix afforded but negative evidence. He had seen a conical cervix with a small round os uteri where the woman had had children.

Dr. BANTOCK inquired how Dr. Williams's remarks would apply to superinvolution.

Dr. SNOW BECK doubted if the clots in the uterine sinuses remained even three or six months. He had examined very many uteri, and never found these clots. He had at present one uterus from a patient who died two months after parturition, and he was unable to detect the slightest evidence of clots in the sinuses or in any part of the uterus.

There was no sign by which we could possibly affirm that a woman had had children.

Dr. SAVAGE said he believed Dr. Beck was right. The uterine-sinus system was a thing of the past: it received its deathblow from Dr. Hicks, whose exhaustive papers on the subject had already appeared in the *Transactions*. In separating *placenta prævia* from the uterus, the finger came in contact with no large veins; nothing was easier than the detachment of it. As was well known, hemorrhage ceased on its complete separation, the uterus of course remaining still uncontracted. He thought that with no history given—merely a uterus to examine—we were not justified in giving any opinion, except that given by Dr. Meadows, as regards nulli- or multiparity.

Dr. ROGERS said he had now had an experience of over thirty years in examining uteri, and had come to the conclusion that a positive opinion founded on precise and unimpeachable data could not be given. He had brought two specimens of virgin uteri for inspection, differing materially from each other. He would be glad if other observers could substantiate the statement made by Dr. J. Williams.

Dr. HAYES called attention to the remarkable thinness of the uterine walls, as compared with the large size of the cavity in the specimen exhibited by Dr. Hicks. He thought the thinness could be explained partly by the relaxation of the muscular fibres after the

disappearance of the rigor mortis, and partly by the desiccation of the tissues. Doubtless, too, the duct-like disintegration of the tissues during the lapse of time had something to do with it. It was impossible to suppose that the process of soaking the dried uterus could ever permeate the tissues with fluid in the same fashion as the heart during life.

The PRESIDENT expressed a hope that in future debates of the Society, Fellows who desired further information from speakers would address their interrogations through the President, as it was a great inconvenience for any Fellow to cross-examine those who had spoken, and was likely to interfere with the usual course of discussion.

A committee, consisting of Dr. Hicks, as Chairman, Dr. Savage, Dr. Meadows, Dr. J. Williams, Dr. Snow Beck, and Mr. Randall, was appointed to investigate the subject further, and to report to the Society.

The PRESIDENT gave notice that at the next meeting Dr. Arthur Farre would send some specimens, and Mr. Jonathan Hutchinson would read a paper on the importance of studying the diseases incidental to parturition in the lower animals.

---

## OBSTETRICAL SOCIETY OF EDINBURGH.

*Meeting, November 24th, 1875.*

Dr. PEEL RITCHIE, *Vice-President, in the Chair.*

Dr. UNDERHILL showed a uterus, deeply imbedded in the cervical wall of which was a cyst about the size of a large filbert. When cut into, it was found to contain a clear transparent yellowish glutinous fluid.

Mr. JAMIESON exhibited a placenta, in which the vessels forming the umbilical cord divided before they reached the edge of the placental structures, the so-called insertio velamentosa.

Dr. PEEL RITCHIE showed a polypus nearly as large as a walnut, which came from the rectum of a child. He remarked that, though hemorrhage from the bowels was stated by authorities on children's diseases to be frequently caused from the bowels by polypus, such was not his experience at the Children's Hospital here.

Dr. RATTRAY showed an umbilical cord, 42 inches in length, which had not been wrapped round any part of the child, either neck, trunk, or extremities.

Dr. BRUCE showed an instrument which he had devised for the performance of vaccination, and which consisted of a pencil-case handle with a needle at the one end to be used as a scratcher, and a flat blade for applying lymph at the other, both sliding into the case



when not required. The advantages of this instrument were, its extreme neatness and handiness, and the almost invariable success which attends its use, as out of 340 vaccinations failure was only once met with. Besides, the needle could easily be kept clean, or replaced by another if required.

---

*Laceration of the Perineum.*

By JAMES YOUNG, M.D.

The treatment of laceration of the perineum is one of the most important questions that can come under the notice of the obstetric surgeon. I propose to cite two cases, illustrative of the benefit of treatment by the interrupted suture, more to elicit discussion than for the purpose of bringing forward any new matter. The second case is almost unique in regard to the extent of the rupture, and likewise the result.

CASE I.—Some twelve months ago I was in attendance upon a lady in Maitland Street. It was her first confinement. The labour was protracted and difficult, requiring the use of the short forceps. I used very considerable strength in traction, but without the pendulum movement, and failed to extract the head. I sent for my friend, Dr. Charles Bell, who kindly came to my assistance. The forceps were again applied, and the pendulum motion, with powerful traction, was successful in delivering the patient of a fine, large, healthy boy; the perineum was torn, but not through the sphincter ani. The wound was carefully sponged, and brought together with the interrupted suture in three places in less than half an hour after the accident. The result was most satisfactory, and the patient made an excellent recovery. The ligatures came away in four days, and the wound was absolutely healed in ten, at every point. The usual rules were enforced.

CASE II.—To this one I would direct attention. On the 28th of June, 1875, I was summoned to see Mrs. M., aged thirty-five, a primipara. At 6 P.M. the os uteri was small (size of a shilling), although the patient had been in labour for twelve hours. I was again called at 6 A.M. next day, when I found the first stage almost over, and the head presenting in the occipito-anterior position. The woman had been twenty-four hours in labour, and as I considered it unjustifiable to leave her longer, I sent for the forceps. The vagina was hot, and the pains were becoming feeble. While under chloroform, I used steady traction during each pain, allowing the external parts time to dilate slowly. Notwithstanding every care, the perineum ruptured right along through the sphincter ani, and into bowel three inches, my whole index finger easily passing from bowel into vagina. When the placenta was expelled, and the uterus contracted, the wound was carefully sponged. The anesthesia being maintained, the

torn parts were brought together with the interrupted suture. Seven ligatures were used, which had been dipped in carbolic oil, and the wound was left in perfect approximation. The urine was drawn off every twelve hours. The thighs were tied together, and by the administration of opium the bowels were confined for six days. No local dressings were used. The patient made a perfect recovery; the wound healed throughout at every point; and on the fourteenth day she was left to her own care. Several weeks ago I examined the patient by placing one index finger in the bowel, and the other in the vagina, and found the recto-vaginal septum complete. Let me here mention, in connexion with her history, that when Mrs. M. was married, I understood that perfect sexual intercourse was precluded for some months in consequence of the extreme rigidity of the vagina, and four years elapsed ere this child was born.

*Remarks.*—1st. Causes of laceration of the perineum. 2nd. Means of prevention. 3rd. General rules of treatment. Many obstetricians will agree with me in saying that, in numerous primiparous cases, the perineal portion of the vaginal mucous membrane is frequently ruptured, and only heals by leaving a sulcus, which rather favours than hinders future labours. Among the causes of perineal rupture might be enumerated—1st. When the age exceeds *thirty* years. 2nd. Cases where the head of the child is very large. 3rd. Malpresentations. 4th. A small or deformed pelvis. 5th. The use of forceps. 6th. A rigid perineum; each cause operating more especially in primiparous women.

*Prevention.*—I generally adopt the plan of having lard, butter, or cold cream rubbed over the perineum during the extrusion of the head forwards. 2nd. Gentle dilatation of the external parts with the finger may be adopted during each pain. 3rd. Slow traction, when the forceps are used, and only during each consecutive pain. 4th. The application of the hand in supporting the perineum during strong expulsive pains; and when forceps are employed, during the delivery of the head, the left hand may be spread over the distended surface of the perineum.

*Treatment.*—In simple cases of laceration of the mucous membrane of the vagina, or even where the margin of the *sphincter vaginæ* is torn, the only treatment necessary is mere cleanliness and sponging. I extremely deprecate, in any case, the use of bandages, pads, or plasters, as being more irksome to the patient than useful. In severe perineal rupture, as in Case II., the immediate closing of the wound is of paramount importance, so as to secure healing by the first intention. The interrupted suture of carbolized catgut should be used; and the entire rupture must be brought into exact approximation. Careful and frequent sponging must be attended to by the nurse, to avoid any irritation from the lochial discharge. The urine must be drawn off every twelve hours; no dressings applied; the patient kept in the horizontal position; the thighs kept together; and the bowels must not be allowed to move for six days.

Dr. CARMICHAEL agreed with Dr. Young in his treatment. In his earlier days of practice, he had not been prepared to stitch the perineum at once in cases of severe rupture ; but now he always took the necessary materials with him. He had found that patients of considerable age had a great tendency to suffer laceration. He found the use of hot sponges to the tensely stretched perineum of great service in assisting expansion.

Dr. PEEL RITCHIE had also found great benefit from the application of the hot sponge to the perineum, and had never found any bad consequence from it. His own experience of serious rupture was not very great. He was glad to hear Dr. Young advocating the early stitching up of such cases. Dr. More Madden, in a paper read before the Society some years ago, had advised a more elaborate method of applying the sutures, but he thought the simple plan was the better.

Dr. MACDONALD agreed with the preceding speakers as regards the value of early application of sutures. But he preferred wire to cat-gut sutures, and found the wounds healed readily with them. In his experience, and he happened, curiously, to have been called upon to operate for this accident three times within the last three weeks, if the perineum were soon sewn up and the patient kept quiet, there was little risk of fever, and the cases do surprisingly well. In a case which he had seen some time ago, where there was a contracted pelvis, the head and hand presenting, he had put on the forceps without any one to help him, and a sudden pain drove the hand and head together out of the vulva, tearing the perineum severely, the rupture extending an inch at least up the rectum ; it was sewed up twelve hours afterwards, and healed well. He gave no opium, as the bowels are generally confined during the first week without it. Nor did he think there was any need of using the catheter. Many women go through life with hardly any perineal support in consequence of a little lack of courage on the part of their medical attendants. He considered that the difficulties in the way of successful operation in bad perineal tears were greatly exaggerated in the minds of the profession ; and much comfort would result to patients if every bad rupture were sewn up, either at the time of its occurrence, or within the next twelve hours or so, according to convenience.

Dr. PRIDIE mentioned a case in which laceration had taken place during the expulsion of the shoulders, the head having come through without doing any injury.

Dr. BALLANTYNE supported Dr. Young's views as to the advisability of early dressing. In a recent case of cross birth, where there had been a tear, he had sewn it up at once, and the patient had had a child since without a repetition of the laceration ; he had often found a small tear caused by the head made worse during the passage of the shoulders.

Dr. BRUCE thought prevention was better than cure. In employing the forceps, it was of great convenience to direct the head well for



ward when it stretches the perineum, and to remove the instruments before the termination of the labour. In the majority of cases of laceration, he thought nothing need be done beyond cleanliness and attention to position ; if sutures were necessary, probably they were better applied at once.

Dr. BURN agreed with Dr. Bruce on removing the forceps, when it is only the perineum which prevents the termination of the labour. He had practised midwifery since 1824, and had only once had a bad tear into the rectum ; it was sewn up and did well ; the tear was repeated in the patient's next labour, and she is going about and has perfect command of the bowel. The rapid forcing of the head on a rigid perineum was in his experience a frequent cause of rupture.

Dr. RATTRAY had adopted the plan of removing the forceps, when the head was on the perineum.

Dr. MENZIES thought that there was great risk of rupture in first cases, where turning is necessary, and the child has to be extracted rapidly to save its life.

Dr. MILNE had seen but little of torn perineum ; he had used forceps very often, but thought he had had more tears in cases where no instruments had been used. He found most of the cases heal well if left alone, and had only once had to sew ; in that case the tear was through the sphincter.

Dr. SIMPSON remarked, that no one had yet referred to diseased conditions of the perineum as a cause of rupture ; such, for instance, as the soft friable state of the tissues, sometimes produced by syphilis. A case of this kind had happened in the Maternity a few years ago. Distortion of the pelvis was another common cause, of which the two worst varieties were the flat sacrum and narrowing of the arch of the pubes ; the former preventing the proper sweep of the occiput, the latter forcing it far down towards the anus. As to rupture in forceps cases, he was not sure that removing them before the head was quite extracted was of any service in preventing injury. In a recent case, he had removed one blade, but could not get the other away, as it was caught over one of the ears ; in this case there was a slight laceration. The worst case he had seen in his own practice was in a primipara, the head being in the occipito-posterior position ; the tear passed entirely through the sphincter ; this was not sewn up till the next day, as he had not the opportunity of stitching it immediately ; but it should always be done at once, within thirty-six hours after the accident at latest, when the tear passed through the sphincter. All other cases he left alone ; if not done at once, the operation for repair of the perineum should be delayed until lactation is accomplished, as the surfaces sometimes will not unite earlier.

---



*Meeting, January 12th, 1876.*Dr. ALEXANDER R. SIMPSON, *President, in the Chair.**Against the Pendulum Movement in Working the Midwifery Forceps.*

By J. MATTHEWS DUNCAN, M.D.

My object in writing this short paper is to contribute to the conclusive giving up of a bad practice. I have long publicly taught that the pendulum movement is useless and injurious ; but that kind of publicity is not so wide as is desirable. Others, of great authority, from their recognised science and experience, such as Litzmann of Kiel, have made objection to this manoeuvre, but it is still extensively used and vigorously recommended. There is, indeed, a natural tendency to resort to this swinging motion in the hands or minds of many practitioners, who probably have erroneous notions of what is called gomphosis, and think of drawing a nail out of a board. Besides, the movement is an element in that fussiness which recommends itself to many, if not the majority, of practitioners, and satisfies the admiring onlookers. The solemn arguments by which obstetric authors support its use I shall presently discuss.

My objection does not extend to changes in the direction of traction by the forceps, such as may be required according as the dragged head descends, or such as may be called for when the head has been previously inefficiently dragged in a wrong direction. It is to the changes implied in the oscillatory or leverage or pendulum-like movement of the dragging instrument that my objections are raised. These movements I regard as not merely useless, but as also injurious. They have been considered as leading to economy of force ; but this is the opposite of the truth, as I shall hereinafter show. I mention this imagined advantage now in order to say that, were it true only so far as the practitioner is concerned, I would esteem such economy as of no value, for the accoucheur is present with a practically boundless supply of force, which he is willing to lavish for behoof of his patient.

In the following remarks I shall make reference only to that pendulum movement from side to side, which alone is, so far as I know, recommended in these latter times. The pendulum movement in a sagittal direction, as recommended by the early describers of the forceps operation, is still more open to objections than the former.

In describing or defending the pendulum movement, two great points are made : first, that it is analogous to, or identical with, that of a lever and double rack ; and, second, that by resorting to it there is an economy of force.

The lever and double rack hypothesis may be considered first. It is difficult to dispose of it only because it is so flimsy that it is impossible to get hold of it. There is no toothed rack on the wall of

the pelvis. There is no roughness to take the place of a toothed rack. Were there any such teeth or roughness, the worst use that could be made of them would be to make them assume the function of a rack. The accoucheur should strive to advance the head as smoothly and with as little impression as possible on the walls of the pelvis. Further, there are no teeth or roughnesses on the fetal head to fit into the teeth of the supposed rack.

Without the lever and double rack hypothesis substantiated, the movement must be useless; for it is, and must be, done to no purpose. It is conceivable that the head may be seized with such a degree of firmness by the blades of the forceps as to be moved or made to revolve as on a pivot; but such movement, without the additional force required in simple and successful traction, uncomplicated by the movement, would be of no avail. It would not advance the head. Without the lever and double rack, it would only cause revolutions as on a pivot—it would not produce progress.

Let us imagine that the accoucheur, dragging to one side, produces advance of the opposite side, which he maintains by continued dragging, while he makes the oscillatory movement in order to drag to the other side. In this way he may make the head advance while using an oscillatory movement. Every one knows that this is easily done. But then every one must also recognise the utter inutility of the movement. It is the imitation of the action of a lever and double rack without a trace of its utility. An unnecessary movement like this has great disadvantages. The pressure exerted, and the traction force used, are probably greater, certainly not less, than if simple traction were exerted to produce the desired result. Pulling the head down at one side and then at the other, and so advancing, is merely an injuriously complicated way of producing simple progress. It produces no evasion nor diminution of the difficulty to be overcome, while it has concomitant and avoidable evils.

In answer to this reasoning one might refer to an analogy in the way in which a cork is sometimes extracted from a bottle without using a corkscrew. But a study of this analogy only confirms the argument. For the cork would be better, and on the whole easier, extracted by simple uncomplicated traction as by a corkscrew. Besides, the cork and the bottle mutually exert such friction force as prevents retraction of one side if advanced, just as if the bottle were a rack and the cork the lever; and such is in no sense the case with the fetal head and pelvis. Lastly, this oscillatory advance of the cork is only sought when power fails to produce a direct advance, and, in the case of the forceps, there is never deficiency of power; for, as we have already said, the power applicable by the accoucheur is practically boundless, or, in more sober terms, it is greater than he can safely apply.

The idea that there is any saving of force, so far as pressure on the mother's and child's parts is concerned, by resort to the oscillatory or pendulum movement, is such that I cannot argue against

it. The question involved is purely mechanical and of extreme simplicity. It is this: a mechanical difficulty in bringing a child's head through a resisting passage has to be overcome; further, the difficulty is not to be evaded by changing the position of the child's head; on the contrary, that position may be supposed to be the most favourable for facility of propulsion. Now, can any oscillation or other imaginable movement diminish the mutual, and, in this case, injurious pressure or force required to produce advance? The question requires no answer. The supposition is absurd. Moreover, the absurdity is not less, whatever phase may be given to the hypothetical advantages of the pendulum movement. A certain amount of work has to be done; the head has to be advanced against resistance that must be overpowered if the effort is successful. Direct uncomplicated traction does the work in the simplest way, and no complication of it by pendulum movement or other can diminish the amount of work expended below that required by simple traction. The complication of simple traction may, however, increase the expenditure of force to a great extent.

The pendulum movement necessarily involves an injurious amount of pressure and consequent friction, in all cases, between the parts of the head to which the blades of the forceps are applied and the adjacent maternal structures. No doubt this friction is in most cases so slight and temporary as to be of little moment. But in some cases, when the resistance to progress arises from tight and undilatable soft parts, it may be very injurious. The most important forceps cases, however, are those where the obstacle to progress arises from hard parts; and, of these cases, the most frequent and characteristic are those of simple narrow or flat pelvis. In such cases the head has to be slowly dragged and perhaps moulded between the promontory of the sacrum and the pubic bones. Now here the pendulum movement involves special evils and dangers; for by it there is necessarily produced, besides the trivial friction, which is most extensive at the points where the blades are applied, a violent and powerful squeezing of the soft parts between the head and the opposing pelvic bones, on which the head works. This combination of wriggling and squeezing is altogether unnecessary, and must greatly aggravate the necessary or unavoidable mutual pressure, which is bad enough.

If for the carrying out of the plan of pendulum movement the forceps is made to compress the head so strongly as not to slip on it, which mode is probably regarded as desirable, then the points of the forceps, and especially the point of that blade which is on the side of the head towards which the movement is given, will exert a specially powerful, and certainly undesirable, amount of pressure on the parts of the child's head or face which they touch. If, on the other hand, the blades do not press the head with such firmness as to obviate a to-and-fro motion of them on the head, then the scalp will be liable to be much injured, and its surface abraded; conditions which are often observed as the result of this kind of proceeding.



There is in the mechanism of delivery, whether natural or morbid, nothing analogous to this artificially produced oscillating or pendulum movement. Nature pushes a fetal head through an obstructing passage by force, which produces, or may produce, on the one hand, dilatation or laceration of the passage; and, on the other hand, various kinds of changes in the shape and size of the fetal head. Our best guide in forceps cases is the process of nature; and it is probable that any future improvements in the working of the instrument will be the consequence of closer and successful investigation of the mechanism of labour in these difficult cases. The pendulum movement, in working the forceps, does not advantageously increase the power of the instrument to produce desired results.

The use of the forceps is to contribute, by artificial pulling, to the strength of the natural expulsive efforts which push. To this traction, judiciously applied, the practitioner should confine himself. The oscillatory movement will contribute nothing to the forward traction, and it is the forward traction which alone is desirable.

In corroboration of these theoretical views as to the injuriousness and inutility of the pendulum movement in the working of the midwifery forceps, I might appeal to the extensive experience of myself and of many other practitioners. But such appeal can only be held as evidence sufficient to show that the pendulum movement is not necessary. It affords no evidence that using it or abstaining from it is the preferable plan. And I cannot imagine any method available at present whereby the results of experience can be made suitable for the final settlement of the matter.

Both plans are used by good practitioners. Traction without oscillation is simple, effective, and in accordance with the method of nature's own efforts. Traction with oscillation is complicated; and many theoretical and practical objections may be made to it.

Other objections might be adduced against the pendulum movement, and some are candidly stated by authors who recommend its adoption. I have confined myself to the discussion of its supposed utility and inevitable evils. The only advantage which I can conceive it to possess, is one to which no one will avow his indebtedness, for it would be an admission of culpable want of knowledge, and consequent unjustifiable practice. A practitioner ignorant as to the proper direction of pulling may, by this motion of the forceps while extracting, fall accidentally upon the right direction, and thus do some good by mere luck, and at much risk, which should have been done intelligently, and without avoidable risk. It is, perhaps, in order to insure this kind of possible success, that some authors recommend the movement to be not pendulum-like, but rotatory, or in the grand so-called tours.



## Obstetric Summary.

### *Extra-uterine Pregnancy.*

At a meeting of the Medical Society in Buda-Pesth on November 6th, 1875, Julius Dollinger showed the organs of a woman, aged forty, who, while pregnant, fell down a short flight of stairs and soon died. The uterus was as large as at the third month; it was lined with a decidua five kilometres thick, the os was plugged, and its lips were rounded. The abdominal cavity contained about  $1\frac{1}{2}$  kilogramme of clotted blood, among which lay an embryo about six months old, enveloped in its membranes. The part of the uterus connected with the left Fallopian tube was stretched into a cord 30 centimetres long, ending in a dilatation as large as a man's fist, with a laceration at its lower part; this dilated portion was filled with placenta. The wall of the uterus at this part was nearly as thick as in an uterus at the ninth month. From the anterior and lower end of the cleft proceeded the remaining part of the Fallopian tube, the canal of which was pervious to a sound, and its abdominal extremity intact. It was 9 centimetres long, and as the right tube was 10 centimetres long, it was concluded that the ovum had become lodged and developed one centimetre from the uterine end of the tube. The most recent Graafian follicle was in the left ovary. Dollinger remarks that in tubal pregnancy hypertrophy of the muscular structure of the Fallopian tube takes place with the growth of the embryo; but that after a time the development of the embryo proceeds too rapidly for the tube to keep pace with it, and at last the latter gives way at the weakest point. It appears that in this case the fall was not the direct cause of the rupture of the tube, but that it excited labour pains. The woman went to bed, expecting that abortion would occur; suddenly, however, she felt a violent cutting pain in the left inguinal region, gradually grew pale, and in a few seconds was a corpse. It was very probable that the laceration was the result of violent contractions of the hypertrophied muscular wall of the tube. In other cases, the Fallopian tube is not equally hypertrophied; but a part of it, generally the upper (which is normally thinner) becomes so much distended and thinned by the embryo as to be easily torn. In such cases in consequence of the atrophied state of the vessels of the tube, the hemorrhage is insignificant unless separation of the placenta takes place at the same time. The embryo may produce very little irritation of the peritoneum, and may undergo fatty or calcareous degeneration; but more frequently it gives rise to general or limited peritonitis. A preparation was shown, taken from a woman in whom a diagnosis of extra-uterine pregnancy was made at the fourth month, she having been seized with severe pain, and the parts of a fetus being felt *per rectum* in Douglas's space. She died six months later of amyloid degeneration of the kidneys. At the post-mortem examination, some of the convolutions of the uterus

were found glued together by false membrane, covering in Douglas's space so as to separate it from the remainder of the peritoneum. Douglas's space was filled with very offensive matter containing black flocculi; and at its lower part lay the macerated bones of an embryo about four months old. In the vicinity of the left Fallopian tube, and communicating with it, was a cavity as large as a child's fist, which also opened into Douglas's space by an aperture the size of a hazel-nut. There was also an opening of the same size from Douglas's space into the rectum, 12 centimetres from the anus. Through this, no doubt, the fetal remains would have been discharged if the woman had not died.—*Pester Medicinische Presse*; and *Medicinische Chirurg. Rundschau*, January, 1876.

---

*On the Fetal Placenta of the Pachydermata.*

At a recent meeting of the Biological Society of Paris (*Ann. de Gynékol.*, January, 1876) M. Dastre communicated the results of his researches. In the pig, the villous folds which constitute the placenta are disposed in a radiating manner around the little bald or smooth centres. The structure of the stroma of the chorion is modified at these points. Moreover, the villosities do not extend over the whole surface of the chorion; they leave free in one part and another of the middle zone two ill-determined bands. Each half of the sac of the chorion is divided into three zones: a middle zone, vascular and villous; a zone vascular only, but not villous—a continuation of the former; finally, a third zone (horns of the chorion), which is neither vascular nor villous.

In the thickness of the stroma of the chorion of ruminants and pachydermata one finds in addition a network of chalky aspect, formed principally by a deposit of phosphate of lime. The chorion plates of this deposit are in an intimate relation with the junction of the fetal placenta. But among the pachydermata they occupy the same situation as the villosities; they exist only in the middle zone.

---

*The Contra-indications for Turning in Shoulder Presentations, and the Measures which may Replace this Operation.*

Dr. A. Pinard (in his *Thèse d'Agrégation*, 1875) discusses at length the various classes of cases in which version may not inappropriately be replaced by other expedients. Numerous cases are cited exemplifying the methods resorted to, and illustrative cases from other authors quoted at length.

The contra-indications are briefly summarized as follows:—

1. Non-dilatation of the cervical orifice, when this is due to anatomical or spasmodic rigidity of the neck, or where it results from an organic affection, such as the existence of cancer, or of a fibrous tumour.

2. Profound engagement of the fetal presentation.

3. Tetanic contraction of the uterus.

4. Extreme contraction of the pelvis.

It is important to distinguish cases where the child is living from those where it is dead. In the latter case it is necessary to perform version whenever the introduction of the hand is possible. If the infant be living, on the contrary, M. Pinard proposes Cesarean section.

The operations which may replace version are divided into three main classes :—

1st. Those which have for their aim and definite result forced version, such as amputation of the arm and evisceration.

2nd. Those which have for their aim the successive extraction of the two parts of the fetus, into which the vertebral column has been divided by means of the *écraseur*, crotchet, forceps, saw, scissors, &c.

Nearly a third of the thesis is devoted to this heading, the inventions of various authors being described, and cases given in illustration.

*The Physiologico-Pathological Phenomena of the Circulation in Pregnant Women.*

Dr. Barnes read a paper upon this subject at a meeting of the Metropolitan Branch of the British Medical Association, and indicated the following conclusions :—

1. There exists in pregnancy a peculiar state of the blood, an increased arterial tension, and a disposition to phlebectasis, general or affecting particular vascular regions or systems, which may, under certain circumstances, lead to hemorrhagic effusions.

2. These hemorrhagic effusions may be salutary or conservative. Hemorrhages from the mucous membranes, by relieving excess of tension, may avert internal hemorrhages or abortions, or that oppression of the kidneys which results in albuminuria and eclampsia.

3. Hemorrhages from the uterus inducing abortion may, in the same way, avert more serious mischief, especially albuminuria and eclampsia.

4. But these hemorrhages, although we may recognise in them a conservative purpose, easily exceed physiological or useful limits, and may themselves induce danger by anemia.

5. Hemorrhages, from whatever part, occurring in women within the period of sexual life, should suggest the probability of pregnancy, or of menstrual disorder.

6. The hemorrhages described should be taken as indications to reduce vascular tension. This may be done in some cases by bleeding, general or topical; by purgatives; by salines; by abstinence from stimulants; by regulation of diet; by digitalis, hydrocyanic acid, and other means.

7. Bleeding from the arm may avert abortion, not only by reducing arterial tension, but also by a derivative action determining the direction of the blood from the pelvis to the opening in the vein. This derivative action of bleeding was much relied upon in a variety of cases by Lisfranc.

8. In some cases, where the hemorrhages are frequent and great, and induce serious symptoms, the induction of labour is indicated.

## Gynecic Summary.

### *On the Pathology of Dysmenorrhea Membranacea.*

In the *Archiv für Gynäkol.*, Band ix. Heft 1, Dr. Beigel contributes an exhaustive article upon this subject. From the result of his examinations he concludes that—

1st. The so-called dysmenorrhea membranacea occurs in consequence of primary or secondary disease of the uterine mucous membrane—endometritis.

2nd. The characteristics of this disease consist in a pathological change of the mucous membrane, whereby, in consequence of well-marked proliferation from beneath, the membrane is shed in large shreds or as a continuous sac.

3rd. The expulsion of this, as a rule, occurs through contractions of the uterus after preceding hemorrhage—menstrual or otherwise—whereby closure of the os internum by the membrane leads to retention of blood and the production of intense pain.

4th. As menstruation plays a secondary part in the development of this affection, and the formation of the membrane is in no way connected with conception or abortion, it seems better to give up the terms “dysmenorrhea membranacea” and “menstrual decidua,” and adopt the denomination “endometritis exfoliativa.”

5th. The microscopic examination of the membrane furnishes no similar result. In one class of cases we find the normal elements of the mucous membrane merely pathologically increased; in another series the individual elements of this, as the glands or epithelium, are lost, or in a state of degeneration; in a third class embryonal cellular tissue is formed, and in a fourth epithelium plates or cells, which are very similar to these, alone or in conjunction with the embryonal tissue of the skin elements of the membrane.

6th. In all cases it forms a marked cellular production, constituted of free cells, which form the definite occasion of the loosening of the mucous membrane from its attachment.

7th. The pathological changes of the uterine mucous membrane in endometritis exfoliativa appear to be such that the development of an impregnated ovum in the uterine cavity cannot take place; therefore the patients, as long as they are under the influence of the disease, are sterile.



*On Ablation of the Body of the Uterus in Cases of Irreducible Inversion by External Hysterotomy.*

M. Donné (*Ann. de Gynékol.*, January, 1876), in a recent communication to the Academy, formulates the following conclusions:—

1st. External hysterotomy is an extreme surgical resource, but precious for cases of irreducible inversion which threaten immediately the life of the patient.

2nd. This operation does not furnish a greater mortality than that of the greater number of grave operations.

3rd. In the actual state of science it ought to be made preferably by the ligature, bearing in mind the perfection attained by this method.

4th. For the first months of an inversion, even the first year, as far as possible, repeated tentative efforts at reduction, at lactation, which generally suppresses the hemorrhage, and all sorts of palliative methods should be fairly tried.

The operation ought to be reserved for cases recognised as irreducible, and for the period, remote from the commencement of the malady, when involution has completely taken place, and the neighbouring organs have undergone changes, rendering the risk of peritonitis much less, this being very important.

BOOKS, PAMPHLETS, AND PAPERS RECEIVED.

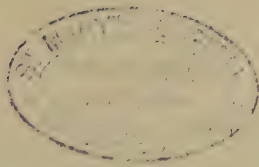
“Lectures on Obstetric Operations, including the Treatment of Hemorrhage, and forming a Guide to the Management of Difficult Labour.” By Robert Barnes, M.D. Lond. Third Edition. J. and A. Churchill. 1876. Pp. 606.

“On the Development of the Ova and Structure of the Ovary in Man and other Mammalia.” By James Foulis, M.D. Edinburgh. Neill & Co. 1875.

Communications have been received from Dr. Atthill, Dr. Bozeman, Dr. Henry, Dr. Edis, Dr. Williams, Dr. Hayes, Dr. Cassells, Mr. Cullingworth, Dr. Gervis, and Dr. Ashburton Thompson.

*All communications, books for review, letters, &c. for the Editor, may be addressed to the care of the Publishers, 11, New Burlington Street, London, W.*

NOTICE.—All the Back Numbers of this Journal may be had of the Publishers direct, or through any Bookseller.



## GENERAL INDEX.

---

**A**BORTIONIST, execution of an, 143

Abortions, medico-legal aspect of, by Dr. Leblonde, 488

Albuminuria of pregnant women, advantage of milk diet in, 701

Anderson, E. G., note on the spontaneous disappearance of ascitic fluid, 309

Anesthesia, on surgical, in infants, by the administration of chloral by the stomach, by Dr. Bouchut, 632

Anus, on the treatment of fissure of the, by chloral, by Dr. Crequy, 557

Apoplexy, pulmonary, in an infant, by M. Budin, 558

Ascitic fluid, note on the spontaneous disappearance of, by E. G. Anderson, M.D., 309

Atresia, on complete, of the female genital organs, or unilateral hematometra, by Dr. A. Vulch, 536

Atthill, Dr. Lombe, "Clinical Lectures on Diseases of Women," review of, 96; inaugural address to the Obstetrical Society of Dublin, 688

Aveling, Dr. J. H., influence of posture on women, 145; a letter in facsimile and other hitherto unpublished memorials of Harvey, 217, 310

**B**ANTOCK, Dr. G. G., abdominal cyst, with pulmonary complication, 489

Barber, Mr. O., specimen of anencephalic fetus, 667

Barker, Dr. Fordyce, on puerperal fever, 322

Barnes, Dr., on puerperal fever, 183; address on obstetric medicine, 289; specimen of a dermoid cyst, 321; venous coagulum, 321; colloid tumour, 321; the physiologico - pathological phenomena of the circulation in pregnant women, 840

Barnsfather, J., physiological changes in the uterine mucous membrane, 287

Beck, Dr. Snow, on puerperal fever, 322, 331

Beigel, Dr., on the pathology of dysmenorrhea membranacea, 841

Bell, Dr. C., cases of fatal hemorrhage after childbirth, 413

Bell, Dr. R., the treatment of whooping-cough by the iodide of silver, 589

Bladder, hair in the female, 127; examination of the female, by J. Matthews Duncan, M.D., 465; accidental penetration of a hairpin into the, 775

Bouchut, Dr., on surgical anesthesia in infants, by the administration of chloral by the stomach, 632

Braithwaite, Dr. J., nitric acid as a caustic in uterine practice, 467; on the avoidance of the frequent use of the speculum, 504

British Medical Association, annual meeting of the, abstracts of papers read in the obstetric medicine section of the, 457

Brown, Dr., on puerperal fever, 194

Bruce, Dr., a new vaccinator, 829

Brunton, Dr. John, on the postural treatment of prolapsus of the funis umbilicalis, 16; on puerperal fever, 191; case of extreme dropsy, 269; on accidental concealed hemorrhage, with cases, 437

Brussels Congress, 695

Budin, M., on pulmonary apoplexy in an infant, 558

**C**AIRNS, Dr., on the risks run by obstetricians, 123

Calderwood, G., a case of difficult labour from abnormal rigidity of the os, 657

Callender, Mr., on puerperal fever, 199

Cassells, Dr. J. P., remarks on an unsuspected cause of defective hearing and of deaf-mutism in infancy and childhood, 799

Cautery, on the employment of the actual, in uterine disorders, 215

Cesarean section, case of, post mortem, 421; case of, 699; case of, by Dr. J. W. Oswald, 673; section in cases of fibroid tumours, 701

Cephalhematoma, on the causation of the so-called, by Dr. H. Fritsch, 487

- Cephalotribe, the ; its inconvenience and its dangers, 358
- Chadwick, J. R., a case of immediate transfusion by Aveling's method, 66
- Chalmers, Dr., extra-uterine fetation, 825
- Chambers, Mr. T., on "strange obstetric practice," 35
- Championniere, Dr., on the uterine lymphatics, and their influence in uterine pathology, 630
- Chloral and chloroform combined, on the physiological action of, 141 ; on the employment of, as an anesthetic in natural labour, by Dr. Choupe, 357 ; in puerperal convulsions, by Dr. Portal, 487
- Chloroform, on, in natural deliveries, 141 ; in the surgery of infants, 216 ; influence of, upon the fetus in utero, by Dr. Zweifel, 421
- Chlorosis, on, by Dr. E. Frankel, 557
- Chorea, on the treatment of, by ether spray and ice to the vertebral column, by Dr. Fabry, 559 ; observations on, cured by chloral administered by rectum, 702
- Choupe, Dr., on the employment of chloral as an anesthetic in natural labour, 357
- Chronic affections of the uterine system, on lesion and disease in the, 142
- Churchill, Dr. Fleetwood, address to, by the council and members of the Obstetrical Society of Dublin, 615
- Circulation, the physiologico-pathological phenomena of the, in pregnant women, by R. Barnes, M.D., 840
- Cirrhosis of the liver in infants, 143
- Coghill, Dr. J., on uterine flexions and displacements, 466
- Corpus luteum, the, of pregnancy, 139
- Cranial positions, on the relative frequency of the, by Dr. J. G. Swayne, 372
- Craniometry, note on intra-uterine, by Dr. J. Matthews Duncan, 417
- Crede, Prof., on the use of salicylic acid, 288
- Cremation, note on, 72
- Crequy, Dr., on the treatment of fissure of the anus by chloral, 557
- Croly, H. G., case of ovarian tumour, 277
- Crombie, Dr., a new chloroform inhaler, 824
- Cullingworth, C. J., retroversion of the gravid uterus, 88, 91, 815 ; some cases of puerperal illness in private practice, 221, 299 ; case of extra-uterine fetation, 448, 660
- Cystocele, case of—Stoltz's operation—cure, by Heywood Smith, M.D., 314
- Cysts, serous ovarian, 214 ; rupture of ovarian, by Dr. G. Nepveu, 423 ; abdominal, with pulmonary complication, by G. G. Bantock, M.D., 489
- DALY, Dr., fibroid tumour of the uterus, 825
- Dastre, M., on the fetal placenta of the pachydermata, 839
- Dawson, Dr. B. F., artificial diet of infants, 703
- Day, Dr. W. H., on the tolerance of sedatives in malignant uterine disease, 646
- Deaf-mutism and defective hearing in infancy and childhood, remarks on an unsuspected cause of, by J. P. Cassells, M.D., 799
- Decidua, uterine, expelled without metrorrhagia, 774
- Denham, Dr. J., case of extra-uterine gestation, 129
- Denmark, regulations for midwives in, 136
- Denidation, 245
- Depaul, Prof., on extra-uterine peritoneal pregnancy, 423
- Dermoid cyst, 321
- Dog's milk for children, 775
- Dollinger, J., extra-uterine pregnancy, 838
- Donné, M., on ablation of the body the uterus, 842
- Donovan, Wm., on Indian hemp in post-partum hemorrhage, 353 ; placenta prævia, 460
- Draper, W., folding short forceps, 715
- Dresselhuys, Dr., case of extra-uterine pregnancy, 284
- Dropsy, case of extreme, by Dr. John Brunton, 269 ; ovarian, some points in its pathology and treatment, by Protheroe Smith, M.D., 459
- Drysdale, Dr. C. R., neuralgic dysmenorrhea, 475
- Dublin Obstetrical Society, meetings of, 56, 129, 270, 532, 604
- Duncan, Dr. J. Matthews, contributions to the mechanism of natural and morbid parturition, review of, 96 ; on puerperal fever, 180 ; cursory notes on ovarian pathology, 361 ; note on intra-uterine craniometry, 417 ; the examination of the female bladder, 465 ; case of spina bifida, 603 ; against the pendulum movement in working the midwifery forceps, 830

Duncan, A. G., notes on a case of triplets, complicated by double uterus, 469

Duroziez, Dr., on the influence of heart disease on menstruation, pregnancy, and the fetus, 630

Dysmenorrhea, on, by E. Garrett-Anderson, M.D., 469; neuralgic, by C. R. Drysdale, M.D., 475; membranacea, on the pathology of, by Beigel, 841

Dysmenorrheal membrane, the, by Dr. F. Finkler, 630

ECLAMPSIA, on the temperature of puerperal, 139

Eczema, intermittent, by J. Williams, M.D., 729

Edinburgh, meetings of Obstetrical Society of, 48, 123, 202, 353, 394, 477, 516, 603, 674, 759, 829

Edis, Dr. A. W., retroversion of the gravid uterus, 170; cases exemplifying the necessity of making a vaginal examination, 390; on the prevention and management of miscarriages, 459; hydrocephalic head impeding delivery, 599; epithelioma of the cervix uteri and pregnancy, 668

Embryotomy, its various modes of procedure: with illustrative cases, by A. Keiller, M.D., 457

Emmenologia, by Professor A. K. Simpson, 675

Ergot, new preparation of, 822

Ewart, J. H., case of inversion of the uterus of three months' standing, 460

Examination, cases showing the importance of making a careful physical, by C. Godson, M.D., 591

Execution of an abortionist, 143

FABRY, Dr., on the treatment of chorea by ether spray and ice to the vertebral column, 559

Farre, Dr. Arthur, on puerperal fever, 248

Féréol, M., a case of transfusion, 358

Féréol, Dr., uterine decidua expelled without metrorrhagia, 774

Fetation, specimen of extra-uterine, 269; case of extra-uterine, by C. J. Cullingworth, 448, 660; extra-uterine, by Dr. Chalmers, 825

Fetus, anencephalous, 55; general anasarca in a, by Protheroe Smith, M.D.,

599; injuries to the, in breech presentations, by Dr. C. Ruge, 536; case of general dropsy in a, by Lawson Tait, F.R.C.S., 600; anencephalic, specimen of, 667

Finkler, Dr. F., the dysmenorrheal membrane, 630

Finlayson, Dr., hysterical (?) paralysis and cough in a child—recovery, 172

Funis, on prolapse of the, during labour, by G. Roper, M.D., 602

Fistula, hypogastric, on a case of, by Dr. Rattray, 674

Flexions, uterine, on, and displacements, by J. G. S. Coghill, M.D., 466

Forceps, a new kind of midwifery, by D. Gordon, M.D., 458; on the history and use of the short straight midwifery, as a tractor, and of the long double curved forceps as a compressor and lever, by T. More Madden, M.D., 532; on the obstetric, as a time-saver, by Dr. G. F. Francis, 556; folding short, by W. Draper, M.R.C.S., 715; against the pendulum movement in working the midwifery, by J. Matthews Duncan, M.D., 834

Foulis, Dr., the development of cysts in the stroma of the ovary, 527

Francis, Dr. G. F., on the obstetric forceps as a time-saver, 556

Frankel, Dr. E., on chlorosis, 557

Fritsch, Dr. H., on the causation of the so-called cephalhematomata, 487

Fruea, Dr., on a case of prolapse of the urinary bladder in an infant, 359

GALABIN, Dr., specimen of extra-uterine fetation, 269

Gander month, by Dr. Aquilla Smith, 175; by Dr. Niven, 317; by Dr. J. Williams, 317

Garrett-Anderson, Dr. E., on dysmenorrhea, 469

Gastrotomy, extra-uterine fibroid, removed by Dr. Routh, 321

Gervis, Dr., exhibition of a pessary, 512

Gervis, F. H., case of complete inversion of the uterus, 513

Gestation, case of extra-uterine, by Dr. J. Denham, 129; extra-uterine, terminating by the ovum becoming encysted, 357; on the means of ascertaining the length of, by Dr. Sutugin, 397

Giles, P. B., junr., a case of natural turning, 432

Godson, Dr. C., cases showing the importance of making a careful physical examination, 591



- Godson, Dr., irregular pigmentation, 668  
 Gordon, Dr. D., a new kind of midwifery forceps, and an obstetric thimble for rupturing the membranes, 458  
 Graafian follicles, on the development of the, in the ovaries of newly-born infants, by M. de Sinety, 559  
 Greene, Dr. W. T., on puerperal fever, 340, 344; double or dicotyledonous placenta, 826  
 Greenhalgh, Dr., on the use of the actual cautery in the enucleation of fibroid tumours of the uterus, 617  
 Griffiths, Dr., on puerperal fever, 345  
 Grosholz, F. H. Y., and W. Y. Martin, notes of one hundred cases of abnormal labour, 32; notes on puerperal fever, 237  
 Gynecic summaries, 72, 141, 214, 287, 358, 423, 556, 628, 702, 775, 841
- HADDON**, Dr. J., intemperance in women, 465  
 Haining, Dr. W., case of rupture of the umbilical cord during delivery, 476  
 Hardie, Dr. J. R., on prickle cells in the capsule of a fibrous polypus of the uterus, 54  
 Harrinson, I., on a form of accidental hemorrhage, 458  
 Harvey, a letter in facsimile, and other hitherto unpublished memorials of, collected by J. H. Aveling, M.D., 217, 310  
 Hayes, Dr. T. C., specimen of placental polypus, 270  
 Heart disease, on the influence of, on menstruation, pregnancy, and the fetus, by Dr. Duroziez, 630  
 Heart, fetal, a case in which the, was heard early in pregnancy, by Dr. Underhill, 529  
 Heiberg, Dr. Hjalmar, the puerperal and pyemic processes, 73  
 Hematoma of the vulva and vagina, by Wernich, 628  
 Hemorrhage, post-partum, Indian hemp in, by W. Donovan, 353; uterine, occasioned by a fibrous polypus, 359; cases of fatal, after childbirth, by Dr. C. Bell, 413; the arrest of post-partum, by compression of the abdominal aorta, 423; on accidental concealed, with cases, by J. Brunton, M.D., 437; on a form of accidental, by I. Harrinson, F.R.C.S., 458; astringent injections in post-partum, 700
- Hewitt, Dr. Graily, on puerperal fever, 196  
 Hicks, Dr. J. Braxton, reposition of the prolapsed funis umbilicalis, 84; on puerperal fever, 109, 191; on dissection of a uterus, 515  
 Hillas, T., ovariectomy with pregnancy, 626  
 Hime, Mr. T. W., on the management of the lying-in woman, 468  
 Honiton, the obstetric arms of, 628  
 Hook, on a new form of blunt, and sling, for assisting delivery in cases of breech presentation, by J. G. Swayne, M.D., 601  
 Hooping-cough, remarks on, and its treatment with carbolic acid vapour, by R. J. Lee, M.D., 476; the treatment of, by the iodide of silver, by R. Bell, M.D., 589  
 Hospital practice, reports of, St. Mary's Hospital, Manchester, 88, 448, 660, 815; Glasgow Western Infirmary, 172; Samaritan Hospital, 243; Hospital for Women, 314; Middlesex Hospital, 390; Guy's Hospital, 507; St. Bartholomew's Hospital, 591; University College Hospital, 729  
 Hubert, M., junr., maternity hospitals, 690  
 Huntley, Dr., on puerperal fever, 192  
 Hutchinson, Jonathan, on puerperal fever, 114  
 Hydrocephalic head impeding delivery, by A. W. Edis, M.D., 599  
 Hypertrophy of the anterior lip of the uterus, 141
- IMLACH**, F., on prolapsus and retroversion as alternating misplacements of the gravid uterus, 394  
 Infants, artificial diet of, by Dr. B. F. Dawson, 703  
 Ingerslev, E., on the weight of newborn children, 705, 777  
 Inhaler, new chloroform, by Dr. Crombie, 824  
 Intemperance in women, by J. Haddon, M.D., 465  
 Inventions, new, an instrument to facilitate removal of growths from the female urethra, 247; new chloroform inhaler, 824; new vaccinator, 829  
 Inversio uteri, exhibition of Crosse's work on, 213
- JAMIESON**, Patrick, prolapsus of the funis umbilicus, 86

**KEILLER, Dr. A.**, embryotomy, its various modes of procedure, with illustrative cases, 457

**Kelly, Dr. W.**, inversion of the uterus, 462

**Kidd, Dr. G. H.**, further observations on fibrous tumours of the uterus, 604

**Krohne and Sesemann** on Dr. Chadwick's case of transfusion, 92

**LABOUR**, notes of one hundred cases of abnormal, by W. Y. Martin, L.R.C.P., and F. H. Y. Grosholz, L.K.Q.C.P., 32; on protracted, by S. Nicholls, 134; note of inquiry with reference to some points in the management of the third stage of, by J. Wallace, M.D., 474

**Larynx**, cyst of, in an infant, 822

**Leading articles**:—the case of Mr. Peacock, 38; criminal responsibility in puerperal fever, 93; lochial drainage, 177; denudation, 245; the status of obstetric medicine, 318; the discussion on puerperal fever, 392; the medical student's final examination, 455; quick with child, 510; the disposal of still-born children, 595; the Brussels Congress on maternity hospitals, 665; the College of Surgeons and obstetric medicine, 732; the development of obstetric medicine, 819

**Leblonde, Dr.**, medico-legal aspect of abortions, 488

**Lee, Dr. R. J.**, remarks on whooping-cough, and its treatment by carbolic acid vapour, 476

**Leishman, Dr.**, on puerperal fever, 104; "System of Midwifery," second edition, review of, 598

**Leopold, Dr.**, on habitual death of the ovum, and its artificial expulsion, 626

**Lewald, Dr.**, absorption of medicaments by the milk, 558

**Liebmán, Dr. C.**, clinical notes on the early course of cancer of the cervix uteri, 41

**Liver**, cirrhosis of the, in infants, 143

**Lochial drainage**, 176

**Locock**, memoir of Sir Charles, 755

**London**, meetings of the Obstetrical Society of, 41, 97, 180, 248, 319, 512, 598, 667, 737, 822

**London**, the Obstetrical Society of, 775

**Lowsley, O.**, a case of rupture of the uterus, with protrusion of the intestine, 166

**Lutaud, Dr.**, vaginismus: its causes, nature and treatment, 288

**Lying-in woman**, on the management of the, by T. W. Hime, M.B., 468

**Lymphatics**, on the uterine, and their influence in uterine pathology, by Dr. Championniere, 630

**McCLINTOCK, Dr. A.**, memoir of the life of William Smellie, M.D., 463

**Macdonald, Dr.**, description of a diprosopus monster, 48; syphilitic placenta, 472; report of and observations upon a case of puerperal tetanus, 516

**Macdougall, Dr.**, on a case of fetal monstrosity, 126; hair in the female bladder, 127; case of fibroid polypus, 127

**MacSwiney, S. M.**, report of a case in midwifery practice, 270

**Madden, Dr. T. M.**, on metro-peritonitis following the use of the ordinary female syringe: a plea for the vaginal irrigator, 56; on the history and use of the short straight midwifery forceps as a tractor, and of the long double curved forceps as a compressor and lever, 532

**Madge, Dr. H. M.**, notes on puerperal fever, with remarks on midwifery statistics, 377

**Martin, W. Y., L.R.C.P., and F. H. Y. Grosholz, L.K.Q.C.P.I.**, notes of one hundred cases of abnormal labour, 32

**Maternity hospitals**, the Brussels Congress on, 665

**Maxon, Dr. P. R.**, postural treatment of shoulder presentations, 701

**Meadows, Dr. A.**, on the diagnosis and treatment of the curable forms of fibroid tumours of the uterus, 561; note on the post-mortem diagnosis of a nulliparous uterus, 670

**Membrane**, physiological changes in the uterine mucous, by J. Barnsfather, M.D., 287

**Menorrhagia**, tetanus following, 358

**Menstruation**, ovulation without, 56; early, 631

**Metritis**, on acute parenchymatous, 72

**Metrorrhagia** arrested by the application of heat to the lumbar region, by Dr. N. G. de Mussy, 421

**Midwifery practice**, report of a case in, by S. M. MacSwiney, M.D., 270

**Midwives**, 360; regulations for, in Denmark, Sweden, and Norway, 136; laws of foreign countries relating to, 143

- Milk, absorption of medicaments by the, by Dr. Lewald, 558
- Milk diet, on the advantage of, in albuminuria of pregnant women, 701
- Miscarriages, on the prevention and management of, by A. W. Edis, M.D., 459
- Mole, carneous or fleshy, 822
- Monod, Dr. Charles, contribution to the study of non-cancerous tumours of the breast, 1
- Monster, description of a diprosopus triophthalmus, 48
- Monstrosity, case of fetal, by Dr. Macdougall, 126; case of, by Dr. F. Wallace, 269
- Muller, Dr. A., rare case of vaccination, 558
- Mussy, Dr. N. G. de, menorrhagia arrested by the application of heat to the lumbar region, 421
- Myo-fibromata of the uterus, 215
- N**AVEL-STRING, on laceration of the, by Dr. Pfannkuch, 355
- Nepveu, Dr. G., rupture of ovarian cysts, 423
- New-born children, on the weight of, 705, 777
- Newman, Dr., on puerperal fever, 107; specimen of a uterus, 320
- News, 143, 360, 559, 775
- Nicolls, S., on protracted labour, 134
- Nitric acid as a caustic in uterine practice, by J. Braithwaite, M.D., 467
- Niven, Dr., on gander month, 317
- Norway, regulation of midwives in, 139
- O**BITUARIES, 704
- Obstetric medicine, address on, and its position in medical education, by Robert Barnes, M.D., 289; the status of, 318; the College of Surgeons and, 732; the development of, 819
- Obstetric Summaries, 66, 139, 213, 284, 355, 421, 487, 556, 624, 699, 774, 838
- Obstetrical works, exhibition of old, 282
- Obstetricians, on the risks run by, by Dr. Cairns, 123
- Omentum, malignant tumour of the, removed post-mortem, 824
- Ortille, Dr., treatment of whooping-cough by carbolic acid inhalations, 424
- Os, a case of difficult labour from abnormal rigidity of the, by G. Calderwood, M.D., 657
- Oswald, Dr. J., suppurating tumour of left ovary, 268; case of Cæsarian section, 673
- Ova, note on the discharge of, and its relation in point of time to menstruation, by J. Williams, M.D., 620
- Ovary, suppurating tumour of left, by Dr. J. Oswald, 268; cyst of the, of thirty-four years' standing, 359; the development of cysts in the stroma of the, by Dr. Foulis, 527
- Ovariectomy, on the performance of, twice on the same patient, by T. Spencer Wells, F.R.C.S., 243; four cases of, by C. H. Routh, M.D., 579; with pregnancy, by T. Hillas, M.R.C.S., 626
- Ovum, case of, expelled, 270; specimen of blighted, 320; on habitual death of the, and its artificial expulsion, by Dr. Leopold, 626
- P**ACHYDERMATA, on the fetal placenta of the, by M. Dastre, 839
- Panas, M., serous ovarian cysts, 214
- Paralysis, hysterical (?), and cough in a child—recovery, 172
- Pathology, cursory notes on ovarian, by Dr. J. Matthews Duncan, 361
- Peacock, Mr., case of, 38
- Pediatric Summaries, 72, 143, 216, 359, 424, 558, 632, 703, 775
- Pemphigus, acute, of the newly-born and early infancy, 216
- Perineum, case of central rupture of the, by Dr. Wilson, 128; rupture of the, 229; laceration of the, by James Young, M.D., 830
- Peritoneum, case of congenital deficiency of the, by Lawson Tait, F.R.C.S., 425
- Pessary, exhibition of a, by Dr. Gervis, 512
- Pfannkuch, Dr., on laceration of the navel-string, 355
- Pigmentation, irregular, by Dr. Godson, 668
- Pinard, Dr. A., the contra-indications for turning in shoulder - presentations, 839
- Placenta prævia, by W. Donovan, L.R.C.P., 460; syphilitic, by Angus Macdonald, M.D., 472; note on a diseased, by Lawson Tait, F.R.C.S., 603; double or dicotyledonous, by Dr. Greene, 826
- Playfair Dr., on puerperal fever, 262
- Pneumonias, the, of childhood, as illustrated by clinical charts, by W. Stephenson, M.D., 477



- Polypi, exhibition of, 674
- Polypus, fibroid, case of, by Dr. Macdougall, 127; fibrous, 202; placental, by Dr. F. C. Hayes, 270; case of intra-uterine fibrous, by Dr. Underhill, 415; uterine hemorrhage, occasioned by a, 359
- Portal, Dr., chloral in puerperal convulsions, 487
- Pregnancy, the corpus luteum of, 139; enlargement of the thyroid body in, by Lawson Tait, 203; case of extra-uterine, by Dr. G. Dresselhuys, 284; on the treatment of tubal, by Dr. Thomas, 421; on extra-uterine peritoneal, by Prof. Depaul, 423; vomiting in connexion with, by H. Fly Smith, M.B., 474; extra-uterine, 838
- Presentations, postural treatment of shoulder, by Dr. P. R. Maxon, 701; the contra-indications for turning in shoulder, by Dr. A. Pinard, 839
- Priestley, Dr., on puerperal fever, 737
- Prolapsus, on, and retroversion as alternating misplacements of the gravid uterus, by F. Imlach, M.B., 394
- Puerperal fever, criminal responsibility in, 93; on the relation of, to the infective diseases and pyemia, by T. Spencer Wells, 97, 180, 348; Dr. Leishman on, 104; Dr. Newman on, 107; Dr. Braxton Hicks on, 109, 191, 259, 268; Mr. Jonathan Hutchinson on, 114; Dr. Richardson on, 118; Dr. Matthews Duncan on, 180; Dr. Barnes on, 183, 259; Dr. Squire on, 188; Dr. J. Brunton on, 191; Dr. Huntley on, 192; Dr. Brown on, 194; Dr. Swayne on, 194; Dr. Graily Hewitt on, 196; Mr. Callender on, 199; on the non-existence of an essential, 213; some cases of puerperal illness in private practice, by C. J. Cullingworth, 221, 299; notes on, by Dr. F. H. V. Groscholz, 237; Dr. Arthur Farre on, 248; Dr. Savage on, 255; Dr. Wynn Williams on, 259; Dr. Playfair on, 262; Dr. Tilt on, 265; Dr. Snow Beck on, 322, 331; Dr. Fordyce Barker on, 322; Dr. West on, 328; Dr. W. T. Greene on, 340, 344; Dr. Routh on, 343; Mr. Wallace on, 344; Dr. Griffiths on, 345; notes on, with remarks on midwifery statistics, by Dr. H. M. Madge, 377; the discussion on, 392; on, by Dr. Priestley, 737
- RATTRAY, Dr., case of hypogastric fistula, 674
- Reviews and Notices of Books: Duncan's "Contribution to the Mechanism of Natural and Morbid Parturition," 96; Atthill's "Diseases of Women," third edition, 96; Steiner's "Diseases of Children," translated by Lawson Tait, 97; Ziemssen's "Cyclopedia of the Practice of Medicine," edited by Buck, 179, 597; Leishman's "System of Midwifery," second edition, 598
- Richardson, Dr., on puerperal fever, 118
- Roper, Dr. G., on prolapse of the funis during labour, 602
- Routh, Dr., extra-uterine fibroid removed by gastrotomy, 321; on puerperal fever, 343; four cases of ovariectomy, 579; fibroid of uterus, 823
- Royal Medical and Chirurgical Society, meeting of, 617
- Royal Society, meeting of, 620
- Ruge, Dr. C., injuries to the fetus in breech presentations, 556
- SALICYLIC acid, on the use of, by Prof. Crede, 288
- Sarcoma uteri, by A. R. Simpson, M.D., 759
- Savage, Dr., on puerperal fever, 255
- Savory, Dr. C. T., epithelioma of cervix uteri complicated with pregnancy, 47
- Sedatives, on the tolerance of, in malignant uterine disease, by W. H. Day, M.D., 646
- Serdukoff, Dr. A., contributions to the determination of the diminution of the uterus after delivery, 477
- Sheffield Hospital for Women, 559
- Simpson, Dr. A. R., emmenologia, 675; sarcoma uteri, 759
- Sinety M. de, on the development of the Graafian follicles in the ovaries of newly-born infants, 559
- Sireday, Dr., on the non-existence of an essential puerperal fever, 213
- Smellie, William, M.D., memoir of the life of, by A. H. McClintock, M.D., 463
- Smith, Dr. Aquilla, gander month, 175
- Smith, Dr. Heywood, case of cystocele—Stoltz's operation—cure, 314; notes of a case of ruptured vagina during labour, with recovery, 671
- Smith, H. Fly, vomiting in connexion with pregnancy, 474
- Smith, Dr. Protheroe, ovarian dropsy, some points in its pathology and treat-
- QUICK with child, 510



ment, 459 ; general anasarca in a fetus, 599

Specula, exhibition of, 674

Speculum, on the avoidance of the frequent use of the, by J. Braithwaite, M.D., 504

Spina bifida, case of, by J. Matthews Duncan, M.D., 603

Squire, Dr., on puerperal fever, 188

Statistics, obstetrical, by J. G. Swayne, M.D., 466

Steiner's "Diseases of Children," translated by Lawson Tait, review of, 97

Stephenson, Dr. W., the pneumonias of childhood, as illustrated by clinical charts, 477

Still-born children, the disposal of, 595

Student's, the medical, final examination, 455

Sutugin, Dr., on the means of ascertaining the length of gestation, 397

Swayne, Dr., on puerperal fever, 194 ; on the relative frequency of the different cranial positions, 372 ; obstetrical statistics, 466 ; on a new form of blunt hook and sling for assisting delivery in cases of breech-presentation, 601

Sweden, regulations for midwives in, 137

**T**AIT, Mr. Lawson, enlargement of the thyroid body in pregnancy, 203 ; case of congenital deficiency of the peritoneum, 425 ; case of general dropsy in a fetus, 600 ; note on a diseased placenta, 603

Tait's translation of Steiner's "Diseases of Children," review of, 97

Tetanus following menorrhagia, 358 ; report of and observations upon a case of puerperal, by Angus Macdonald, M.D., 516

Thimble, obstetric, for rupturing the membranes, by David Gordon, M.D., 458

Thomas, Dr., on the treatment of tubal pregnancy, 421

Thompson, J. A., on laceration of the gravid uterus, 633, 718, 804

Tilt, Dr., on puerperal fever, 265

Transfusion, immediate, by Aveling's method, by J. R. Chadwick, M.D., 66 ; case of, by Mons. Féréol, 358 ; notes of a case of, by Aveling's apparatus, by Dr. G. Hoggan, 469

Triple birth, case of, 422

Triplets, notes on a case of, complicated by double uterus, by A. G. Duncan, M.B., 469

Tumour, fibrous, 56 ; intra-mural cal-

careous, impeding labour, 269 ; case of ovarian, by H. G. Croly, F.R.C.S.I., 277 ; colloid, by Dr. Barnes, 321

Tumours (non-cancerous) of the breast, by Dr. Charles Monod, 1 ; multiple fibroid, of the uterus, 140

Turning, case of natural, by P. B. Giles, jun., L.R.C.P., M.R.C.S., 432

Twins, adherent, case of, by Professor Valtuta, 624

**U**MBILICAL cord, case of rupture of the, during delivery, by W. Haining, M.D., 476

Umbilicalis funis, on the postural treatment of prolapsus of the, by J. Brunton, M.D., 16

Underhill, Dr., a case of intra-uterine fibrous polypus, 415 ; a case in which the fetal heart was heard early in pregnancy, 529

Urethra, female, an instrument to facilitate removal of growths from the, 247

Urinary bladder, case of prolapse of the, in an infant, by Dr. G. Frua, 359

Uteri, clinical notes on the early course of cancer of the cervix, 41 ; epithelioma of, complicated by pregnancy, 47 ; cervix and pregnancy, epithelioma of the, by Dr. Edis, 668

Uterine system, on lesion and disease in the chronic affections of the, 142

Uterus, on prickle cells in the capsule of a fibrous polypus of, by Dr. J. R. Hardie, 54 ; retroversion of the gravid, by C. J. Cullingworth, 88, 91, 815 ; cystic myomata of the, 140 ; multiple fibroid tumours of the, 140 ; hypertrophy of the anterior lip of the, 141 ; rupture of the, with protrusion of intestine, by O. Lowsley, M.R.C.S., 166 ; retroversion of the gravid, by Dr. A. W. Edis, 170 ; myo-fibromata of the, 215 ; specimen of a, removed from a patient post-mortem in which Cesarian section had been performed, 320 ; inversion of the, of three months' standing, by J. W. Ewart, 460 ; inversion of the, by Dr. W. Kelly, 462 ; contributions to the determination of the diminution of the, after delivery, by Dr. A. Serdukoff, 477 ; the mucous membrane of the body of the, by Dr. J. Williams, 496 ; case of complete inversion of the, by Mr. F. H. Jervis, 513 ; on a dissection of a, by Dr. J. Braxton Hicks, 515 ; on the diagnosis and treatment of the curable form of fibrous tumours of the, by Dr.

A. Meadows, 551; further observations on fibroid tumours of the, by Dr. G. H. Kidd, 604; on the use of the actual cautery in the enucleation of fibroid tumours of the, by Dr. Greenhalgh, 617; on laceration of the gravid, by J. A. Thompson, 632, 718, 804; note on the post-mortem diagnosis of a nulliparous, by Dr. A. Meadows, 670; retroflexion of the, by C. J. Cullingworth, 815; fibroid of, by Dr. Routh, 823; fibroid tumour of the, by Dr. Daly, 825; on ablation of the, by M. Donné, 842

**V**ACCINATION, rare case of, by Dr. A. Muller, 558

Vaccinator, new, by Dr. Bruce, 829

Vagina, case of complete occlusion of the, and retention of menses, consequent upon an abortion, 507; case of ruptured, during labour, with recovery, by Dr. Heywood Smith, 671; spontaneous rupture of the, recovery, by Dr. Wiltshire, 672

Vaginal examination, cases exemplifying the necessity of making a, by Dr. Edis, 390

Vaginismus: its causes, nature, and treatment, by Dr. Lutaud, 288

Valtuta, Professor, case of adherent twins, 624

Venous coagulum, by Dr. Barnes, 321

Vulch, Dr. A., on complete atresia of the female genital organs, or unilateral hematometra, 556

**W**ALLACE, Dr. F., case of monstrosity, 269

Wallace, Dr. J., note of inquiry with reference to some points in the management of the third stage of labour, 474

Wallace, Mr., on puerperal fever, 344

Wells, T. Spencer, on the relation of puerperal fever to the infective diseases and pyemia, 97; on the performance of ovariectomy twice on the same patient, 243; on puerperal fever, 348

Wernich on hematoma of the vulva and vagina, 628

West, Dr., on puerperal fever, 328

Williams, Dr. John, on gander month, 317; the mucous membrane of the body of the uterus, 496; note on the discharge of ova, and its relation in point of time to menstruation, 620; intermittent eczema, 729

Williams, Dr. Wynn, on puerperal fever, 259; specimen of intra-mural calcareous tumour impeding labour, 269

Wilson, Dr., case of central rupture of the perineum, 128

Wiltshire, Dr., spontaneous rupture of the vagina—recovery, 672

Whooping-cough, treatment of, by carbolic acid inhalations, by Dr. Ortille, 424

Women, influence of posture on, by J. H. Aveling, M.D., 145

**Y**OUNG, Dr. J., on laceration of the perineum, 830

**Z**IESSÉN'S "Cyclopedia of Medicine," edited by Buck, review of, 179; vol. x.; "Diseases of the Female Sexual Organs," by Professor Carl Schroeder, review of, 597

Zweifel, Dr., influence of chloroform upon the fetus in utero, 421

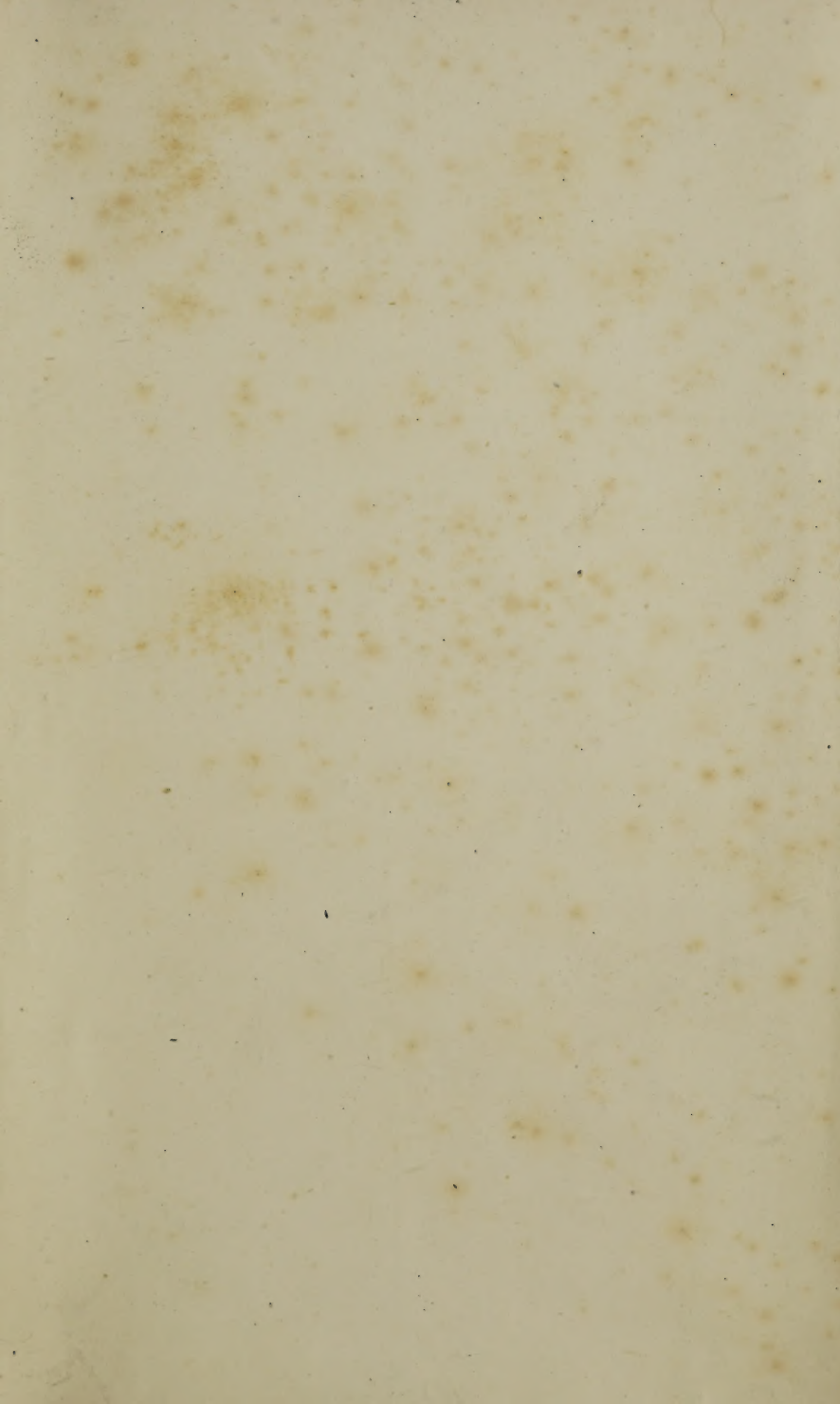
END OF VOL. III.



LONDON:

SAVILL, EDWARDS AND CO., PRINTERS, CHANDOS STREET,

COVENT GARDEN.





WELLCOME HISTORICAL MEDICAL LIBRARY STORE,  
WELLCOME CHEMICAL WORKS,  
805, BAY 1,  
DARTFORD, KENT.



